

MEMORANDUM

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**Tracing Haavelmo's steps from Confluence Analysis to the
Probability Approach**

*By
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by

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Two Nobel Laureates

This article narrates Trygve Haavelmo's experiences from the time he was hired as an assistant at Ragnar Frisch's Institute of Economics in 1933 until the publication of *The Probability Approach in Economics* in 1944. The title should be taken literally, it is Haavelmo's physical steps which are recounted rather than his thought processes. His meetings with others who contributed to the foundation of modern econometrics are mentioned, but with no attempt at making this a contribution to the history of econometrics. Particular attention is given to the communication between Frisch and Haavelmo in this period, also their sometimes half joking exchanges.

There are some striking, though superficial, similarities in the early career of Frisch and Haavelmo. With somewhat different motivation they both seem to have chosen the *cand.oecon.* study at the University of Oslo, for the reason that it was a short education. About three years after their respective graduations both men went abroad for studies in Europe, Frisch a couple of years older than Haavelmo due to the time spent as an apprentice in a jeweller's shop. Frisch stayed abroad more than two years in 1921-23, Haavelmo's two visits abroad in 1936-38 lasted altogether less than one year. They both got Rockefeller fellowships for studies in the United States, Frisch when he was 32 years old, Haavelmo was 28. When each of the two was around 35-36 years old, they were in the United States and faced the decision of whether to return home or accept an attractive offer to stay. Both chose to return to Norway and remained there. They both became professors at the University of Oslo when they were 36. Furthermore, both men delved deeply into theoretical statistics in the early part of their career and both have their names firmly associated with econometrics.¹

Ragnar Frisch and Trygve Haavelmo received Nobel prizes for their scientific achievements. Frisch's prize in 1969, jointly with Jan Tinbergen, was the first Nobel Prize awarded in economics, the prize to Haavelmo in 1989 was a belated recognition of his contribution to econometrics.² These honourable recognitions stand out in a Scandinavian context, in which Norway otherwise is the clear underdog. Outside economics Swedish scientists have from the prize to Arrhenius in 1903 been awarded around 15 Nobel prizes, Danish scientific achievements with Niels Bohr and others have been rewarded with close to ten prizes, against a single Norwegian prize!³

The two Norwegian prizes in economic science do not reflect, however, a strong prior Norwegian tradition in economics. Before Frisch became professor of economics and statistics at the University of Oslo Norway had had no economist or statistician of international stature. Few, if any book by Norwegian economists had been read abroad and

¹ Frisch's Nobel prize was, however, contrary to common belief, not awarded for his econometric achievements, if we may presume that the Swedish Royal Academy of Sciences chose its words carefully. While Tinbergen is credited for "pioneering work in econometrics", "an econometric model for the Netherlands" and "an econometric analysis of the market variations in the United States", there is no 'econometric' crediting of Frisch at all (apart from having been one of the initiators of the Econometric Society). The investigations Frisch led at his Institute since the early 1930s are in the official prize announcement described as comprising "dynamic processes, theory of production, national accounting, economic planning, mathematical programming, etc." (*Swedish Journal of Economics*, 1969, 300-301). No econometrics!?

² Two other Nobel Prizes in economic science have been bestowed upon Scandinavians, both of them to Stockholm School economists: Gunnar Myrdal in 1974 (shared with F. A. von Hayek) and Bertil Ohlin in 1977 (shared with J. E. Meade).

³ Odd Hassel in Chemistry in 1969. Norwegians are, however, happy to count as Norwegian prizes also those to Lars Onsager in Chemistry in 1968 and to Ivar Giaever in Physics in 1973, as both men were born in Norway. (A similar principle of accounting would return to Russia and the Ukraine the prizes of *inter alia* W. Leontief and S. Kuznets!)

very few articles published in German and English economic and statistical journals. The situation was widely different from that of the neighbouring countries. Sweden had since Knut Wicksell, Gustav Cassel and others established a strong position in economics and the after-growth with the Stockholm School was considerable. Denmark had with Harald Westergaard a statistician and economist of international renown.⁴ But over a couple of decades the international stature of Norwegian economists changed dramatically. Ragnar Frisch and Trygve Haavelmo had a deep impact on the profession and a great outpouring of international contributions. Needless to say their decisions to remain in Norway had the greatest importance for the development of economics and statistics in Norway.

Haavelmo's rise to fame is particularly notable. Although the Swedish Nobel Prize committee did not recognize and honour his achievement till 1989, the *Probability Approach* had been a classic since it was published in 1944. Haavelmo's magnum opus is by many regarded as having "founded econometrics as a separate discipline" and since the mid-1980s it has figured prominently in a large number of books and articles on the history of econometrics.⁵

What was Haavelmo's contribution? Rephrasing the title of his treatise, it could be summarized as: applying probability reasoning for empirical verification in economics. Several keywords prop up in any discussion of Haavelmo's contribution: *identification*, *autonomy*, *simultaneity*, *testing*, *prediction* and *estimation*. Perhaps it could be suggested that the underlying motivation behind his study was an urge to provide an answer as to what constitutes a proper *explanation* in economics in the confrontation between theory and empirical facts, that is also including the question of *verification*. Haavelmo attaches a fundamental role to probability considerations, adhering to the Neyman-Pearson approach. The logical structure of a proper *explanation* is similar to what according to Haavelmo would constitute a proper *testing* of the theory or a *prediction* of empirical observations. A profound interest in *explanation* can be found in Haavelmo's work from an early stage.

