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Survey on Recent Work in the History of Econometrics

A Witness Report

Marcel Boumans



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Survey on Recent Work in the History of Econometrics A Witness Report

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Abstract

This survey is written to show historians of economics what is happening in history of econometrics, and is the second survey I did with this aim. The first survey, published in 2011, concluded that interest in the history of econometrics has arisen primarily from within econometrics itself and that its histories have been written mainly by econometricians. After the publication of the first survey, history of econometrics remained mainly the interest of econometricians. More recently, however, one can observe an increasing interest in the history of econometrics among historians of economics and historians of science. It seems that if the subject of study is econometrics as a discipline it remains to be of interest only to the econometricians, but if the subject is the artefacts created by econometricians, such as econometric models, it caught the attention of historians of science.

Keywords: history of econometrics, metaphors, scientific revolution, discipline, crediting, science practice.

JEL classification: history of econometrics, metaphors, scientific revolution, discipline, crediting, science practice.

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Introduction

This essay is the fourth “invited survey paper generally constructed to show all historians of economics what is happening in a particular research sub-community” (Weintraub 2015, 361), in this case history of econometrics. To write a survey on “what is happening” in a field to which one is contributing creates the problem of what kind of position one could or should take. I am not a ‘detached observer’ or ‘reporter,’ which would suggest too much distance; neither am I primarily a ‘historian of econometrics’ (my identification lies with history and philosophy of science), which suggests too less distance. I am not a main actor in this field but I am ‘involved.’ I have been involved with the history of econometrics because I was ‘there’ at several occasions and events, as participant, observer, contributor, author, organizer, or commentator. I believe this makes me a ‘witness’ in its etymological meaning of someone who makes an “attestation of fact, event, etc., from personal knowledge,” or “see or know by personal presence” (Harper 2001-2018). This survey is therefore personal in a particular way: it reports mainly about those events which I *wit*-nessed.¹

This survey is also meant as a follow-up of our essay ‘A History of the Histories of Econometrics’ (Boumans and Dupont-Kieffer 2011) written as an introduction to the *HOPE* annual supplement on the Histories on Econometrics (Boumans, Dupont-Kieffer and Qin 2011). In that introduction we observed that “interest in the history of econometrics has arisen primarily from within econometrics itself and that its histories have been written mainly by econometricians” (9), and we concluded by using Leo Corry’s (1989) terminology that “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” (Boumans and Dupont-Kieffer 2011, 27).

Another involvement with the history of econometrics, however, will be used to structure this report. This is related to a coordinated response to a book review of Mary Morgan’s *History of Econometric Ideas* (1990) by Leland G. Neuberg (1995), consisting of three comments written by Duo Qin, G. Michael Lail and Neil De Marchi, myself, and a reply by Morgan. It was an exceptional invitation by Peter Phillips, the editor of *Econometric Theory*, giving Morgan the opportunity to organize this response to Neuberg’s review.

The third involvement consists of having participated at three econometric conferences which allows me to reflect on the relationship between econometricians and historians. The last and most recent involvement is a conference on the history of macroeconomic modeling which I organized to find out what is currently happening on this topic.

¹ This has an unintended consequence of a language bias. Because of my inability to read French well enough, I will not discuss the French contributions to the history of econometrics, such as those of Michel Armate and Alain Desrosières, notwithstanding their relevance to French reading historians. I thank Ariane Duont-Kieffer for emphasising this bias.

Errors and Refutations

Within a short period around 1990, three monographs were published with the aim of covering the history of econometrics: Roy J. Epstein's (1987) *A History of Econometrics*, Mary S. Morgan's (1990) *The History of Econometric Ideas*, and Duo Qin's (1993) *The Formation of Econometrics: A Historical Perspective*.² Histories about themes and topics related to econometrics had appeared before, such as C. Hildreth's (1986) *The Cowles Commission in Chicago, 1939-1955*, but for the first time the subject was econometrics as a discipline. This rise of the historical interest of econometrics as a discipline can be explained by the growing dissatisfaction with the dominant Cowles Commission methodology among econometricians at the end of the 1970s (Boumans and Dupont-Kieffer 2011, 11-16).³

My interest in the area was stimulated by the intense debate among econometricians in recent years over the scientific foundations of current [Cowles Commission] methodology. (Epstein 1987, 1)

It induced the interest of historical exploration of econometrics *before* and *beside* the history of the Cowles Commission.

