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Abstract

Trygve Haavelmo spent the academic year 1938/39 at the University of Aarhus as a teacher in statistics. He would immediately after his Aarhus stay leave for the United States, where he completed *The Probability Approach in Econometrics* (1944) and later worked at the Cowles Commission before returning to Norway in 1947. The purpose of the paper has been to assess whether Haavelmo in Aarhus was already on a path towards the Probability Approach or, as suggested in the history of econometrics literature, this path did not really open up until Haavelmo came to the U.S.A. and got converted to probability reasoning. The paper gives a survey of Haavelmo's papers and other work while in Aarhus. The evidence indicates that Haavelmo had adopted probability ideas by the time he was in Aarhus and seemed well prepared to embark on his magnum opus.

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1. Introduction*

Anyone who looks up Trygve Haavelmo's curriculum vitae on the web page of the Swedish Nobel Foundation (Nobelprize.org) will find that after his graduation in economics in 1933 followed:

1933-38	Research assistant, Institute of Economics, University of Oslo
1938-39	Lecturer in statistics, University of Aarhus
1940-42	Rockefeller Fellow

The period as research assistant was in the service of Ragnar Frisch, perhaps the most brilliant of the economists who after the foundation of the Econometric Society in 1930 had started to call themselves *econometricians*. During the period as Rockefeller Fellow Haavelmo completed the treatise *On the Theory and Measurement of Economic Relations* (Haavelmo 1941), the first version of *The Probability Approach in Econometrics* (Haavelmo 1944) for which he was awarded the Nobel Prize in economics. One is bound to wonder about the significance of the lectureship in Aarhus in 1938/39.[1] The newly founded University of Aarhus and its Institute of Economics was not exactly a world centre of econometrics at that time, or a centre of anything within the realm of economics and statistics.

That Trygve Haavelmo spent the academic year 1938/39 at the Institute of Economics, University of Aarhus as a lecturer in statistics is a fact but not obviously a topic of interest in the history of econometrics. Below we shall state briefly how Haavelmo happened to come to Aarhus in the first place and also discuss what he was doing there, but the main purpose is to shed light on how far in his preparations for completing his magnum opus Haavelmo had come by the time he visited Aarhus. Before coming to Aarhus Haavelmo had worked as assistant and staff at Frisch's Institute of Economics since 1933. Immediately after his stay in Aarhus, Haavelmo went to the U.S.A. in June 1939 for further studies and research. Although the visit was not planned to last very long, because of the war Haavelmo did not return to Norway until 1947.

The interest in the history of econometrics surged in 1980s and 1990s, resulting in influential books, like Epstein (1987), Morgan (1990), and Qin (1993), in special journal issues and many articles. The history covered early econometric attempts, some quite far back in time, but above all the "formative period of econometric theory from approximately 1930 to 1960" (Qin, 1993:1). In the early part of this formative period the ideas and contributions of Ragnar Frisch figure prominently, among them the "confluence analysis". The crowning achievement of the formative period is Haavelmo (1944). The further development and formalization of ideas rooted in Haavelmo's treatise by the team gathered at the Cowles Commission in

* The article draws on correspondence and documents from Rockefeller Archive Center, Tarrytown, New York, Frisch Correspondence Files at the National Library of Norway, and the Frisch and Haavelmo archives, currently at the Department of Economics, University of Oslo. I thank participants of a seminar at CREATES, Aarhus University for comments. I have benefited from the work of Professor emeritus Tore S. Thonstad in organizing the Frisch and Haavelmo archives and from that of Professor emeritus Kåre N. Edvardsen in preparing the Frisch bibliography. I have benefited from communication with John Aldrich, Marcel Boumans, and Mary Morgan. I have in selected passages drawn on my earlier work Bjerkholt (2005, 2007a, 2007b). I thank Inger Bjerkodden for unspecified help and encouragement.

[1] The cv information, presumably submitted by Haavelmo himself, is not entirely correct. Haavelmo was in the period 1933-38 not employed by the University of Oslo, as Frisch's Institute of Economics was not part of the University, even though it was located there. Haavelmo was Rockefeller fellow in the U.S.A. for two years in 1940-41. Haavelmo got a recruiting position ("adjunktstipend") at the University of Oslo from 1941, while he was in the U.S.A. and thus belonged to the University's Faculty of Law throughout the war, exempted from fulfilling his teaching duties, see Bjerkholt (2005, 2007a).

Chicago, directed by Jacob Marschak from 1943 and later by Tjalling Koopmans, resulted in the highly influential Cowles Commission Monographs No. 10 and 14 (Koopmans, 1950; Hood and Koopmans, 1953) and the foundation of econometrics as a scientific discipline.

The picture of Haavelmo drawn in these historical works emphasizes his experience as Ragnar Frisch's student and assistant. But at the same time Haavelmo (1944) is viewed as an outcome of Haavelmo escaping from the overwhelming influence of Ragnar Frisch and coming under the influence of probability reasoning, above all from Jerzy Neyman. A widespread attitude among economists in the early part of the "formative period" was that probability reasoning was not generally applicable to economics because of the non-experimental nature of economic data and other reasons. Frisch was regarding as adhering to such a view, to the extent of being denoted a "anti-probabilist."[2]

In Morgan (1990), a very widely read introduction to the history of econometrics, comprise in this regard the following passages:

> "Haavelmo had been a student of Frisch and later his research assistant … Despite Frisch's influence, Haavelmo was converted to the usefulness of probability ideas by Jerzy Neyman …It was apparently while trying to convert Jerzy Neyman to confluence analysis that Haavelmo's conversion to probability reasoning occurred."
> …
> "Given the attitudes of econometricians in the 1920s and 1930s, it is not surprising that a slightly defensive tone is evident in the published presentation of Trygve Haavelmo's 'The Probability Approach in Econometrics' in 1944. The paper also bore signs of the evangelicism of the newly converted."
> (Morgan 1990: 242.)

Morgan (1990), although curiously imprecise with regard to time and place, yet conveys the impression that the "conversion to probability" took place around 1939-40. This fits with the fact that Haavelmo spent most of the autumn term 1939 in Berkeley, California where Jerzy Neyman had been teaching since he left London in 1938. Morgan's version of these events is corroborated by a passage in Haavelmo's Nobel Speech in 1989:

> "I then had the privilege of studying with the famous statistician Jerzy Neyman in California for a couple of months. At that time, young and naïve, I thought I knew something about econometrics. I exposed some of my thinking on the subject to Professor Neyman. Instead of entering into a discussion with me, he gave me two or three numerical exercises for me to work out. He said he would talk to me when I had done this exercises. When I met him for that second talk, I had lost my illusions regarding the understanding of how to do econometrics. But Professor Neyman also gave me hopes that there might be other more fruitful ways to approach the problem of econometric methods than those, which had so far caused difficulties and disappointment.
> (Haavelmo 1989:285.)

If we take this story about the time and place for Haavelmo's move from Frisch to Neyman at face value, what does it imply for the importance of his stay in Aarhus 1938/39? Perhaps not too much, it would be Haavelmo's last year in Europe before leaving for a visit to the U.S.A. expected to last for at most 1-2 years. He could sum up and reflect on what he had learned from Frisch and others in the preceding years as a research assistant and practitioner. But Haavelmo had not yet been converted to probability ideas, thus he could hardly have done much in preparatory work for his forthcoming *Probability Approach* treatise.