The article may shed some light on the genesis of the *Probability Approach* through tracing the experiences Haavelmo had and some of the influences he was exposed to, and hopefully inspire further investigations based on the evidence Haavelmo left behind. An important aspect of Haavelmo's experience prior to embarking on the quite philosophical *Probability Approach* is that Haavelmo, unlike many theorists of science, had conducted much empirical work.

Haavelmo's work has been viewed as owing much to the influence exerted on him particularly by Ragnar Frisch, Abraham Wald and Jerzy Neyman, as stated e.g. in Morgan (1990), perhaps the most widely read book about the history of econometrics. This is, indeed, also the impression one gets from Haavelmo's treatise. The influence of these three of Haavelmo's seniors was exerted in different ways. The relationship between Haavelmo and Frisch is of particular importance. Did Frisch groom Haavelmo for making a career as an econometrician? Did Haavelmo's work owe much to Frisch's influence? Did the two men have a conflict of opinion over the appropriateness of applying probability reasoning in economics? I would answer yes, yes and no.

Haavelmo's embracement of the Neyman-Pearson theory of hypothesis testing is clearly expressed in the treatise. Morgan (1990)'s account of the origin of the *Probability Approach* includes the statement that "it was apparently while trying to convert Jerzy Neyman to

⁴ The first Norwegian doctoral dissertation in economics (O. Jæger) is from 1896 on Adam Smith, at a time when the post-1870 revolution in economics was firmly entrenched in Sweden and Denmark due to Wicksell and Westergaard.

⁵ The quote is from Spanos (1989, p.409). See also Morgan (1987, 1990), Malinvaud (1988), Aldrich (1989, 1994), Qin (1989), Christ (1994), Hendry & Morgan (1995), Epstein (1987), *et al.*

confluence analysis that Haavelmo's conversion to probability reasoning occurred" (p.242). Although the statement seems to be based on direct information from Haavelmo, it is an odd statement as the "conversion" is not located in time and space. Is Morgan's statement perhaps meant to refer to the visit Haavelmo, shortly after his arrival in the United States in 1939, made to Berkeley to see Jerzy Neyman? Haavelmo was, however, thoroughly versed in Neyman-Pearson theory long before this visit, neither was confluence analysis new to Neyman. Morgan also states that the *Probability Approach* "bore signs of the evangelicism of the newly converted" (p.242).⁶ Apart from the uncalled-for religious overtones in Morgan's statement one may well ask "conversion, when, and by whom?".

In the surge of interest in the history of econometrics Haavelmo's work has figured prominently, particularly Haavelmo (1943a) and Haavelmo (1944). The interpretation of his contribution and its importance as a paradigm is not beyond controversy. Heckman (1992) is critical both of Haavelmo's paradigm and of Morgan (1990)'s interpretation of Haavelmo, asserting that "the importance of his research program to empirical economics is greatly exaggerated by Morgan" (p.882). Spanos (1989) argues that the textbook version of econometric modeling procedures with obligatory reference to the *Probability Approach*, has in fact "little in common with the methodology of Haavelmo's 1944 monograph".

Haavelmo had during his formative years contact with a number of other economists and statisticians who contributed to the statistical methods of modern econometrics, in Norway (Frisch, Olav Reiersøl), in Europe (e.g. Jan Tinbergen, Egon Pearson), in the United States (Abraham Wald, Gerhard Tintner, Joseph A. Schumpeter, Harold Hotelling, Leonid Hurwicz, Paul A. Samuelson, Lawrence R. Klein et al.), or on both sides of the Atlantic (Jerzy Neyman, Tjalling Koopmans, Jakob Marschak). The mentioning of persons exerting influence on Haavelmo is, needless to add, not meant to detract from the originality of his contributions. In retrospect one may trace, as is attempted below, the path Haavelmo followed towards the *Probability Approach*. That path has some interest in itself as Haavelmo moved between key events and central personalities in the small international community of genuine econometricians in the 1930s and early 1940s.

It may seem as if Haavelmo was lucky to be at the right spot at the right time! He graduated and began to work as research assistant in the midst of Frisch's peak scientific period. He was lucky to get to the United States with a forthcoming Rockefeller fellowship at the last opportunity before the war closed that option to Norwegians. In the United States he found himself amidst a great number of gifted scientists who had fled Europe. Haavelmo planned to stay in the United States for one year or so, but got stranded there when Norway was occupied by Germany. It was while forced to stay in the USA that he completed the publications that created his fame in the history of econometrics.

Confluence analysis

Frisch is recognized as the one who introduced modern statistical theory in Norway. He had after graduation studied statistics in Paris and London, and his doctoral dissertation of 1926 was the first in Norway on a topic of theoretical statistics.⁷ All of Frisch's published works

⁶ The same formulation is also used in Morgan (1987), p.182.