These three histories are admittedly 'internal,' in that they are not embedded in or related to any cultural, institutional, sociological, economic, or psychological context. Nevertheless, they had still to develop an appropriate historiography for a discipline such as econometrics. Except the introductory remarks of Morgan (1990)'s chapter 7, where she refers to Imre Lakatos's *Proofs and Refutations* (1976) as a resource to write an integrated account of the material, the reasons for the resulting historiographies of these three histories remain implicit. Fortunately we do not have to guess Morgan's approach because in retrospect it became explicit in her response to Neuberg's (1995) review of her book.

Neuberg's review was very critical, if not negative, about Morgan's history:

Despite its smooth and well-written narrative, frequent thoughtful insights, and consistent documentation, Morgan's tale fails to take a fully convincing form. She tries to see the story as one of progress from Jevons to the 1940s, followed by a probabilistic revolution. Rather than try to persuade the reader, she takes it for granted that she chronicles a science in formation and that Haavelmo's contribution is a scientific revolution in Kuhn's (1970) sense. (Neuberg 1995, 371-2)

This verdict was based on two arguments. The first argument is that Morgan does not notice the relevant role of metaphors to enfold the history of economics. In the pre-Haavelmo history the "cyclic harmonic motion metaphor" from physics first grew in

² In this context, another closely related publication should be mentioned: De Marchi and Gilbert's (1989) *History and Methodology of Econometrics*, with contributions of John Aldrich, David F. Hendry and Morgan, Karl A. Fox, Qin, Epstein, J.J. Thomas, Aris Spanos, Nancy J. Wulwick, A. J. Hughes Hallett, Philip Mirowski, and Tony Lawson.

³ According to Morgan the particularities of what was covered by these publications could also be driven by "what has been or was being lost since the first generation of econometricians (time series, dynamics, etc.)" (personal e-mail correspondence with Morgan).

sophistication and then failed in the work of Tinbergen. Moreover, “missing the failure of the cyclic harmonic motion metaphor in Tinbergen [...] distorts Morgan’s handling of [Keynes’s] critical reaction to his work” (p. 375). With respect to this Tinbergen-Keynes debate about the epistemological nature of correlations, Neuberg comments that Morgan “attends too much to avenging an unjust beating that she perceives Tinbergen took from Keynes” (p. 379). In post-Haavelmo econometrics the cyclic harmonic motion metaphor was replaced by the “random sample metaphor” from statistics but Morgan “fails” to analyze its initial success and later failure.

Neuberg’s second argument is that Morgan seems to suggest that the probabilistic revolution caused by Haavelmo (1944) is a Kuhnian revolution, and that she did not answer the questions that follows from this view, but even did not raise them: “What was the pre-Haavelmo paradigm? What specific phenomena did it successfully explain? What historical examples arose that it failed to explain? How did Haavelmo’s ‘paradigm’ explain the anomalous cases?” (Neuberg 1995, 379).

There was also a third element in his critique, but less detailed. Neuberg noted that Morgan took too less critical distance to fully appraise the accomplishments of the early econometricians:

Sympathy leads her to ignore important doubt and error. Does empathy for a criticized author lead Morgan to adopt his view of the causality-correlation relation? If so, then it also leads her to overlook the crucial viewpoint difference that is the basis of the critic’s attack. Because her authors sometimes confuse accomplishments with unachieved research aims, she does too. (Neuberg 1995, 381)

The early econometricians aimed at science and, according to Neuberg, this meant “physics and its deductive form of explanation,” but they failed to achieve it. Neuberg concludes then his review with the rhetorical question: “That researchers confuse their aims and accomplishments is no surprise. Should not historians of research unscramble the confusion?” (381).

History of Metaphors

In her response to Neuberg’s first point of critique, Morgan commented that the “metaphor-led” interpretation of the history of econometrics is not one without attendant dangers:

It can involve a strange kind of determinism in which economists are unthinking appropriators of metaphors from other disciplines, unable to deal with their own subject matter except through the organizing metaphor from another science. (Morgan 1995, 392)

Relevant to know is that she made these remarks in the context of a debate at that time in the history of economics community initiated by the historiographical approach Philip Mirowski had used in his *More Heat than Light* (1989). Mirowski’s approach can be briefly summarized with Jorge Luis Borges’s remark: “It may be that universal history

is the history of a handful of metaphors" (Mirowski 1989a, 1), added with the idea that when these metaphors are not fully comprehended they imply failure in the field of their application. Mirowski's central metaphor for his history of neoclassical economics is the physicist's concept of energy. This historiography became the subject of a *HOPE* conference organized by Neil De Marchi and which contributions were published in *Non-Natural Social Science* (De Marchi 1993).