But the story as set out above is - overwhelmingly likely - wrong in details and misleading in the overall impression! Morgan, Epstein and other authors may have relatively little access to

[2] See e.g. Epstein (1987:72, fn.15), cf. Qin (1993:19).

information about what Haavelmo had been doing in the years before he left for the U.S.A. Even in interviews with Haavelmo the facts were not forthcoming, and Haavelmo's own statement in the Nobel Speech was imprecise about time and place.

The story about Haavelmo in Morgan (1990) this runs counter to plausibility for several reasons. It is in the first place hard to imagine Haavelmo arriving in the U.S.A. in 1939, undergoing a "conversion" to probability theory under the influence of Jerzy Neyman and then goes ahead in conceiving and completing (the first version of) the *Probability Approac*h in less than two years. If that had been the case one would have expected Neyman's instrumental role to be acknowledged. But, although the Neyman-Pearson testing procedure figures prominently role in the *Probability Approach*, there is no such acknowledgement. On the contrary the preface of the *Probability Approach* indeed states that the idea of undertaking the study was conceived in Oslo. Another point of interest is that while Haavelmo was in Berkeley in 1939 Neyman invited him to give a seminar on his work, clearly also suggesting that Haavelmo had reflected on his study purpose for quite some time and was prepared to present it.

The pertinent fact about the relationship between Neyman and Haavelmo is that they met for the first time already in the autumn of 1936. Haavelmo spent the entire term at the London University College attending lectures both by Neyman and by Egon Pearson. The "loss of illusions" alluded to by Haavelmo surely took place during that term. Haavelmo's Nobel speech passage about his relation with Neyman is, despite being somewhat misleading, highly meaningful. Neyman was the one who put him on the right track with regard to applying probability in economics. In the ensuing years he studied Neyman's works and also other recent works in probability theory. The visit to Berkeley in 1939 is in light of this more to be viewed as a return visit, Havelmo reporting back on the results he had achieved after Neyman had put him on the right track. This does not rule out that Neyman might have given Haavelmo additional lessons and advice at their reunion.

The revised history amplifies the importance of Haavelmo's year in Aarhus. It invites us to think about the year in Aarhus as a period of preparatory work for the *Probability Approach*. Therefore the traces we may find of what Haavelmo was doing in Aarhus is potentially of some interest in understanding how the *Probability Approach* came about.

In the following we review in section 2 Haavelmo's experience prior to his arrival in Aarhus. Section 3 looks at what Haavelmo wrote while he was in Aarhus. Section 4 discusses a paper he presented at a conference at the very end of his stay in Denmark, and section 5 concludes.

2. Haavelmo's training 1933-38[3]

Trygve Haavelmo was born in 1911 near Oslo. He began to study economics at the University of Oslo in 1930 and graduated in 1933. Ragnar Frisch was in the U.S.A. in the first year of Haavelmo's study, but was teaching economic theory and statistics in lecture series attended by Haavelmo in his second and third year. Haavelmo was hired immediately after graduation as an assistant at Frisch's Institute of Economics, which was still in its initial stage, it was founded at the beginning of 1932 on the basis of a grant from Rockefeller Foundation. Frisch was 38 years old and Haavelmo 21 when they entered into the master-apprentice relation in 1933.

Haavelmo graduated in a time of poor job opportunities in general and graduates from the 2-3 year study in economics was not exactly in high demand. They did for instance not qualify for recruitment to career positions in government. Like several other students and young

[3] This section recapitulates a more extensive version given in Bjerkholt (2005).

graduates Haavelmo was hired to be a "computer." Frisch had acquired computational equipment of various kinds and had a great ability to organize and execute numerical calculations. Frisch's theoretical studies required comprehensive numerical simulations and experiments.

Frisch could hardly have avoided discovering Haavelmo's suitability for qualified work already before he graduated, but he was not offered particularly favourable conditions. Early on as assistant Haavelmo became involved in confluence analytic problems, which meant "tilling" of data and construction of bunch maps. He became extremely proficient in undertaking the calculations that the bunch map analysis required. Haavelmo was the first person to read the *Confluence Analysis* monograph (Frisch 1934) from beginning to end, as Frisch passed the responsibility for proofreading to Haavelmo as he went to the U.S.A. to spend the summer of 1934 as research consultant at the Cowles Commission in Colorado Springs.

Haavelmo's work as assistant in the first 2-3 years did not result in any documents with his name on them. Frisch may have been quite ego-centred in not paying much attention to the career needs even of a very gifted assistant like Haavelmo. It is also difficult to assess Haavelmo's interest and motivation for the work at the Institute. He was not employed by the university and thus not on a career path. Neither was the work well paid. The university had recruiting positions, but no vacancy was in sight. In the spring of 1935 Haavelmo applied for a job as clerical assistant in the social security administration.[4] Soon after Frisch redefined Haavelmo's position to become "chief computor" and doubled his pay. The pay rise perhaps reflected a (belated) recognition on Frisch's part that Haavelmo was as good an apprentice that he could ever hope for.

In the autumn of 1935 Tjalling Koopmans spent three months at the Institute. Koopmans who originally had studied physics, had switched to economics, influenced by Jan Tinbergen who may well have suggested the visit to Oslo. Koopmans had been to London where he had met with both Ronald Fisher, Jerzy Neyman and Egon Pearson. In Oslo he gave a series of seminar lectures under the title *On Modern Sampling Theory*, attended by Frisch, Haavelmo, other assistants and foreign visitors.[5] Koopmans and Frisch exchanged views on confluence analysis and on probability reasoning. Koopmans' doctoral dissertation gave a thorough discussion of Frisch's confluence analytic approach, embracing Frisch's views as well as pointing out shortcomings in the confluence analysis.

Koopmans' visit provided Haavelmo's introduction to the idea of Ronald Fisher as well as to the recently developed Neyman-Pearson theory of testing. The experience may have conveyed the impression that there might be more to learn in probability theory and statistics.[6]

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[4] Haavelmo was indeed offered the position but chose to decline the offer. A reference letter written by Frisch in November 1934 described Haavelmo's work as secretarial, but also consisting of checking mathematical formulae, numerical checking of statistical and other tables, providing numerical examples, etc. He praised Haavelmo as nimble, energetic, discreet and pleasant and as someone who had his unconditional trust, noting that although he would much regret if Haavelmo took another position, he found it reasonable that he sooner or later would do just that, as the University Institute of Economics had no opportunity to offer much in terms of salary.

[5] Koopmans left lecture notes, Koopmans (1935), divided in three parts: (1) on fundamental concepts; (2) Fisher's theory of estimation; and (3) Neyman and Pearson's theory on hypothesis testing. Among foreign visitors who attended the lectures was Georg Rasch from Denmark.

[6] The author once queried Haavelmo about his impressions from Koopmans' visited. Haavelmo stated merely that at the time his position was such that he was not invited to take part in the real discussions. Thus the personal contact between Koopmans and Haavelmo at the time was limited.