⁷ His doctoral dissertation, Frisch (1926), dealt with semi-invariants (cumulants) of the binomial and hypergeometric distribution. In the concluding paragraph he pointed towards future tasks:

"Le problème inverse: comment remonter d'une distribution empirique donnée au schéma qui a donné naissance à la distribution observée, est un problème d'un aspect assez différent. Pour le traiter d'une façon approfondie on ne peut éviter d'entrer dans des questions philosophiques et plus particulièrement dans des questions des relèvent de la théorie de la connaissance. Il nous semble que trop souvent les savants statisticiens et mathématiciens ont refusé d'entrer dans ces questions philosophiques pour se

before 1926 were on statistical topics. His main econometric contribution was the *Confluence Analysis* essay.⁸ The quotes from and discussion of Frisch's early work which follow below is primarily directed towards his view on the relevance and need for probability reasoning.

Frisch's confluence analysis was rooted in the cluster theory set out in his *Correlation and Scatter* treatise, completed during his first visit to the United States in 1927-28 and published in the not very widely distributed *Nordic Statistical Journal*.⁹ Frisch's elegant non-probabilistic theory focused on the structure of the matrix of correlation coefficients of the data set to classify different types of clustering, i.e. of deviations from a random scatter, and the interpretation of regression results.¹⁰

In December 1930 Frisch wrote a long letter on profound econometric issues to Joseph Schumpeter, just two weeks prior to the foundation of the Econometric Society. What led him to put his thoughts on paper on this occasion was as often both earlier and later in his scientific career, his perception of misleading reasoning by others. In this case it was an article by Pigou, but primarily a long essay by Wassily Leontief.¹¹ As Frisch put it:

I got a little excited over these, as I see it, misleading methods, so I looked up my own not yet finished notes on the subject and gave some new thought to the matter. It seems quite surprising to me that the problem has not yet been stated in the following simple and rather natural form.¹²

which he then proceeded to set out:

Let x_1, x_2, \dots, x_n be a set of economic magnitudes (price, quantities consumed, produced, etc.) for which we have a certain static theory, in the sense that we postulate for a priori reasons a number of structural relations:

$$(1) \quad \begin{aligned} &F_1(x_1, x_2, \dots, x_n) = 0 \\ &F_2(x_1, x_2, \dots, x_n) = 0 \\ &\dots\dots\dots \\ &F_n(x_1, x_2, \dots, x_n) = 0 \end{aligned}$$

equal to the number of variables, thus making the system determinate.

This involves, of course, the further assumption that the n relations considered really give a determinate solution for the n quantities x_1, x_2, \dots, x_n , but never mind. The relation $F_1=0$ may, for instance, represent a certain demand relation, $F_2=0$ a certain supply relation, etc. Each of the functions $F_1, F_2 \dots$ will contain a number of constant parameters that characterize the shape of the function. We may indicate this explicitly by writing the functions,

$$F_1(x_1, x_2, \dots, x_n, a_{11}, a_{12}, \dots) = 0$$

borner à traiter exclusivement les questions de la technique. C'est là croyons nous la raison de ce que l'interprétation critique du fondement et des méthodes de la statistique n'ont pas tenu pied au développement technique et l'extention croissante du champ d'application de notre discipline et dans le domaine des science sociales et dans le domaine des sciences de la nature." (Frisch, 1926, p.86).

⁸ Frisch (1934a).

⁹ Frisch (1929). *Nordisk Statistisk Tidskrift* was issued from 1922 with articles in German and English, but mostly in Scandinavian languages. A parallel English edition called *Nordic Statistical Journal* was issued from 1929. Frisch's treatise was the first econometric approach using matrix notation, hence he included an introduction to linear algebra.

¹⁰ The use and interpretations of the theory were extended in Frisch & Mudgett (1931), which also criticized current regression practices. Malinvaud (1964) applies a somewhat simplified version of Frisch's approach in its introductory chapter on econometrics without stochastic models, but is exceptional among modern textbooks in this regard.

¹¹ Frisch/Schumpeter 13 Dec. 1930. Pigou (1930), Leontief (1929).

¹² Ibidem.

$$(2) \quad \begin{array}{l} F_1(x_1, x_2, \dots, x_n, a_{11}, a_{12}, \dots) = 0 \\ \dots\dots\dots\dots\dots\dots\dots\dots\dots\dots\dots\dots \\ F_n(x_1, x_2, \dots, x_n, a_{n1}, a_{n2}, \dots) = 0 \end{array}$$

the set of quantities a_{ij} being the constant parameters in question. The problem of determining such a set of parameters for actual data is an interesting example of an econometric problem.

Now we have the curious situations that if the material at hand fulfills our assumptions it is impossible to determine these constants a_{ij} that express the nature of our assumptions, because in this case we would only have a single observation, namely, the one corresponding to the solution of the system (1). But if our assumptions are not fulfilled, then it may be possible to determine what they were, that is to say, now it may be possible to determine the constant a_{ij} .