As far as I know, this "metaphor-led" historiography has not been taken up in any of the histories of econometrics, except by Mirowski (1989b) himself.

Some familiarity with recent discussions in the history and philosophy of science would suggest that an understanding of the "Econometrics Revolution" demands a broader scope than that which presumes a simple model of diffusion of techniques, or one which tries to explain the rise of econometrics solely from such "internalist" considerations as the "demands of the data" or the "logic of the economic problem". (Mirowski 1989b, 217)

The "broader scope" was the same as of his *More Heat Than Light* (1989a): "To understand the history of econometrics, one must first consider the history of neoclassical economics; and, to understand neoclassical economics, one must first have some understanding of the history of physics" (1989b, 218). The concepts that were now badly "copied" where the probabilistic concepts of quantum mechanics.

Instead of a metaphor-led historiography, what should be taken up from history of science, according to Morgan, is the idea that econometrics did not develop in isolation:

I would argue that there is a continuing transfer of ideas, concepts, methods, models, and, yes, metaphors, among the sciences, and a study of such transfers provides insight and understanding into how science proceeds. (Morgan 1995, 392)

As a good example of such history she referred to the work of the historian of science, M. Norton Wise, 'Work and Waste' (1989-1990), published in three parts in the journal *History of Science*.

Probabilistic Revolution

It was not accidental that Morgan referred to Wise, he was one of the contributors to the two-volume publication of *The Probabilistic Revolution*. This publication was the joint output of group of twenty-one scholars that had gathered at Bielefeld, Germany, during the academic year 1982-1983 to study the "Probabilistic Revolution" (Krüger 1987, xv). Beside Morgan and Wise, it included the following historians of science: Lorraine J. Daston, Stephen M. Stigler, and Theodore M. Porter. Their publications became standard references for historians of econometrics, such as Stigler's (1986) *The History of Statistics* and Porter's (1995) *Trust in Numbers*.

Morgan's usage of the term "probabilistic revolution" in her *History of Econometric Ideas* therefore was meant as a reference to her involvement with the Bielefeld project rather than an expression of commitment to a Kuhnian account of scientific change. Although Thomas Kuhn contributed to the *Probabilistic Revolution* project by writing its first chapter 'What are scientific revolutions?' and the prehistory of the project began in 1974 when Krüger had participated in a research seminar given by Kuhn (Krüger 1987, xv), Morgan (1995, 394), like any other contributor, felt free to study the probabilistic revolution "in all its multitudinous guises and varied interpretations."

In relation to this Kuhnian reference, I would like to readdress the issue that we discussed in our introduction to *Histories on Econometrics* (Boumans and Dupont-Kieffer 2011), namely whether or to what extent we need philosophy of science for the historiography of econometrics. After the notorious Capri conference in 1989 (De Marchi and Blaug 1991), historians of economics generally turned away from philosophy of science as a useful source for historiographic frameworks. Concepts as Kuhn's 'normal science' and Lakatos's 'scientific research program' became increasingly considered to be too restricted as the units which development should be studied. These concepts, including the closely related Kuhnian concept of revolution, were rather unhelpful in explaining change and development in economics. Other explanatory frameworks came into view, such as from sociology, anthropology and economics.

With this ban on philosophy that became popular among historians of economics in the 1990s, the proverbial baby was also thrown out with the bathwater. Philosophers of science are perhaps not good in explaining how science develops, but they can be good in clarifying what science is. I give two examples of these babies.

The first is Kuhn's (1970) concept of a discipline, which he called "normal science." If one aims at writing a 'history of econometrics,' one could set oneself the task of writing a history of a discipline. This means that one not only aims at writing about the development of theories but also about the other elements that together shape a discipline. Writing the history of an entire discipline is complicated because a discipline consist of several interacting components, such as a set of tools and techniques, one of models and theories, one of methodologies, and so on. Kuhn's notion of "disciplinary matrix" reflects nicely the multi-component character of a discipline. According to Kuhn a discipline consists of four elements: symbolic generalizations, metaphysical parts, values and paradigms. Paradigms are here meant to be 'exemplars,' the concrete solutions to problems that one for example can find in textbooks. A history can therefore be split up in histories of each of these elements.⁴ This approach for example allows for a history of a discipline by studying the development of its major textbooks (see also Giraud 2018).