Haavelmo was a key assistant in Frisch's most ambitious and demanding project, namely the study of time series and business cycles. Influenced by the findings of Slutsky and Yule Frisch had reoriented his original interest in time series analysis towards *macrodynamics*. As expressed in Frisch (1933) the macrodynamic structural model of the economy worked as a linear operator of the random disturbances to which it was exposed and thereby generated and maintained cycles. Throughout the 1930s the exploration of this idea was Frisch's main research interest. In the Institute jargon it was called "shock theory", cf. Bjerkholt (2007b).

To show that the Slutsky-Yule mechanism, for which the far-sighted Wicksell had provided the *rocking-horse* metaphor, could produce cycles that simulated observed ones was only the first step on the road. Frisch aimed at establishing a general theory for determining "the exact nature of the cycles which are created when a linear operator is applied to a random series" (with known distribution properties) and posed the challenge of the "inversion problem", namely to determine from a given time series produced by such a mechanism the weight curves by which the random disturbances had been accumulated. Given the Institute's equipment at the time these tasks called for enormous and tedious human efforts in which Haavelmo took central part.

Frisch's two conceptual schemes, *confluence analysis* and *shock theory*, constituted Haavelmo's understanding of the main problems facing the econometrician. In both of them probability or random influences played an important role. To Frisch they were not really two separate schemes, it was all about understanding how the macroeconomy functioned. The shock theory provided the explanation of fluctuations. As the macroeconomic mechanism was caused by an interplay of simultaneous equations confluence analysis was needed as a tool to *identify* the individual equations, or assess at least how closely they could be estimated. The term "identify" had not been coined yet, but that was what it was about. Thus the key words in these conceptual schemes were *dynamics* and *simultaneity*. Shock theory and confluence analysis could be fruitfully applied to also other subject matters than the macroeconomic mechanisms. Inside this overall framework was also another concept, which would be prominently dealt with in Haavelmo (1944), namely *autonomy*. Frisch had coined this term in 1931, but it did not appear in print until Haavelmo's work was published.

Haavelmo also took part in empirical studies and was Frisch's teaching assistant, drafting and editing lecture notes. Frisch's international connections as a council member of the Econometric Society and editor of Econometrica might have given Haavelmo an impression of what people in other countries were concerned with.

Haavelmo attended an Econometric Society meeting for the first time in 1936. It was the sixth European meeting and took place in Oxford. Frisch was prominently present, he had been an active participant at almost every Econometric Society meeting in Europe. The Oxford meeting was the largest meeting so far with 64 participants. At the meeting was also Jerzy Neyman who with great eloquence presented innovative ideas.

Haavelmo had submitted the paper *Confluent Relations as Means of Connecting a Macrodynamic Subsystem with the Total System*. As the title suggested the topic was chosen within Frisch's paradigm. Haavelmo posed the problem of what to do when the system of structural equations was mathematically underdetermined. The idea of representing the economy as a determined system of equations had been promoted by Frisch and was of recent origin. Haavelmo argued that rather than adding more structural equations or redefining endogenous variables as rather arbitrarily chosen exogenous time functions, it would be better

to add "confluent relations" which fitted the data reasonably well.[7] Haavelmo's paper was after further rewriting and a change of title eventually published as Haavelmo (1938).

After the meeting Haavelmo remained in England until the beginning of December. He had received a grant from the University of Oslo for studying "the problem of using sampling theory in economic statistics" and spent his time at the Department of Statistics, London University College.[8] There he followed lectures by Egon S. Pearson on general statistical theory and by Jerzy Neyman on testing statistical hypotheses and on orthogonal polynomials.

After his return to Oslo Haavelmo worked on a study of the demand for milk using a number of different data sets. The study aimed at estimating price and income elasticities as functions of household income. The data sources were topped up by interview data, an enquête among housewives, that corroborated the other findings. It was a joint paper with Frisch but naturally the overwhelming part of the work fell on Haavelmo.[9]

The visit to London had just whet Haavelmo's appetite for a longer study visit abroad. In the spring of 1937 Haavelmo was awarded a Norwegian grant for "further study of statistical theory and techniques" abroad. He also nurtured hope at some later stage of getting a Rockefeller grant that would allow him to visit the U.S.A. While he finished up the milk study in the autumn of 1937 Tinbergen visited the Institute and the topic for discussion would naturally be Tinbergen's project for the League of Nations (he had presented ideas and plans for it at the Oxford meeting). Learning that Haavelmo prepared to travel in Europe he extended an invitation to visit him in Geneva.

Haavelmo took off on a European study and research tour in December 1937. He spent a month and a half in Berlin at the well known *Institut für Konjunkturforschung*, directed by Ernst Wagemann, using the more advanced computational equipment in Berlin, namely harmonic analysers based on light interference, for analysing time series data, brought from Oslo. It turned out that the results were hardly as accurate as the results achieved in Oslo.

From Berlin Haavelmo travelled to Geneva in mid-January 1938 to work with Tinbergen at the League of Nations' Financial Section. Tinbergen was close to completion of the first League volume when Haavelmo arrived and was working hard on the second volume, in preparation for the special conference to be convened in Cambridge in July 1938 to discuss the results. Tinbergen brought Haavelmo into a little informal group of 6-7 "econometricians" he had gathered in Geneva and met with almost daily.

From the beginning of April 1938 Haavelmo was in Paris. Frisch had put him in touch with François Divisia and equipped him with introduction letters to others in the econometric circuit. The last couple of weeks of the trip abroad were spent at the Institute of Statistics in Oxford, directed by Jacob Marschak, where Haavelmo, at Marschak's invitation, lectured on confluence analysis.

While Haavelmo was in Oxford in May 1938 Frisch was asked by the head of the newly established Institute of Economics, University of Aarhus, Professor Jørgen Pedersen, whether he had someone in Oslo who could be a substitute statistics teacher for the coming academic

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[7] Haavelmo's presentation resulted in an interchange about structural and confluent relations, cf. Phelps Brown (1937).

[8] The stated study purpose is of some interest here. In the context "sampling theory" meant probability, hence Haavelmo expressed an interest in probability ideas already prior to meeting Neyman. But this was hardly a choice he made in opposition to Frisch.

[9] Frisch and Haavelmo (1938). The article was close to 100 pp. This was the only published joint work of Frisch and Haavelmo. They had worked on a somewhat similar but smaller study of the demand for beer a couple of years earlier but that was never published.

year. Without thinking twice Frisch offered Haavelmo. He was thus more or less called to a position as teacher of statistics in Aarhus and accepted without hesitation when he got to know about it. As Frisch was an intermediary we have to assume that Frisch found it a good idea for Haavelmo to be away from Oslo for a while. Haavelmo might well have expressed an interest in teaching statistics, in London he had made a point of studying teaching programs in statistics.