Suppose, for example, that the functions $F_1, F_2 \dots$ contained also another set of variables, $\xi_1, \xi_2, \dots, \xi_m$, m being at least equal to 1. Our set of structural relations will take on the form

$$(3) \quad \begin{array}{l} F_1(x_1, x_2, \dots, x_n, a_{11}, a_{12}, \dots, \xi_1, \xi_2, \dots, \xi_m) = 0 \\ F_2(x_1, x_2, \dots, x_n, a_{21}, a_{22}, \dots, \xi_1, \xi_2, \dots, \xi_m) = 0 \\ \dots\dots\dots\dots\dots\dots\dots\dots\dots\dots\dots\dots \\ F_n(x_1, x_2, \dots, x_n, a_{n1}, a_{n2}, \dots, \xi_1, \xi_2, \dots, \xi_m) = 0 \end{array}$$

Furthermore, let $\Omega(\xi_1, \xi_2, \dots, \xi_m)$ now be the frequency distribution of the set $(\xi_1, \xi_2, \dots, \xi_m)$. Then to this frequency distribution of the set there corresponds by (3) a certain frequency distribution $w(x_1, x_2, \dots, x_n)$ of the set (x_1, x_2, \dots, x_n) . And this latter distribution is known from observation. We see that now we really do get variation in the set (x_1, x_2, \dots, x_n) . This we may call the principle of at least one-dimensional indeterminateness (since m must be at least equal to 1).¹³

Frisch's formulation suggests that he looked at the way probability entered economic relations as deeply embedded variables rather than residual terms. He then proceeded to set out to Schumpeter the two-variable demand-supply case, which was the case Leontief had studied.¹⁴

The critique of Leontief's method for estimating both the demand and the supply curve from a set of price-quantity data dealt with in the letter to Schumpeter was followed up in Frisch's *Pitfalls* essay, in which he argued on the basis of the non-probabilistic approach in his cluster analysis, applied to the two-variable case.¹⁵ Frisch clearly saw Leontief's approach as an example of erroneous reasoning based on what Frisch denoted as "fictitious determinateness created by random errors".

The *Confluence Analysis* monograph can also be seen as an outgrowth and improved version of the theoretical structure of Frisch's cluster analysis after an intensive period of applying the methods to empirical problems.¹⁶ Frisch's general attitude towards the quality of statistical

¹³ Ibidem.

¹⁴ Shortly afterwards Frisch tried to draw Harold Hotelling's attention to common pitfalls in correlation analysis among American statisticians, expressing his worry that his critique he had set out in the chapter "Types of clustering and the non-significance of partial correlation" in the *Correlation and Scatter* essay seemed to have had no impact. He encouraged Hotelling to address the issue, but Hotelling brushed him off somewhat cynically: "I do not believe that you need to worry about most American statisticians not knowing it, since most of these gentlemen know nothing whatever of a theoretical nature" (Frisch/Hotelling 21 Feb. 1931, Hotelling /Frisch 26 Feb. 1931). Hotelling's cynicism is consistent with Wolfowitz' postwar assessment that "Hotelling was already then [1938] one of the leading American teachers of the modern theory of statistics. He was one of the few voices in the wilderness proclaiming the importance of the new subject" (Wolfowitz, 1952, p.2).

¹⁵ Frisch (1933).

¹⁶ The *Confluence Analysis* seems not to have been conceived as a book, but rather as an article that grew wildly out of hand, summarizing the methods Frisch with various co-workers and assistants had worked intensely on over about two years. Frisch wrote on the spur of the moment in April 1934 to the editor and publisher of the

methods used in economics at the time he wrote the *Confluence Analysis* comes through in a sweeping statement on the opening pages: “I believe that a substantial part of the regression and correlation analysis which have been made on statistical data in recent years is nonsense for this reason”. The “reason” was the negligence of confluent relationships: “If the statistician does not dispose of an adequate technique for the statistical study of the confluence hierarchy, he will run the risk of adding more and more variates in the study until he gets a set that is in fact multiple collinear and where his attempt to determine a regression equation is therefore absurd”.¹⁷ In modern terminology Frisch attempted in the confluence analysis to solve simultaneously the errors-in-variables problem, the simultaneity problem and the model choice problem.¹⁸

In the Introduction to the *Confluence Analysis* he set out the nature of his investigation, referring to the weakness of the method suggested in *Correlation and Scatter* as having no criteria for judging the *significance* of the scatterances:

In the subsequent years I reverted to the question on and off, on various occasions, attempting to push the analysis further. The line of approach which suggest itself from the view-point of sampling theory is to attempt to find the sampling distribution of the scatterances. I did not concentrate much on this aspect of the problem, primarily because I felt that – at least when the data are of an economic sort – this would not be the most fruitful way of approach. Indeed, if the sampling aspect of the problem should be studied from a sufficiently general set of assumptions, I found that it would lead to such complicated mathematics that I doubted whether anything useful would come out of it. (p.7)

Instead of basing his approach on an explicit consideration of probability aspect Frisch offered his method as an “attack on the problem more from the experimental side, working out numerically – on actual economic data as well as on constructed examples – various other types of criteria which intuitively and heuristically may suggest themselves. These experiments converged towards a definite method which after applications to various kinds of data, was found to give satisfactory and plausible results”. (pp.7-8)

He did not comment much in the continuation about probability considerations apart from a passage in the middle of the treatise:

It is on purpose that I have not attempted to give any formal and rigorous definition of the “probability” for a specified result obtained by the different minimalisations. Such a formal definition may indeed be obtained by starting from many *different* types of abstract schemes. Each scheme will lead to a particular definition of the probability in question. By focussing too much attention on the probability there is some risk that one will forget the very relative and limited meaning which must be attached to such a numerical computation of a “probability”. It is indeed only in a very special meaning that any such probability can be said to measure the “significance” of the results. At least, to start with, I believe it will be a better application of time and energy to work experimentally with the method and rely on one’s

Nordic Statistical Journal, Thor Andersson, and offered an article to be sent within one week. A couple of days later he wrote again to say that the article might come to more than 25 pages (the usual limit). One month later he offered to refrain from a honorarium if the article could still be published even though it surpassed the limit somewhat. In June he indicated that the article would come to 125 pages. Frisch completed shortly afterwards the manuscript, which in the end came to 192 pages. The *Confluence Analysis* was not published in the journal after all, because the journal folded when the editor & publisher Thor Andersson got ill and later died in 1935. The 1000 reprints which had been ordered by Frisch, were delivered, as reprints from a journal issue that never appeared!