The other baby is not Lakatos's framework of research programs, which indeed proved problematic for a discipline like economics, but his approach of rational reconstruction as applied in his (1976) *Proofs and Refutations*. It appeared to be rather useful for Morgan in writing the last but one chapter of her *History of Econometric Ideas*. Although she also observes that "accounts of scientific change taken from the philosophy

⁴ I found this approach very useful to write a brief entry (Boumans 2016) on the history of econometrics for the *Handbook on the History of Economic Analysis*.

of science seemed to me similarly constraining, and so of limited use,” she explains that “Lakatos’s *Proofs and Refutations* (1976) was an acknowledged resource that enabled me to write an integrated account of my material” (Morgan 1995, 394). The rational reconstruction approach of *Proofs and Refutations* is different from the rational reconstruction of his “methodology of scientific research programs.” The latter is normative because “progress” is assessed on how “novel facts” are accounted for by the theories of empirical research programs, whereas the first is a non-normative “distilled history.” It describes the development of a mathematical theory in terms of “problem-solving,” “concept-stretching” and “monster-barring,” and not in terms of “progress” or “growth”: “one should not be surprised if *one does not solve the problem one has set out to solve*” (Lakatos 1976, 90; italics in the original), and therefore this account is close to the practice of “mathematical discovery.”

To present history as a dialogue, either as an exchange of letters in the case of Morgan’s history of econometric ideas (1990), or as taking place in an imaginary classroom in the case of Lakatos’s history of the proof of Euler’s conjecture for all regular polyhedral (1976), and in the case of Weintraub’s history of general equilibrium analysis (1985) serves very well a specific goal: to bring to its bare bones the “objective” structure of a Socratic dialogue without reference to any other authority than the power of reason itself. The dynamics of the dialogue is only determined by “internal” factors.

The question however is for which fields this kind of history works well. Perhaps only for those fields where knowledge is most “objective,” such as mathematics and logic.⁵ And perhaps it worked for Weintraub and Morgan because they were discussing foundational issues, such as identification, simultaneity and causality in the case of Morgan and the assumptions underlying general equilibrium theory in Weintraub’s work. It is a modernist approach, in the sense that it uses “characteristic methods of a discipline to criticize the discipline itself, not in order to subvert it but in order to entrench it more firmly in its area of competence” (Greenberg 1965, 193; see also Klammer 1993), typical for “reflexive sciences,” according to Corry (1989).

Personal intermezzo: As well as I very much appreciate the abstract work of Mondriaan, I admire the historical abstractions of Lakatos. I have studied mathematics and read *Proofs and Refutations* near the end of my study with enormous joy: it shows the beauty of the practice of mathematical “discovery” in a rather direct and condense way. But I also soon had to admit that the domain of mathematical beauty is rather small. Even the book itself seems to show that this kind of abstraction only works well for Euler’s conjecture, that is, for geometry, and not for Cauchy’s Principle of Continuity, that is, for functional analysis, explored in an Appendix of this work.⁶

⁵ Most mathematicians and logicians are Platonists. One of the core ideas of Platonism is that mathematical objects are perfectly real and exist independently of us (Brown 1999, 11).

⁶ Euler’s conjecture is that for all regular polyhedra $V-E+F=2$, where V is the number of vertices, E the number of edges and F the number of faces of a polyhedron. Cauchy’s Principle of Continuity is that the limit of any convergent series of continuous functions is itself continuous.

Historical Crediting

The third point of Neuberger's criticism is related to the problem of distance. Neuberger claims that Morgan's undue "sympathy" or "empathy" with her subjects has led her to miss certain mistakes or errors they had made. This problem of distance entails a few aspects that are relevant for the historiography of econometrics.

One aspect of distance is related to the observation mentioned at the beginning of this survey, namely that the history of econometrics is mainly written by econometricians themselves, often with the purpose of delineating the discipline. Delineation can be done in various ways, by nominating the "giants" of the field, by indicating which ideas or methods belong most to the "core" of the field, or by declaring which period covers the "high days" of the discipline's history (Boumans and Dupont-Kieffer 2011). What we did not address is that history can also be used as a source of "scientific credit." In their joint work *Finding Equilibrium*, Till D ppe and Weintraub (2014; see also D ppe 2018) discuss the problem of scientific credit with respect to the proof of the existence of economic equilibrium: "actors strategize on credit by creating their own historical narratives about their work. This history of science becomes part of the machinery that justifies and brings about credit" (xvii).