In mid-July 1938 a conference convened in Cambridge to discuss Tinbergen's work for the League of Nations, much discussed in the history of econometrics.[10] Frisch had been invited to submit a paper for the conference but did attend it. He wrote and submitted a paper (Frisch 1938), which, however, did not arrive in Cambridge until after the delegates had left. Haavelmo got a copy with him when he left for Denmark in August 1938 in ample time before the autumn term started, and drew attention to it in Haavelmo (1944).[11]

3. Haavelmo's activities in Aarhus

Aarhus provided a nice break for Haavelmo. The position may not have been very well paid but surely was more remunerative than the assistant position in Oslo. The teaching burden was light and gave him ample time for reflection, not least, we may imagine, on the possibilities of applying probability theory in economics. He had struggled with reading himself up on modern statistical theory since he was in London. In a way the Aarhus break came at a convenient time.[12]

At the Institute of Economics in Aarhus there were two members of the Econometric Society, professor Jørgen Pedersen and professor Erich Schneider, who was German. Schneider was an admirer of Frisch's work in production theory whom Haavelmo probably was acquainted with knew from Schneider's visits to Oslo.[13] Among other staff were two future ministers, professor Thorkil Kristensen who soon would become professor in Aarhus, and teaching assistant ("undervisningsassistent") Kjeld Philip, who became professor in Aarhus in 1943 and later in Copenhagen.[14] Another teaching assistant with a future career as professor of economics both in Aarhus and in Copenhagen was Jørgen Gelting.[15] Philip and Gelting had both been Pedersen's students and were of exactly the same age as Haavelmo.

Haavelmo gave a course on statistical theory in the autumn of 1938, accompanied by mimeographed lecture notes (Haavelmo, 1939a). He took part in the choice of textbook and was decisive in choosing Davis & Nelson (1935) rather than Westergaard & Nybølle (1927), the revised edition of Westergaard's 1890 book. Haavelmo found the emphasis on philosophical foundations in Westergaard's book commendable and often missing from other textbooks, but as a textbook for economists in 1938 it was insufficient. He praised Davis & Nelson (1935) for conveying the impression that statistics was a "laboratory science", emphasizing mathematical processing of data and comprehensive computations, but criticized

[10] Tinbergen's work was published as Tinbergen (1939), but made available for the conference participants in printed proof versions dated 1938.
[11] Haavelmo did not attend the 1938 conference at Cambridge as asserted in Epstein (1987:57).
[12] On Haavelmo in Aarhus, see also Andersen & Kærgård (2000).
[13] Schneider did some work in investment theory, a topic which interested Haavelmo who cited Schneider's work from the Aarhus period in draft versions of Haavelmo (1960).
[14] Thorkil Kristensen was Minister of Finance 1945-47 and 1950-53 and for several years Secretary General of OECD. Kjeld Philip later became professor of economics in Copenhagen 1951/69 and served in three different minister posts 1957-64.
[15] Gelting's claim to fame is his discovery of the balanced budget multiplier, published in Danish after Haavelmo had left but prior to Haavelmo (1945), hence a case of "who-influenced-whom", see Andersen & Kærgård (2000).

it for being too crowded with formulae and too scarce on the underlying philosophical aspects. Even these off-hand remarks in a note to Frisch may be read as an indication of the shift in Haavelmo's concern from algorithms to philosophy, as what was in short supply for econometric progress.[16]

Jørgen Pedersen had initiated a research program for the new institute of investigations of price and market conditions in Denmark, which in practice meant for agricultural goods. He also established a publication series for the new institute. The first issue was by Pedersen on the British butter market 1923-36.[17] The second issue was also by Pedersen about income fluctuations in agriculture and their repercussions for other industries, while the third issue was a treatise by Kjeld Philip on the crisis laws and regulations 1931-38. Pedersen had surely invited Haavelmo to contribute to the research program as well as to the publication series. Haavelmo rose to the challenge and published for Pedersen's research program a study of the demand for pork in Copenhagen, and in addition also a "dynamic study" of pig production. Haavelmo had by then great experience in empirical studies. He must have learnt a lot about pigs and pork in Denmark. His two studies were completed in the spring of 1939 and issued as no. 5 and 4, respectively, in the publication series (Haavelmo 1939b, 1939c).

The pig production paper was not so much about pig production per se as about the attempt to regulate the production.[18] The huge bacon export to England got into difficulties after Great Britain adopted a quota system for the imports and the Danish authorities adapted to that by introducing a two-price system for the price paid for pigs at the slaughterhouse. Delivered with a "pig-card" the pig got full price, but without a pig-card the price was low. The pig-cards could be traded. Thus this became econometrics of regulation. It has probably amused Haavelmo to embark on this dynamic model of an important real world problem, after the endless numerical experiments with Frisch.

Haavelmo's had noted that the main difficulties of a clear discussion were the same here as in other discussions of economic policy: "there are too many variables to be kept apart from each other in a purely verbal treatment." He found that the problem was essentially a "matter of econometrics … what we need are quantitative measures of the different effects and dependencies." He saw his task as that of to procuring "tools for such a discussion", for that purpose "a mapping of the different interrelations must be made if a rational discussion shall be possible."

Haavelmo determined the key feature in the structure of pig production, namely that the outflow of finished pigs per month was a lagged function (by 10.05 months!) of a linear combination of the inflows to the stocks of first-time-breeding sows and other breeding sows.

But what made the paper really interesting derived from the regulation. The production was regulated by means of the issuance of pig-cards and the prices set for pig with and without cards. To deliver pigs without cards implied a price less than the production costs. The pig-cards were transferable among farmers at market price.

The formal structure Haavelmo had ended up with was follows:

The pig production regulation model

[16] H. T. Davis and W. F. C. Nelson were both associated with the Cowles Commission. Haavelmo's assessment was summed up in a note dated 27 Jan. 1939. Another textbook considered was F. C. Mills: *Statistical Methods Applied to Economics and Business*.

[17] The publication was rather critically reviewed in *Econometrica* by one of Hotelling's students at Columbia, Irma Hilfer, see Hilfer (1938). The thorough 15 pp. review found Pedersen's results "erroneous", due to a mistake in calculating the multiple correlation.

[18] Haavelmo also referred to the paper as "A Dynamic Study of the Regulated Pig Production in Denmark" (the author's emphasis), as if that perhaps had been the original title.

(8.1) $x_1(t+\varphi+\theta_0) = ay_0(t) + by_1(t)$

(8.2) $x(t) = mx_1(t-\theta_1-\theta_2-\theta_3)$

(8.3) $y_0(t) = c_1(x(t)-x^*(t)) + c_2 s(t) + c_0$

(8. 4) $y_1(t) = k_1 y(t-\alpha) + k_2 s(t) + k_0$

(8.5) $s(t) = h_1(p_1(t) - v(t)) + h_0$

(8.6) $\dfrac{v}{p_1(t) - p_2(t)} = f(x(t) - x^*(t))$

(8.7) $y(t) = y_0(t) + y_1(t)$

Variables

$x_1(t)$ = inflow to stock of weaned pigs below 35 kg
$y_0(t)$ = inflow to stock of first time breeding sows
$y_1(t)$ = inflow to stock of other breeding sows
$y(t)$ = total inflow of breeding sows
$x(t)$ = outflow of finished pigs
$s(t)$ = price of small weaned living pigs sold in the market
$v(t)$ = price of pig-cards in trade between farmers
$p_1(t)$ = standard price of pigs delivered with cards
$p_2(t)$ = standard price of pigs delivered without cards
$x^*(t)$ = flow of pigs delivered with cards

Equations (8.1)-(8.2) are the production structure relating inflow of finished pigs as a function of the stocks. The combined lags of these two equations amounts to 10.05 months. Equations (8.3)-(8.4) are the equations of "production starting", a term borrowed from Frisch's famous 1933 paper. But unlike Frisch's propagation and impulse model Haavelmo's dynamic model included prices. Equation (8.5) determines the price of small, weaned pigs and equation (8.6) the price of the pig-cards.