¹⁷ Frisch (1934a, p.6).

¹⁸ He thus tried to do too much at the same time in the opinion of Zvi Griliches who also credited Frisch’s work as an early contribution to the principal components and factor analysis literature, see Griliches (1974).

intuitive judgement of whether a given spread in the various determinations of a given regression coefficient is reasonable or not. (ibid, p.88)

We may reasonably assume that Frisch's early work, particularly the confluence analysis, provided Haavelmo's initiation to econometric analysis and was studied thoroughly. Before Frisch rushed off to Colorado Springs in July 1934, he sent a note to the editor of *Nordic Statistical Journal* that Haavelmo would proofread in his absence. Haavelmo thus literally became the first person to study the *Confluence Analysis* essay.¹⁹ One may presume, and perhaps not unreasonably, that an important part of Haavelmo's original inspiration towards the probability approach came from Frisch's ideas and from his work with Frisch on applying confluence analysis.

In the master's workshop: Oslo 1933-37

The University Institute of Economics was founded as an empirical research institute in 1932 with Frisch and Ingvar Wedervang as co-directors.²⁰ Frisch wanted an institute run as a laboratory, acquired the computational equipment he could afford and developed a range of numerical methods for analysis of time series and for efficient use of the bunch map technique of his confluence analysis. He hired a large number of assistants to work under his direction.

Haavelmo graduated in spring of 1933. He was hired as an assistant immediately afterwards and in 1935 appointed to Frisch's "chief computer".²¹

Frisch's scientific work attracted a number of visitors, not least the econometric laboratory work that Frisch conducted. The first long-term visitor at the new Institute was Frederick V. Waugh who stayed in Oslo most of the academic year 1932/33, working closely together with Frisch. Waugh belonged to the U.S. agricultural economists, whom Karl Fox, serving as the historian of this tradition, has characterized as "world leaders in applied econometrics during 1917-33", with Waugh and M. Ezekiel as the foremost representatives. Waugh was impressed by Frisch's approach to estimating the marginal utility of money and that was the main reason for his visit. In Oslo Frisch absorbed him in econometric work, especially the use of "bunch maps".²² Frisch's work with Waugh continued with the Australian Maurice Belz in the autumn of 1933. The results from the work with Waugh and Belz figures prominently in the *Confluence Analysis*.

Among the other visitors at the Institute that may have exerted an influence on Haavelmo's development were Jan Tinbergen, Tjalling Koopmans, and Erich Schneider. From 1933 Frisch was Editor of *Econometrica* and thus gave Haavelmo excellent opportunities of following the development of the new association and journal and later used him as referee.

¹⁹ The meticulous Haavelmo was at the time an inexperienced proofreader, overlooking that Frisch, who was notoriously inexact in references to his own earlier and future publications, referred to his *Correlation and Scatter* (Frisch, 1929) repeatedly on the opening pages as being published in 1928 with the same misdating also in Frisch & Mudgett (1931), perhaps causing Malinvaud (1964) to make the same inexact reference 30 years later.

²⁰ The events and circumstances that went prior to the founding of the Institute and ensured its financial support from Rockefeller Foundation are set out in Bjerkholt (2000a).

²¹ There is some evidence that Haavelmo after two years at the very low pay that Frisch offered his assistants, applied for and was offered a clerical job in the Social Security Administration in the spring of 1935. Frisch's may have bought him back with double pay and a new title!

²² See Fox (1986, 1989). Fox (1989) noted that Frisch's influence on Waugh was paramount and that the latter's work changed decisively after his year with Frisch who also sent Waugh on to three of his associates in Europe: F. Divisia, E. Schneider and J. Tinbergen before he returned to USA. The result of Waugh's work was Waugh (1935), another outcome of the visit was Frisch & Waugh (1933).

Frisch used Haavelmo for a variety of tasks, not only as “computer”. Haavelmo assisted in preparing lecture notes for the most important of Frisch’s lecture series, including Macrodynamics (1933/34), Monetary theory (1934 and 1935), Time series analysis (1934/35), and others, and probably exerted considerable influence on Haavelmo’s interest in macroeconomic theories as well.