I would like to give a hypothetical example of how this machinery could have worked for econometrics, namely the Nobel Prize in Economics for Trygve Haavelmo in 1989. This example is indeed only hypothetical because there is not yet any evidence available about the nomination process; the names of the nominees and other information about the nominations for a specific Nobel Prize will not be revealed until 50 years after each nomination.⁷ The starting point for this machinery is the growing dissatisfaction of the dominant econometric approach, the Cowles Commission approach, in the early 1980s. This dissatisfaction revitalized the interest of econometricians in their discipline's history, particularly in the developments before the 1950s when the Cowles Commission program had become the dominant approach (Boumans and Dupont-Kieffer 2011). The first issue of *Econometric Theory* founded in 1985, announced as one of its editorial policy aims

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 1940s 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. (Phillips 1985, 4)

Some years earlier, David Hendry discovered history as a source of relevant ideas:

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

⁷ There is however evidence of a different kind, namely bibliographic data, which were used by Hoover (2014) to investigate a closely related question, namely the significance of Haavelmo's 'The Probability Approach in Econometrics.'

and ignored the evolution of our discipline. Dick Stone agreed, and he helped me to obtain funding from the ESRC. 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)

The ESRC grant for the Study in the History of Econometric Thought was also used to finance Morgan's PhD research, which resulting doctoral thesis (1984) was published as *The History of Econometric Ideas* in 1990. The collaboration between Hendry and Morgan cumulated in *The Foundations of Econometric Analysis* (1995).⁸ This reader with an extensive introduction by Hendry and Morgan was "the outcome of the extensive archival research on which Morgan (1990) was based" (xi) and includes publications from 1891, by John Neville Keynes, to 1952, by G.H. Orcutt.

This increased interest in the history of pre-Cowles econometrics by the econometricians and the specific attention for Haavelmo's 'Probability Approach' in the 1980s could be an explanation for awarding him the Nobel Prize "for his clarification of the probability theory foundations of econometrics and his analyses of simultaneous economic structures." It could explain its rather late date and also the emphasis on "probability theory foundations" rather than "simultaneous economic structures":⁹

many economists are familiar with Trygve Haavelmo's simultaneous equations paper of 1943, but not so many people knew (particularly before Haavelmo won the Nobel Prize in 1989) of his more important contributions in The Probability Approach in Econometrics of 1944. (Hendry and Morgan 1995, 1)

This successful crediting could have increased the interest of econometricians not only into their discipline's history but also in the works by historians. I presented twice at a meeting of the Econometric Society and both times I was happily surprised by the attendance at the history sessions, much more than I have ever experienced at other economic meetings. The first was a session on the history of econometrics, organized by Roy Weintraub, at the North American Summer Meeting of the Econometric Society at Duke University in June 2007, of which the papers were published in 2010 as a mini symposium in *HOPE* 42(1). According to Kevin Hoover (2010, 20), the session "must be marked a success" considering "its warm reception by econometricians. [...] This history session was well attended and the discussion was lively, although the audience was almost entirely practitioners rather than historians."

The second occasion was at a Special Econometric Society History Initiative Session at the Joint Congress of the European Economic Association and the Econometric Society, EEA-ESEM at the Toulouse School of Economics in August 2014, organized by Mary Morgan. Again this session was very well attended and warmly received by the econometricians. Morgan had organized this session because

There is an initiative in the Econometric Society to enhance the history of econometrics [...] At their last meeting, a session on history of econometrics attracted more than 100 people and the programme committee for this year's

⁸ The original expected year of publication was 1986.

⁹ It should be noted here that Haavelmo's simultaneous equations approach can be considered as the "blueprint" of the Cowles Commission methodology (Morgan 1990, 251).

meeting (of which I am a member) is keen to have an even more successful session this time. This is a community of economists genuinely interested in its history and it would be good if members of our history of economics community can respond. [SHOE-list Wed, 15 Jan 2014]¹⁰

Notwithstanding these successes, I have to be cautious not to overstate the role of historians in the credit machinery of econometricians. It could be that the historians are only allowed at the econometrician's meetings in their role as court jesters. While entertaining the econometricians with some historical reflections, they must therefore be cautious not to overdo their act. The program of the Trygve Haavelmo Centennial Symposium in December 2011 held in Oslo captured a nice mixture of invited econometricians and historians to discuss the work of Haavelmo. However, when it came to the publication of the presented papers, an (to me) unexpected dividing line became visible: The two special Haavelmo Memorial issues of *Econometric Theory* (volume 31, issues 1 and 2, 2015) contain only the papers by the econometricians: Olav Bjerkholt, Hendry and Søren Johansen, James Heckman and Rodrigo Pinto, Judea Pearl, Theodore W. Anderson, Erik Børn, Katarina Juselius, John S. Chipman, Hoover and Juselius, and Qin. The papers by Hoover (Hoover and Juselius (2015) was not presented at the conference), John Aldrich, Philippe Le Gall, and me were published elsewhere, if they were published (see Boumans 2014 and Hoover 2014).