Haavelmo discussed the interaction between production lags and the effects of the regulation with regard to how "shock proof" the system was, concluding that it was indeed not very shock proof. Shocks conveyed through the three exogenous variables, the high and low price, $p_1(t)$ and $p_2(t)$, and the flow of pigs delivered with cards, $x^*(t)$, essentially equal to the number of pig-cards, affected the price of small pigs resulting in shocks reverberating though the production system.

The model could not be solved explicitly. Haavelmo may have taken pleasure in using the skill he had derived in Frisch's laboratory in calculating the inherent dynamics of the system, although he called it "a rather tedious job" (p.41). After some intensive numerical calculations Haavelmo concluded that the system had a characteristic and slightly damped cycle of 5.5 months.[19] He did not find it possible to given an elementary explanation of this cycle, but called a "confluence effect" of the whole simultaneous system: "accepting this system we have, implicitly, accepted a 5.5 month cycle."

[19] He noted that there traces of other cycles present and argued that that they could have emerged from the Slutsky effect or induced via the exogenous variables, cf. Haavelmo (1939b, p.45).

This result and conclusion is interesting in view of the conclusion Frisch (1933) had reached that observed cycles could be traced back to damped cycles in the deterministic model, and Haavelmo's modification of that in his first paper after he came to the U.S.A. that also a model with two exponentials might generate cycles under the influence of random shocks, cf. Haavelmo (1940). According to Haavelmo this paper originated in discussions with Marschak in Colorado Springs 1939, cf. Bjerkholt (2007a), and the conclusion Haavelmo had reached about the confluent cycle in pig production may very well have been brought up in that discussion.

In the concluding section named "The sensitivity of the system to erratic shocks" Haavelmo gave an overall assessment:

> One of the main problems of the artificial regulation policy is to obtain a "shock proof" system. This means that the forces acting towards the type of equilibrium in aim must be strong. Some systems may theoretically fulfil the conditions of giving the equilibrium desired, but they may nevertheless be quite unsuitable for practical regulation purposes because of lack of stability.
>
> The present system is evidently not very "shock proof". Indeed, we have just seen how the highly shock like variable *v* (the card price) rules the whole system. The chain of characteristic lag-relations in the system is a typical example of the most perfect shock-collector. The shocks in *v* affect essentially the price of small pigs which in turn carries them into the production activity where they are preserved for a long period, and lead to new shocks when the finished pigs are to be sold. (p.46)

The paper really was a frontier contribution that has never been properly recognized. One reason for this was of course that World War II broke out only two months after Haavelmo left Denmark, and meant the end both of the bacon exports and the regulation system.

In the estimation work Haavelmo used Frisch's bunch map analysis which he was very familiar with, and refrained in accordance with Frisch's recommendations from giving standard deviations (p.12). The regulation scheme had been adopted in 1933. Haavelmo decided not to ignore data from the initial period and used 1935-37 observations, leaving 1938 observations for comparing with the model's predictions.[20]

The general background for the pork demand study was the same as for the pig production, namely the regulation that had been put in place. The regulation had intended to effectuate a price level for pork in Denmark similar to that of the U.K. This meant an increase in the domestic price and a controversy had arisen as to whether a lower price domestically would benefit not only the consumers but also the producers. Hence, the issue at stake was the magnitude of the elasticity of the demand for pork with regard to the price of pork. It thus at the outset had similarities with studies of the demand for beer and for milk in Norway that Haavelmo had conducted together with Frisch.

Just as for the pig production study Haavelmo underlined it was not his task neither to criticize the regulation policy nor to propose changes but to provide analysis that could settle the controversy: "there are other factors than the price which influence the demand…we need a systematic analysis of these various factors' influence to assess the isolated impact of a change in the price of pork" (p.10).

Haavelmo drafted already in October 1939 a memorandum outlining his approach, first, to build a theory for the investigation, then "statistically verify" the relationships rather than just choosing "a mechanical procedure that fits the market data." The demand study was written in

[20] All data were published in the paper, cf. Haavelmo 1939b), Table 1.

Danish and avoided technicalities, Haavelmo seemed very consciously to write for non-specialist readers and took care to explain the concepts and ideas, the importance of being able to distinguish a demand relationship from a supply relationship, etc. Yet, it was a quite sophisticated analysis he conducted.

A study of total domestic demand for pork turned out to be infeasible or inconvenient for various reasons, hence the demand study was limited to Copenhagen, for which the data situation was found satisfactory. The data used were from Copenhagen's publicly controlled slaughter houses on the supply and prices of pork, beef and calf meat from 1924/25 to 1937/38 and somewhat problematic data for income from the tax statistics and living cost statistics for the corresponding years. In addition Haavelmo had access to a household consumer survey in 1931 from which expenditure elasticities could be estimated, population census data for 1921, 1925, 1930 and 1935, and the consumer unit scale used in official statistics.All data were included in the paper. Also this paper reflected Haavelmo's experience in empirical studies and his thorough and sound judgement in sorting out various problems.

When he finally got to the formal regression equation for determining the elasticity of the pork demand it was formulated as follows:

$$(10.4) \quad \ln x = E_{x\alpha} \ln \alpha + E_{x\beta} \ln \beta - E_{xr} \ln r + C$$

where x was pork consumption per consumer unit, α was the price of pork deflated with the cost-of-living index, β the average price of beef and calf meat divided by the price of pork, and r was total income divided by the number of consumer units. From this regression equation and alternative specifications he estimated the average price elasticity for pork to be around -0.65, while the income effect was negligible. Haavelmo went to some length to elaborate on why this result had become the outcome. It was counterintuitive and also contrary to what Haavelmo had found from the consumer survey.

The key tool in the formal analysis was "the modern form of regression analysis called 'Bunch Analysis' without technical details." Haavelmo adhered to Frisch's maxims by declining to give standard deviations of estimates as such "are of doubtful value with short time series." Hence readers interested in the statistical reliability were referred to Figure 11 which comprised the bunch map for the variables in equation (10.4) and also the correlation matrix and the standard deviation for all variables and the remark: "By studying the various 'bunches' in this map one may judge whether the regression analysis gives meaningful results, and furthermore the statistical certainty of the results."

Both the two empirical studies that Haavelmo had conducted reflected the skills and techniques he had acquired in Frisch's laboratory. Both of them used confluence analysis and adhered to Frisch's maxims, rather than representing a step toward the a probability approach.

While he was in Aarhus Haavelmo also wrote book reviews for *Weltwirtschaftliches Archiv*. Three reviews appeared, comprising altogether seven reviewed works (the first review may have been submitted before Haavelmo got to Aarhus). Some of the reviewed books figure prominently in the history of econometrics. These were Tjalling Koopmans' 1937 dissertation, Jan Tinbergen's Dutch model of 1936 (or rather its English translation of 1937) and Herman Wold's 1938 dissertation, cf. Haavelmo (1938b, 1939f, 1939g).