In the autumn of 1935 Tjalling Koopmans visited. Koopmans had studied mathematics and theoretical physics before he became one of Tinbergen’s students in 1934. He decided to write a doctoral thesis in mathematical statistics and wanted to spend half a year with Frisch whom he had never met (we may presume that Tinbergen had exerted an influence). Koopmans wrote to Frisch in March 1934, indicating his chief interest as being “the problems, arising from the circumstances that classical sampling theory does not regard cases in which observational series develop in time in such a way, that the probability distribution of the second term is not independent of the value attained by the first”. He also outlined some of his ideas in more detail.²³

Frisch was enthusiastic and gave an elaborate and critical answer to Koopmans’ scientific ideas, inviting him to become a student of confluence analysis:

With regard to the topic you suggest, here is my reaction. The problem you mention seems to me to resemble very closely those that have been discussed and more or less completely solved by English authors like Student, R. A. Fisher and his school and the group of mathematicians connected with the Galton laboratory. I do not know how much you know of this literature and how deeply your setting of the problem penetrates, but my first impression was that - at least the set-up mentioned in your letter - does not seem to be very promising of yielding some fundamentally new results. Of course, I may not fully have realised your intentions, but at least I think you ought to point out in what sense the results you are looking for should extend beyond the results obtained by the above mentioned group of mathematical statisticians. To me it seems that the point where sampling theory now needs to be developed is not so much along the lines you suggest as in the direction of studying the limiting cases that arise when the set of variables considered are nearly connected with more than one linear relationship. In other words, what happens when the set of observational variables become multiply flattened? You may know that this has been the topic of a book which I have recently published. ... here there is room for much further work, particularly in the direction of developing sampling distributions of the parameters involved. ... The essence of this problem comes in when a frontal attack is made on the basic problems connected with multiply linear connections. May be you would like to devote some energy to these kinds of questions.²⁴

Koopmans had also contacted both Fisher and Neyman and Pearson and visited London before he came to Oslo. At Frisch’s Institute he gave a series of lectures under the title *On Modern Sampling Theory*.²⁵ The lectures were divided in three parts on fundamental concepts, Fisher’s theory of estimation, and Neyman and Pearson’s theory on hypothesis testing. Most likely it was the first set of lectures ever given on Neyman-Pearson theory at an economic institute. The lectures were followed by Frisch and some of his Norwegian actuarial students and assistants, foremost Haavelmo and Olav Reiersøl, but also by foreign visitors, e.g. Georg Rasch (Denmark), Georges Lutfalla (France), John McIntyre Cassels (England, p.t. Harvard).

Koopmans’ visit provided Haavelmo’s introduction to Neyman-Pearson’s theory. To what extent Haavelmo took part in discussions on the appropriateness of probability reasoning during Koopmans’ visit is less clear.²⁶ Frisch’s scepticism towards probability reasoning as a

²³ Koopmans/Frisch 25 March 1935.

²⁴ Frisch/Koopmans 11 April 1935.

²⁵ Koopmans left densely written lecture notes, Koopmans (1935).

²⁶ The author queried Haavelmo on this in 1997 and got the reply that he was not privileged to take part in the

fruitful approach in practical econometric work must have been a topic during Koopmans' visit. In October 1935 towards the end of Koopmans' stay Frisch received a letter from an American statistician, Paul G. Hoel, who also wanted to visit.²⁷ Frisch extended an invitation, elaborated enthusiastically about the ongoing activities, with Koopmans' work described as an attempt to build a bridge between the confluence analysis and Fisher's sampling approach, summarizing the main ideas discussed including his own views as follows:

The difference between these two points of view is this. In sampling theory, in order to test the significance of a statistical observation, one puts up the fiction of a "universe", that is some big collection from which the actual observations are "drawn" in a more or less "accidental" manner. Whatever assumptions one makes are made in the form of assumptions about this universe. This point of view is fruitful, it seems to me, in problems concerning experiments that can be controlled. For instance, agricultural or biological experiments. But this theory is very inadequate when it comes to applications in economics, or in social sciences in general, where we most of the time have to accept observations that are presented to us without our being able to influence the results to any considerable extent. In these cases all the problems of confluence analysis crop up, and these can, it seems to me, be better treated by another type of analysis, namely an analysis where the assumptions being produced are assumptions about the sample itself. For instance, one may assume that each observation is a sum of a systematic part and a "disturbance", and then introduce assumptions concerning what has been the connections, or lack of connections, between the disturbances in the sample. In this way one arrives at identities, exact upper and lower limits, etc., not results which are formulated in probability terms. One does have a means of investigating how a particular constellation of assumptions entails a particular consequence for the result obtained. This analysis of the effects of alternative assumptions is very important for applications to economics. This is of course only a very rough outline of the difference between the two approaches. If I should give a fuller statement I would have to explain that in some sense, the notion of probability comes in in my approach and that, after all, there may be some points of contact between the two approaches. But it would lead too far to go into this in a short letter. I mention it in order to suggest to you a field of research, which, I think, is particularly important and very intriguing.²⁸

One may find it a pity that Frisch did not choose to give a "fuller statement".

After the visit to Oslo, Koopmans went back to London in November 1935 to discuss further with Fisher and Neyman, who at this stage did not talk to each other.²⁹ Koopmans' thesis was completed soon after and gave a thorough discussion of Frisch's confluence analytic approach.³⁰

Haavelmo was also involved in a number of empirical studies that Frisch contracted to do at the Institute, mostly related to demand for food and agricultural products. Some of these have never been published, i.a. an analysis of the demand for beer undertaken for the breweries' association with Frisch and Haavelmo as co-authors of the final report. A study of the demand for milk was published as a joint paper.³¹ Demand for food and agricultural

real discussions during Koopmans' visit.