By the term 'econometrician' I mean someone with a training in econometrics and whose contributions are largely in econometrics. In this respect Bjerkholt and Qin are interesting cases. Olav Bjerkholt is a former student of Ragnar Frisch, worked most of his academic life in applied econometrics on energy or development issues, and was research director at Statistisk Sentralbyrå (Statistics Norway). His interest in Frisch started late in his academic career with publishing historical contributions to *Econometric Theory* (2005, 2007a, 2015a), exhaustively drawing on the Frisch and Haavelmo archives at the University of Oslo and the Frisch correspondence files at the National Library of Norway. But he did not restrict himself to this journal, he also published in historical journals: *History of Economic Ideas* (2009), *The European Journal of the History of Economic Thought* (2015b, 2007b), *The Journal of the History of Economic Thought* (2017), and with Dupont-Kieffer in *History of Political Economy* (2011, 2010). While Frisch did not publish that much, he "accumulated a comprehensive archive of letters, notes, and documents of different kinds from the mid-1920s until his death. [...] The archive is thus huge in terms of personally written documents and not particularly easy to survey" (Bjerkholt and Dupont-Kieffer 2011, 112). When Frisch died in 1973, Haavelmo "reluctantly" took charge of the archive and after some years had the bulk of the correspondence sent to the University Library, which later sent the collection to the National Library of Norway, where it still resides (112). The remaining part of the Frisch archive is still residing at the Institute of Economics, established by Frisch, of the Department of Economics of the University of Oslo. The archive is since 2005 a bit organized but "it is still undecided when and how it will be made accessible" (112).¹¹ It seems that Bjerkholt's mission is to make this archival material more available by

¹⁰ This "session on history of econometrics," Morgan is referring to is probably a session organised by James Heckman (2013 President of the Econometric Society). Heckman's interest could be caused by his involvement with the Haavelmo Centennial Symposium, see next paragraph. (Personal e-mail correspondence with Morgan)

¹¹ In his 2005 paper Bjerkholt expresses his gratitude to Tore S. Thonstad "who has done a great job of organizing the Frisch and Haavelmo archive" (491).

writing about its content and showing “exhibits” of it and also by publishing two of Frisch’s lecture series (with Dupont-Kieffer 2009, and with Qin 2010). Due to the Haavelmo Centennial, Haavelmo’s papers are better accessible.¹²

Except of her (2011a) article in the *HOPE* supplement on Histories of Econometrics, Qin has never published in historical journals, her histories are published in *Oxford Economics Papers* (1989), *Econometric Theory* (2015, with Gilbert 2001) and *Journal of Economic Surveys* (2011b). The probable reason for this can be inferred from her clarification of the perspective she wishes to take in her (1993) monograph *The Formation of Econometrics*: “I have chosen to tell the story from the econometric perspective instead of the usual perspective in the history of economic thought, i.e. presenting the story either according to different schools or economic issues” (2). In her (2013) “sequel” to the 1993 monograph, she again clarifies what the “style” of her first book is, and which she also uses for three chapters of her more recent book: “to examine the history by themes which are selected from the perspective of econometrics rather than from the history of economic thought” (2). History is at the service of econometrics, it “may benefit the econometrics teaching and research in the present and onwards” (2013, vi).

View from Somewhere

This brings us, beside the credit machinery, to another aspect about distance, namely the fact that historical analysis of a practice is inevitably an evaluation of that practice. Although this assessment can have various different forms, ranging from appraisal, appreciation, criticism to disapproval, it cannot “escape from ‘framework.’ There is no view from nowhere, no platforms on which [...] the historian can stand apart and aloof from the materials” (Weintraub 2002, 269).

One of these frameworks is that historians should give immanent critique. Neuberg’s criticism is taking this view by accusing Morgan of not giving immanent critique: the evaluation of a research programme whether it is able to meet its own set standards or aims. This framework is modernistic as characterized above and typical for a reflexive science.