Early in the spring of 1939 Haavelmo got confirmed that he had been granted some means from the Norway-America Foundation and thus could plan a departure for USA. It wasn't much money beyond the fare across the Atlantic, the means would hardly suffice for more than a few months. At Frisch's insistence Haavelmo then drafted an application to Rockefeller Foundation, in which he described his research interest in very few words as follows:

> My further plans for scientific work are to take up the general problem of connecting economic theory and statistical observations. Besides of this I wish to treat some special oscillating problems in economic dynamics. I have also planned a study of individuals' economic behaviour, particularly dealing with the problems of individuals planning over time.[21]

Not a lot can be read into the quite generally formulated first sentence with regard to how Haavelmo's thinking had progressed on the probability issue since 1936, but the formulation suggests a "general" approach to the problem of "connecting" theory and data. In fact is reminiscent of the opening lines of Haavelmo (1944): "The method of econometric research aims, essentially, at a conjunction of economic theory and actual measurements, using the theory and technique of statistical inference as a bridge pier. But the bridge itself was never completely built." The second topic is not very explicit either, but clearly relates to the realm of shock theory. It might refer problems he had worked on in Paris the previous year, or even something arisen from the work on the pig production paper. The third topic is intriguing as it does not seem to be rooted in anything he had worked on earlier. It may be understood as a study of behaviour under uncertainty and thus as an attempt of bringing probability considerations into behaviour relations.

The reaction from Rockefeller Foundation's Paris office was rather cool. The application was too late, the study plan too vague, and, worst of all, Haavelmo without a university position, did not fit into the Foundation's institution building policy. Frisch had a good standing with the foundation's Paris office and had been consultant to the Paris office on a number of applicants for Rockefeller Fellowship, both young Scandinavians and foreigners trying to flee Europe. He rose to the occasion and did his utmost to convince the Foundation officials, that Haavelmo would have a future at the University of Oslo.[22]

During the academic year in Aarhus Haavelmo wrote two more papers: one was for a conference in Copenhagen at the very end of his stay and the other for the Cowles Commission Research Conference in Colorado Springs in July 1939 which would his first stop in the U.S.A. Haavelmo had been asked by colleagues in Oslo to contribute on behalf of the Norwegian association at the Third Nordic Meeting for Younger Economists in May 1939 in Copenhagen. Haavelmo accepted and presented his paper which is of considerable interest, see section 4 below.

The topic for the paper Haavelmo prepared for his presentation at the Fifth Cowles Commission Research Conference in 1939, immediately after his arrival in USA, was nothing less than a new approach towards Frisch's "inversion problem." This was about how to retrieve the coefficients of the underlying deterministic dynamic series from "shock cumulants", i.e. observations generated by a dynamic model exposed to random shocks. Classical regression methods would not give unbiased estimates of structural coefficients. Haavelmo found that the scheme of erratic shocks could be replaced with a formally equivalent model with stochastic variations in the coefficients, as he indeed had suggested in Haavelmo (1938). It was thus a core topic from Frisch's shock theory research agenda, but

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[21] Haavelmo/Rockefeller Foundation, 15 April 1939.

[22] Frisch gave Haavelmo the following recommendation: "He is a constructive thinker with a broad grasp of problems and a considerable ability to distinguish between the essential and the inessential. He has shown a distinct ability to handle statistical data and to combine them in such a way as to fit them into the theoretical frame work. Indeed, he could probably be classified just as well, or even better, as a statistician. He combines in an unusual degree the qualities of an economic theorist and a statistician. He is very energetic." (Frisch/Rockefeller Foundation, 25 May 1939.) He did, however, recommend Haavelmo above other candidates he was consulted about. The outcome was in the end that Haavelmo was offered Fellowship for one year from 1940, but he did not get to know this until mid-November 1939. Haavelmo got a recruiting position with limited teaching duties at the University of Oslo from 1941.

reflecting the increased interest Haavelmo had taken in the confrontation between observations and theory.[23]

4. On statistical testing of hypotheses in economic theory

For the Third Nordic Meeting for Younger Economists, 27-30 May, 1939 Haavelmo's paper had was titled *On statistical testing of hypotheses in economic theory*. The technical level of the presentation was quite elementary and the audience perhaps not exactly erudite in modern statistical theory. Haavelmo presented a highly sophisticated lecture which covered briefly verification in economics, but also touched upon a number of other issues in econometric work. The section headings of the only 18 pp. paper were as follows:[24]

1. Introduction.
2. The hypotheses of economic theory are of statistical nature.
3. About the general principles for statistical testing of hypotheses.
4. Free and system bound variations. "Visible" and "invisible" hypotheses.
5. The "ceteris paribus" clause as a statistical problem.
6. The specification problem.
7. The trend problem.
8. The distinction between average explanation and momentaneous explanation.

The opening section set the tone:

> Anyone who has worked in economic theory knows how it often is the case that several different "correct" theories can be put forward to explain the same phenomenon. The differences are in the choice of assumptions. One comes all the time to cross-roads where one direction a priori seems as plausible as another. To avoid it all becoming just a logical game, one must at each step have these questions clearly in view: Are there realistic elements in my reasoning, or do I operate in a one hundred percent model world? … It is here that the requirement of statistical verification comes to rescue, prevents the reasoning for running astray and forces a sharp and precise formulation of the hypotheses. The statistical corroboration saves us from many empty theories, at the same time as it gives the hypotheses verified by data so much more theoretical and practical value.
>
> It might seem as if we did best by sticking to what we see of data. But that is not so. Then we would never be able to distinguish between essential and inessential traits. Data can give us idea about how we should formulate hypotheses, but theoretical considerations must assist us. Neither must we uncritically discard a hypothesis even if a set of data seems to go in another direction. Many hypotheses, perhaps the most fundamental and fruitful, are often not directly accessible for testing. But we can continue the argument and reach "surface" hypotheses than can be tested. (pp.1-2)

In section 2 he argued for the statistical nature of the hypotheses of economic theory, emphasizing that testing was not an easy task.

> The circuit of problems relating to the testing of hypotheses is not exhausted by the question of the degree of precision in the agreement between data and a certain hypothesis. The key problems in the hypothesis testing lie actually prior to that stage in the analysis. It turns out –

[23] Haavelmo (1939e). The paper cited Frisch, Slutsky, Wold and Yule.
[24] Haavelmo (1939d), title, section headings, and excerpts translated by the author.

> as we shall see – that many hypotheses cannot at all be verified by data, even if they are quantitatively well defined and realistic enough. Yes, we can be led astray if we try a direct quantification. (p.3)

He dealt briefly with the principles of statistical testing in section 3, following but without mentioning or citing Neyman-Pearson. Section 4 on "free and system bound variation" is really about simultaneity, although he barely used that term. The analysis of simultaneity was a key topic in Frisch's confluence analysis, as it would be in Haavelmo (1943). As an illuminating examples Haavelmo used the models (pp.6-7)

(4.1) $x = f(p)$ demand curve

(4.2) $x = g(p)$ supply curve

and

(4.3) $x = f(p, r)$ demand (r is income)

(4.4) $x = g(p)$ supply

to underline various points.