²⁷ Paul G. Hoel, of Norwegian extraction, had followed Frisch's lectures on time series analysis in Minnesota in the spring of 1931 and had later completed his doctorate. He later wrote a textbook in statistics.

²⁸ Frisch/Hoel 15 Oct. 1935.

²⁹ See Reid (1982), pp.124-128.

³⁰ Koopmans' thesis was jointly supervised by Tinbergen and Hans Kramers, the leading theoretical physicist in Netherlands at the time, and the degree awarded in November 1936. It was published as Koopmans (1937).

Koopmans presented his doctoral thesis at the Econometric Society meeting in Annecy, September 1937.

³¹ Frisch & Haavelmo (1938).

products became another area into which Haavelmo also got his initiation from Frisch and to which he would devote considerable effort later on.

Shortly after Haavelmo's visit to London in the autumn of 1936 (see next section) he expressed to Frisch his interest in a longer stay to study abroad. Frisch got enthusiastic and advised a stay of at least two years abroad. In the spring of 1937 Haavelmo was awarded a small Norwegian grant for further study of statistical theory and techniques abroad. The amount was not even sufficient for one year, but Frisch indicated he could get him a Rockefeller fellowship as well.

While attending the Third Cowles Commission Research Conference in Colorado Springs in July 1937 Frisch gave some thought to Haavelmo's plan for studying abroad.³² His first advice was to "read a considerable amount of mathematics before leaving Norway". Haavelmo was an economist who at the time could hardly be regarded as badly equipped with mathematics. Through his work with Frisch on confluence analysis and time series analysis he must have mastered matrix algebra and harmonic analysis quite well and had far ranging experience in numerical methods. His mathematics level was, however, low compared to that of Frisch. Frisch advised further to concentrate on mathematical statistics in the first part of his stay abroad by going back to work with Jerzy Neyman in London and "stay there as long as you think is necessary in order to get a good foundation in sampling theory". Frisch advised then to take up very thoroughly "the construction of demand and supply curves and similar investigations" by studying with Jakob Marschak at the Institute of Statistics in Oxford. These two assignments ought to cover the first year.

For the second year Frisch strongly advised going to United States. His more specific suggestion was to see Louis H. Bean of the US Department of Agriculture and study his work on demand and supply curves for agricultural commodities. More specifically Haavelmo ought to familiarize himself thoroughly with "the various special investigations [Bean] has made on different kinds of commodities regarding their price fixation, acreage planted ... much is to be learned concretely and practically from these various investigations". Bean had according to Frisch "an excellent economic judgement and a good bit of common sense".³³ Furthermore, Frisch advised Haavelmo during his time in the United States to see Theodore Yntema at the University of Chicago, Harold Hotelling at Columbia University and Charles F. Roos, one of the founders of the Econometric Society, now working as a Research Director of Mercer-Allied Corporation in New York.³⁴ Finally, Haavelmo ought to get out to Colorado Springs and "see the work of the Cowles Commission".

Haavelmo continued to work with Frisch until the end of the 1937, i.e. to finish the milk demand study which became a fairly large project. In the autumn 1937 Tinbergen visited the Institute. There would naturally be talk and discussions about the work going on at the League of Nations, not least because of Tinbergen's use of Frisch's methods in the work. Haavelmo was very interested and decided to visit to Tinbergen in Geneva when he went abroad.³⁵

³² Frisch/Haavelmo 12 July 1937. These conferences were long-lasting events, the Third Conference lasted from 28 June until 23 July 1937.

³³ Frisch's advice may have been influenced by whom he was together with at the time. Bean was also attending the Cowles Commission conference, presenting two papers on agricultural demand and supply. These inspired Frisch to write down a memorandum about Bean's methods of which he enclosed a copy for Haavelmo.

³⁴ Both C. F. Roos, T. Yntema and J. Marschak (three successive research directors of the Cowles Commission) attended the Colorado Springs conference. H. Hotelling was at the time President of the Econometric Society. Roos, co-founder with Frisch of the Econometric Society, was pupil of Griffith C. Evans, a highly regarded mathematical economist who never really got into econometrics. Perhaps Roos did not either, but he was the first to exploit econometrics commercially, founding a consulting firm called the Institute of Applied Econometrics.

³⁵ Shortly after Tinbergen left a vacant position for an economist at the League of Nations was announced.

Journeyman abroad: Oxford and London 1936

Frisch and Haavelmo travelled together to the sixth European meeting of the Econometric Society, which took place at New College, Oxford, 25-29 September 1936.³⁶ The earlier meetings had taken place 1931-35 in Lausanne, Paris, Leyden, Stresa and Namur. Frisch had played a major role in preparing the first two meetings and had been an active participant in all but the Stresa meeting. Haavelmo had never been to an international meeting, neither had he yet been invited to become a member of the Econometric Society. The Oxford meeting was the largest of the European meetings so far with 64 participants.

Top billing on the agenda was the discussion of Keynes' *General Theory*, which had been published in February the same year. The first half of Saturday 25 September was used for a symposium on "Mr. Keynes' System". The contributions were by Roy Harrod, John Hicks and James Meade.³⁷ Was it perhaps the first discussions of Keynes in an international forum of economists? Needless to say, the topic was of the greatest interest to Haavelmo.