Other normative frameworks are those who use normative approaches drawn, for example, from philosophy of science to evaluate practices in terms of “progress,” from economics in terms of “efficiency,” and from sociology in terms of Robert Merton’s CUDO’s.¹³

But there is also a middle way between a normative critique at the one side and a hagiography at the other side, namely a naturalized history of science. With this term Morgan’s approach for her *History of Econometric Ideas* could be characterized.¹⁴

¹² They can be found at the Centennial Symposium website, www.sv.uio.no/econ/english/research/news-and-events/events/others/2011/Trygve%20M.%20Haavelmo/publications/. Another fortunate development for historians of econometrics is that the Cowles Foundation has started to post its archival materials on its website: <https://cowles.yale.edu/about-us/archives>.

¹³ Merton’s CUDO’s are the social preconditions for good science he identified: Communalism, Universalism, Disinterestedness, and Organized scepticism.

¹⁴ See Boumans 2018 for a more detailed description of Morgan’s approach, on which this paragraph is based.

Although already quite explicit about her historiography of her 1990 book in 1995, in the following 20 years she developed a more explicit historiography, which is a specific mixture of history and philosophy of science. The opening paragraph of the Preface of her most recent monograph *The World in the Model* (2012, xv) is most telling about her approach:

Science is messy. Historians write seamless accounts to make it comprehensible, and in doing so, sometimes paper over the knots and holes in scientific life. Philosophers provide sparsely argued analyses of scientific method, and in doing so may avoid the many awkward rubs of details. This book is not such a monograph: It offers neither a continuous historical narrative nor a fortified philosophy of modelling. Yet, its ambition is to offer both a history of the naturalization of modelling in economics and a naturalized philosophy of science for economics.

The study of science, whether historical, philosophical or otherwise, should aim at understanding. The goals should not be normative, nor should they take the more neutral position of being descriptive. Both, in fact, are outsiders' positions. The aim should be to stay as close to practice and its practitioners as possible by attempting to see science practice from the perspective of the practitioners as far as possible. Understanding means to see the problems as the practitioners face them and try to comprehend the choices they make to solve them. In a personal correspondence, Mary Morgan, however, emphasized that science should not be reduced to a set of "practices," and that "ideas" are important as well. Without ideas some of the practices would never have developed. In this context it should be noted that her (1990) book on the history of econometrics was called 'The History of Economic *Ideas*.'

According to Morgan, "case studies are the best way that I know to figure out how science goes on" (2012a, p. xv). Her (2012b) article explains what case study entails by listing the following characteristics:

- A case study investigates a bounded whole object of analysis.
- Case-study research maintains a considerable degree of open-endedness, and the boundary between subject of analysis and context is not clear at the start of the research and may remain fluid during the study.
- A case study involves researching directly a "real-life" whole, which creates a considerable depth of engagement with the subject and dense evidential materials across a range of aspects of the topic.
- Many potential research methods may be used within the case study.
- The outcome is a complex, often narrated, account that typically contains some of the raw evidence as well as its analysis and that ties together the many different bits of evidence in the study (Morgan, 2012b, p. 668).

The general turn to practice in history of science and its associated case study approach has also influenced the historical studies of econometrics. Except Qin's (2013) *A History of Econometrics*, econometrics is not studied anymore as a discipline, but as a family of particular kind of practices. As a consequence the "units" that are studied have also changed. More than theories and ideas, other "artifacts" should be "followed" (Halsmayer 2018). According to Halsmayer (2018, 630) artifacts are "whatever

economists treat as their very research material, the things they investigate, manage, and work with (charts, tables, and scatterplots) and the seemingly mundane things involved in the making of economic knowledge (lists, survey forms, and graph paper).” The most often studied artifact so far is the “model.” Artifacts “travel” in time and space, and therefore historians should “follow” them.¹⁵ Artifacts, however, should not be considered as “objects” with inherent qualities but as relational since they condition and are conditioned by practices. As objects they would not travel far. According to Halsmayer “following artifacts” means “to follow all the shifts and changes in their meanings, forms, and interpretations” (632) when they move across time and space. A most recent example of this kind of history of econometrics is the current increase of interest in the history of econometric modeling (Halsmayer 2017, Panhans and Singleton 2017, Rancan 2017).

Hidden Figures

A change of perspective makes other people and artefacts visible, who or which otherwise would remain hidden from ‘standard narratives.’ Standard narratives are usually canons of published theories and ideas. A different perspective such as “through the lens of practice” (Stapleford 2017) will take us beyond the “text” and thus extend the range of possible investigation a historian may pursue: “we might consider pedagogy and training; the form and style of personal interactions; the practices that sustain hierarchies and institutional roles; the hours, organization, and division of labor; and many other behavioral patterns that comprise the communal production of knowledge” (Stapleford 2017, 119).