> Many hypotheses, perhaps especially those we consider as fundamental in economic theory, may apparently contradict the statistical facts. … But it doesn't need to be anything paradoxical in such occurrences. Yes, it might on the contrary be that such apparent contradictions just is a verification of the theoretical hypotheses. (p.6)
> …
> If we have *n* variables and *m* independent relations (*n*>*m*), then there are only *n*-*m* degrees of freedom left. Anyone who <u>now</u> peaks into our model world, will not be able to see the free variations that was the basis for including each one of the relations, he sees only the <u>system bound</u> variation that follows from all relations being fulfilled simultaneously. (p.6)

If the relations hold exactly all variation in data would have to be at the confluent market relation given by the intersection of the planes of the equations. Knowing the model (4.3)-(4.4) we can deduce that the observed relationship between *x* and *p* must be the supply relation. If we just relied on the data alone, we might as well have believed that we had found an increasing demand curve.

That the problem of confluence of simultaneous relations was of a common and widespread nature was common knowledge in Frisch's laboratory. Using here formulations similar to those he would later use in the "Autonomy" chapter of the *Probability Approach*, Haavelmo argued that is was more of a problem in economics as experiments were not possible (apart from interviews), we have only got data for the system bound variations:

> This is precisely one of the main reasons why refined techniques must get such a prominent position in modern economic research. Here, there of no use to come with "sledge hammer" methods, we need the statistical technique's finest tools to come to grips with the problems. (p.8).

He made in section 5 some clarifying remarks on *ceteris paribus* clauses, a topic on which much confusions ruled. He used the problematic income effects in his pork demand study as an example. The brief section 6 on specification, cited Henry Schultz' demand study for 1930, is of lesser importance. He made the point here that formulation of hypotheses and statistical testing are not two successive steps but "a simultaneous process in the analysis of economic problems. It is this which is the basic idea in modern econometric reserach" (p.13).

Section 7 on the trend problem was clearly meant to attck the uncritical use of trend elimination. Haavelmo's argument can be read as also countering the often mentioned barrier for probability in economics that economic time series are not recurrent events to which probability laws apply. Haavelmo did not bring up probability explicitly. The question of

trend elimination, Haavelmo stated, is often conceived as a purely technical-statistical problem, but is in reality of far more profound character:

> In our formulations of theoretical laws we operate always with things of such nature that they can be thought of as repeating themselves. This holds both for static and dynamic formulations of laws. The most important economic data are given as time series, thus a quite particular series of successive events. Is it possible to test laws for recurrent events on the basis of such time bound variations? … Economic time series usually have two features that strike the eye: one is the one-sided straight development, the trend, the other is certain variations around the trend. Often we can track the cause of the trend back to certain slowly, changing things (e.g. changes in population size or structure), things that are outside the range of entities included in our hypotheses and also seem to be independent of the variations we wish to study. In such case it is natural to take the trend as a datum in the analysis and consider the things that happen apart from the trend. This is the rational basis for a statistical elimination of trend in our observations. It is unacceptable to make a purely mechanical trend elimination without a concrete interpretation of the trend's emergence. It could be that an observed trend has its explanation in the relations between the things that are included in our hypotheses. (pp.13-14).
>
> Assume that we have arrived at a determined dynamic system, such that we can solve the system, i.e. find the time paths of the variables under consideration. It might then be the case that the observed trend movements are just the possible solutions of this system. In other words the trend movement can arise as a confluent form of the dynamic system of structural equations. The observed trends can thus be taken as a statistical verification of our system of hypotheses. (p.14)
>
> When our test data are series with marked trend movements, it could be asserted that the hypotheses we can get verified, will not be laws for recurrent events, but only a description of a historical path. If that viewpoint had to be accepted in general, it would be a severe blow for the attempt of establishing economic laws. But we don't have to accept this negative position. The cause of the trend is either outside our system of hypotheses, and if we can state the causes, we are allowed to eliminate the trend and consider only the residual variation, which has the character of recurrence. Or, the trend derives from the structure of the system under consideration, it is the outcome of an analysis of free variations and has its explanation by the same system of hypotheses which led to variations of recurrent nature. (p.15).

Section 8 was a brief (3 pp.) highly instructive and illuminating guide to the interpretation of regression results, showing in particular how specification errors will affect regression coefficients and residual variation. He concluded by underlining "how necessary it is to have a prior formulation of the hypotheses, considering certain counterfactual variations. If one hasn't got that, one risks overlooking certain important variables which by accident or for special reasons have not varied significantly in the available data material (p.18). His points here and in some of the previous sections would even today serve its purpose as an excellent introduction to the fundamental problems of econometrics.

It was not the probability approach, neither was it the occasion for it. Haavelmo's journey had not yet brought him to that stage. His experiences since 1933 and attempts to penetrate probability theory and its application to economics since 1936 had advanced his thinking in leap and bounds and prepared him for further achievement. His sophisticated arguments and carefully phrased formulations in the Copenhagen lecture are reminiscent of passages in Haavelmo (1944). Although key elements in the *Probability Approach* are totally absent in everything Haavelmo wrote while in Aarhus, many arguments and concepts can be traced back, at least in embryo, to what Haavelmo did in Europe. This is in line with Haavelmo's explicitly statement that "the idea of undertaking this study" developed in Europe (Haavelmo, 1944:v). About half a year after Haavelmo left Aarthus he wrote to Frisch about the need for

making probability considerations about the deviations between theory and data to decide ultimately whether a theory was "good" or "bad", but still with mixed feelings about the range of applicability of the idea. It was anyway an idea that he on the eve of his departure was well prepared for.

5. Conclusion

Haavelmo's education had started firmly within Frisch's paradigms, which clearly encompassed some of the most challenging ideas for the development of econometrics, launched in the 1930s. His conception of econometric problems was firmly anchored in Frisch's dynamic structural equations and in the confluence analytic approach to simultaneity problems.

The lack of exact criteria in the confluence analysis was pointed out by Koopmans, whose Oslo lectures and later dissertation must have stimulated Haavelmo to penetrate deeper into the contributions of R. A. Fisher and J. Neyman and E. Pearson. The opportunity to study with Jerzy Neman in London was of decisive importance. In addition to learning statistical testing Haavelmo was inspired while in London to take a deeper look into probability theory. We have not been able to show that Haavelmo had conceived the core of the probability approach before he crossed the Atlantic, but on the other hand, the elements in the universe he structured in his 1941 treatise were to large extent in his baggage.

Haavelmo left Denmark in June 1939. Next stop was Cowles Commission Research Conference at Colorado Springs. There Haavelmo would rejoin Jakob Marschak who had moved to United States at the end of 1938, and for the first time meet with Abraham Wald.

References

Andersen, E. & N. Kærgård (2000) Trygve Haavelmo 13. december 1911 – 28. juli 1999. *Nationaløkonomisk tidsskrift* **138**, 288-300.

Bjerkholt, O. (2005) Frisch's econometric laboratory and the rise of Trygve Haavelmo's Probability Approach, *Econometric Theory* **21**, 491-533.