With rather similar historiographic ideas in mind, Roger Backhouse, Beatrice Cherrier, Pedro Duarte, Kevin Hoover and I gathered in January 2016 at Oxford to discuss the prospects of a history focused on macroeconometric modeling.¹⁶ Our conjecture was that when fuller historical analysis is undertaken, the history of macroeconometric modeling will turn out to be far more central to the history of macroeconomics than has previously been recognized. To investigate this conjecture a conference in April 2017 at the Utrecht University was organized, of which a part of the presented papers will be published as a special issue of *HOPE* (Acosta and Rubin; Backhouse and Cherrier; Chao; Dupont-Kieffer; Goutsmedt, Pinzón-Fuchs, Renault and Sergi; Pinzón-Fuchs; Rancan; Saïdi; Salazar and Otero).

One of the historiographical relevant consequences of looking through the lens of practice is that “practices may have different chronologies from what we commonly label as ‘theories’ and ‘ideas’” (Stapleford 2017, 119). Some of the Utrecht contributions, indeed, show a difference between the chronology of the “standard narrative” and their own account, for example with respect to the influence of the Lucas Critique (Goutsmedt, Pinzón-Fuchs, Renault and Sergi) and the VAR approach (Salazar and Otero).

Changes of chronologies are not only due to a different perspective but could also be the result of a change of historiographic methodology (Claveau and Herfeld 2018). Salazar and Otero (forthcoming) use bibliometric methods, such as citation and

¹⁵ The term “travel” refers to Peter Howlett and Morgan’s *How Well Do Facts Travel?* (2011).

¹⁶ See Boumans and Duarte (forthcoming) for a more detailed account of this project.

cocitation networks, as their main tools of their historiographic analysis. Qin's (2013, chapter 10) citation analysis shows that the "Cowles Commission paradigm," contrary to the standard narrative, "was able to withstand the post-1970 reformative movements" (183). The longevity was primarily sustained by "emulous research into devising estimators for various types of models," but also by the publication of textbooks "which played an important role in topic diffusion, notably outside economics" (184).

The perhaps most striking result, confirmed by most of the papers presented at Utrecht is to see how much of the history of macroeconomic modeling took place outside universities, mainly at central banks, serving mainly if not only non-academic clients. The field of macroeconomic modeling is similar to what has come to be called "big science" in the sense of large-scale research involving different disciplinary teams where division of expertise is necessary. These different kinds of expertise are found both in- and outside academia.

Conclusion

An invitation to survey on what is happening in the sub-community of historians of econometrics is an invitation to look around who is doing what in relation to the history of econometrics and to ask them questions about these doings. The current survey is actually the second survey I did with this aim. The first survey was to investigate whether a *HOPE* conference solely on the history of econometrics was feasible, to see who the earlier contributors were and whether they were still active, and then 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. I then concluded that interest in the history of econometrics has arisen primarily from within econometrics itself and that its histories have been written mainly by econometricians (Boumans and Dupont-Kieffer 2011, 8-9). Although we were able to find a sufficient set of excellent historians for the conference, the heat soon turned down again. After the conference in 2010 and the publication of its papers in 2011, history of econometrics remained mainly the interest of econometricians, in particular Bjerkholt (2015a, 2015b, 2017) and Qin (2011b, 2013, 2015).

When we started the project on the history of macroeconomic modeling, I wanted to find out who else was working on this topic.¹⁷ I therefore posted twice a call for papers, one for a session on 'The History of Macroeconomic Modeling' for the History of Economics Society Meeting at Duke University, June 2016, and another for the Utrecht conference in 2017. An unexpected large group of historians, mainly from continental Europe, answered my call: Juan Acosta, Hsiang-Ke Chao, Ariane Dupont-Kieffer, Aurélien Goutsmedt, Verena Halsmayer, Daniel Otero, Erich Pinzón-Fuchs, Antonella Rancan, Matthieu Renault, Goulven Rubin, Aurélien Saïdi, Boris Salazar, Francesco Sergi. It seems that econometrics as a discipline remains to be of interest only to the econometricians but that the artefacts created by econometricians, because they became so much part of the daily practice of modern economics, have caught the attention of historians of economics.

¹⁷ In the meantime the topic of this project has changed into the more general theme of the History of Macroeconomics.

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