Bjerkholt, O. (2007a) Writing "The Probability Approach" with nowhere to go: Haavelmo in the United States, 1939-1944, *Econometric Theory* **23**, 775-837.

Bjerkholt, O. (2007b) Ragnar Frisch's Business Cycle Approach: The genesis of the propagation and impulse model, *European Journal for the History of Economic Thought* **14**, 449-486.

Cowles Commission (1952) *Economic Theory and Measurement: A Twenty Year Research report 1932-1952*. Cowles Commission, University of Chicago.

Davis, H. T. & W. F. C. Nelson (1935) *The Elements of Statistics*, Bloomington, Indiana: Principia Press [Second Edition 1937].

Epstein, R. J. (1987) *A History of Econometrics*, New York: North Holland.

Frisch, R. (1929) Correlation and Scatter in Statistical Variables. *Nordisk Statistisk Tidsskrift* **8**, 36-102.

Frisch, R. (1933a) *Pitfalls in the Statistical Construction of Demand and Supply Curves.* Veröffentlichungen der Frankfurter Gesellschaft für Konjunkturforschung, Neue Folge Heft 5, Leipzig: Hans Buske Verlag.

Frisch, R. (1933b), Propagation problems and impulse problems in dynamic economics. In *Economic Essays in honour of Gustav Cassel*, London: George Allen & Unwin Ltd, pp. 171-205.

Frisch, R. (1934) *Statistical Confluence Analysis by Means of Complete Regression Systems*. Publication No. 5, University Institute of Economics, Oslo.

Frisch, R. (1938), Statistical versus theoretical relations in economic macrodynamics, Memorandum prepared for the Business Cycle Conference at Cambridge, England, July 1938, to discuss Professor J. Tinbergen's publications for the League of Nations. In Hendry & Morgan (1995), pp. 407-419.

Frisch, R. and T. Haavelmo (1938) Efterspørselen efter melk i Norge. *Statsøkonomisk Tidsskrift* **52**, 1-62.

Frisch, R. & F. V. Waugh (1933) Partial Time Regressions as Compared with Individual Trends. *Econometrica* **1**, 387-401.

Haavelmo, T. (1938a) The Method of Supplementary Confluent Relations, Illustrated by a Study of Stock Prices. *Econometrica* **6**, 203-218.

Haavelmo, T, (1938b) Drei Beispiele der ökonometrischen Forschung in den Niederlanden (Tinbergen, J: *An econometric approach to business cycle problems*, Paris 1937; Dalmulder, J.J.J.: *On econometrics. Some suggestions concerning the method of econometrics and its application to studies regarding the influence of rationalization on employment in the U.S.A.*, Haarlem, 1937; Koopmans, T.: *Linear regression analysis of economic time series*, Haarlem, 1937.), *Weltwirtschaftliches Archiv* **48**, 1938 II, 7*-11*.

Haavelmo, T. (1939a) *Indledning til statistikkens teori*, Forelæsninger ved Aarhus Universitet, Efteraarssem. 1938, Aarhus, Mimeographed, 49 pp.

Haavelmo, T. (1939b) A dynamic study of pig production in Denmark. *Nordisk Tidsskrift for Teknisk Økonomi* **5**, 177-216. [Also published as Studier fra Aarhus Universitets Økonomiske Institut, No. 4..

Haavelmo, T. (1939c) *Efterspørgselen efter flæsk i København*. Studier fra Aarhus Universitets Økonomiske Institut, No. 5, 48 pp..

Haavelmo, T. (1939d) *Om statistisk "testing" av hypoteser i den økonomiske teori*. Det Tredje Nordiske Møte for Yngre Økonomer, København, 27.-30. mai 1939, Aarhus, Mimeographed.

Haavelmo, T. (1939e) Statistical Testing of Dynamic Systems If the Series Observed Are Shock Cumulants. Report of Fifth Annual Research Conference on Economics and Statistics at Colorado Springs, July 3 to 28, 1939, *Cowles Commission for Research in Economics*, 1939, 45-47.

Haavelmo, T, (1939f) Book review (Chait, B.: *Les fluctuations économiques et l'interdépendance des marchés*, Bruxelles, 1938; Derselbe, *Essai d'explication des crises économiques par l'économétrie*, Paris, 1939; Derselbe: *Le problème des crises économiques*, Paris, 1938.), *Weltwirtschaftliches Archiv* **49**, 1939 I, 123*-126*.

Haavelmo, T, (1939g) Book review (Wold, Herman: *A study in the analysis of stationary time series*, Uppsala 1938), *Weltwirtschaftliches Archiv* **50**, 1939 II, 114*-116*.

Haavelmo, T. (1940) The Inadequacy of Testing Dynamic Theory by Comparing Theoretical Solutions and Observed Cycles. *Econometrica* **8**, 312-321.

Haavelmo, T. (1941) *On the Theory and Measurement of Economic Relations*. Mimeographed, Cambridge, MA.

Haavelmo, T. (1943) The Statistical Implications of a System of Simultaneous Equations, *Econometrica* **11**, 1-12.

Haavelmo, T. (1944) The Probability Approach in Econometrics. *Econometrica* **12**, Supplement, 1-118.

Haavelmo, T. (1945) Multiplier Effects of a Balanced Budget. *Econometrica* **13**, 311-318.

Haavelmo, T. (1960) *A Study in the Theory of Investment*, Chicago: University of Chicago Press.

Haavelmo, T. (1989) Econometrics and the Welfare State, *Les Prix Nobel*, Nobel Foundation Stockholm.

Hendry, D. F. & M. S. Morgan (1995) The Foundations of Econometric Analysis. Cambridge UK: Cambridge University Press.

Hilfer, Irma (1938) Differential effect in the butter market, Econometrica 6, 270-284.

Hood, William C. and Tjalling C. Koopmans, eds. (1953) Studies in Econometric Method, Cowles Commission Monograph No. 14, New York: John Wiley & Sons.

Koopmans, T. C. (1935) *On Modern Sampling Theory*. Lectures delivered at Oslo, Autumn 1935, mimeographed.

Koopmans, T. C. (1937) *Linear Regression Analysis of Economic Time Series*. Publication No. 20, Netherlands Economic Institute. Haarlem: De Erven Bohn.

Koopmans, Tjalling C., ed. (1950) *Statistical Inference in Dynamic Economic Models*, Cowles Commission Monograph No. 10, New York: John Wiley & Sons.

Morgan, M. S. (1990) *The History of Econometric Ideas*. Cambridge, UK: Cambridge University Press.

Phelps Brown, E.H. (1937) Report of the Oxford Meeting, September 25-29, 1936, *Econometrica* **5**, 361-383.

Qin, D. (1993) *The Formation of Econometrics*. Oxford: Clarendon Press.

Tinbergen, J. (1939) *Statistical Testing of Business-Cycle Theories*. Vols. I and II. Geneva: League of Nations.

Wald, A. (1939), The Fitting of Straight Lines if Both Variables Are Subject to Error. In *Report of Fifth Annual Research Conference on Economics and Statistics at Colorado Springs, July 3-28, 1939*. Cowles Commission for Research in Economics, pp.25-28.

Westergaard, H. L. & H. C. Nybølle (1927) *Statistikkens Teori i Grundrids*, 3. udgave.

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