The Saturday afternoon sessions continued with macroeconomics. Frisch spoke on "Macrodynamic Systems leading to Permanent Unemployment", drawing on a model of a corn economy that he had discussed in his macrodynamic lectures in Oslo in 1933/34.³⁸ Frisch's concluding remark, according to the report, was that as there already existed 10-15 fully-developed mathematical systems of some plausibility "the task was no so much to develop new systems as to test different systems against the facts". How difficult a task that would be was hardly clearer to any participants than to Jan Tinbergen who spoke after Frisch in the afternoon session on "Dynamic Equations Underlying Modern Trade Cycle Theories". Tinbergen was already well ahead in his project for the League of Nations, that would eventually appear as Tinbergen (1939), and gave an impression illustrated with examples of his approach to empirical macroeconomic modelling. The term *macroeconomic* had not yet been used in writing, but Tinbergen may well have used it in his presentation.

At a colloquium Saturday night Frisch presented an "ideal programme" for macrodynamic studies, which is of interest here as an influence on Haavelmo, as also reflected in the Haavelmo's paper presented later in the meeting.³⁹

Haavelmo was interested and encouraged to apply by Frisch and others, but did not get the position, even with a reference letter from Tinbergen.

³⁶ The report from the meeting in *Econometrica* by E. H. Phelps Brown is quite detailed (*Econometrica*, 5, 361-383).

³⁷ By the time the report on the meeting appeared in *Econometrica* both Harrod's and Hicks' contributions had already been published as "Mr. Keynes and Traditional Theory" (*Econometrica* 5, 74-86) and "Mr. Keynes and the Classics; a Suggested Interpretation" (*Econometrica* 5, 147-159), respectively. Meade's contribution was "A Simplified Model of Mr. Keynes' System", summarized in the report over more than two pages including an eight equation model (*Econometrica* 5, 361-363).

³⁸ Frisch (1934b). Although Frisch preferred the term *macrodynamic* he had in these lectures introduced the terms "makroøkonomisk" and "mikroøkonomisk", probably the first time in writing in any language, Frisch (1934b), p.8506.1.

³⁹ The programme was set out in the report as follows (slightly abbreviated):

A. Theoretical inquiry: (1) Define your variables. (2) State the structural relations which you suppose to exist between the variables. (3) Derive a number of confluent relations, which lead to confluent elasticities, showing the response of one variable in a certain sub-group to another when all the rest are held constant. (3a) Use these relations for reasoning about variations compatible with the subsystem. (3b) Consider the response to the system of exogenous shocks: a dynamic analysis leading to criteria of stability. (3c) Consider how the whole system will evolve in time.

B. Statistical Inquiry: (4) Obtain some final equations. A final equation is a confluent relations which is reduced to its smallest degree of freedom, and in which the coefficients have a statistically uniquely determined meaning. Never try to fit to the data anything but a final equation. (5) [S]crutinise the data, and derive empirical formulæ by the statistical technique now known as "confluence analysis". (6) If the final equation contains only one

On Sunday morning 27 September R. G. D. Allen read the paper “The Assumptions of Linear Regression”, which was in a Frischian vein (also using Frisch’s notation) on the determination of limits for the true regression coefficient between two variables with measurement errors.⁴⁰ After Allen followed Jerzy Neyman’s only appearance at an Econometric Society meeting. His topic was a “Survey of Recent Work on Correlation and Covariation”. In Part III of his presentation he discussed “the Neyman-Pearson theory of testing hypotheses”. The report’s summary of the presentation ran to more than three pages.⁴¹

Monday morning 28 September it was Haavelmo’s turn. He presented his first paper at an international conference on “Confluent Relations as Means of Connecting a Macrodynamical Subsystem with the Total System”.⁴² The paper was about the meaning of *explanation* in macroeconomics when the structural model was only an underdetermined subsystem. Haavelmo’s solution was to enhance the subsystem with confluent relations known to exist. Discussing Haavelmo’s presentation Jakob Marschak queried the distinction between structural and confluent relations “suggesting that they differed only with respect to the *source* of the data, both being ultimately empirically determined”. Frisch answered by expounding the idea of *autonomy* without introducing the term.⁴³

After the Oxford meeting Haavelmo remained in England for a couple of months until the beginning of December to study statistics at the Department of Statistics, London University College. There he followed lectures by Egon S. Pearson on General Statistical Theory and by Jerzy Neyman on Testing Statistical Hypotheses and on Orthogonal Polynomials. He also did numerical calculations for Pearson. During his visit Haavelmo wrote a memorandum on “Standard errors on regression coefficients in multivariate sets” that had come out of his work with Neyman and Robert Jackson, a research worker at the Department. Frisch sent comments back clarifying his view with regard to the distinction between a “mechanical regression”, an “expected-regression” and a “structural relation” according to the confluence analysis.⁴⁴

Haavelmo worked with Jackson on confluence analytic problems, trying to evaluate numerical tests for regression coefficients. They prepared a joint paper, looking forward to Frisch’s comments. When Haavelmo got back to the Institute Frisch was so absorbed in time series problems that he had no time to read the paper. Reiersøl had at the same time also worked on confluence analytic problems and showed that the true regression coefficient was

variable, and is linear, construct the corresponding characteristic equation and consider its roots. The actual time-shape will now be a weighted average extending over [a stream of erratic] shocks, the weights of the average being those given by the system as it would proceed in isolation. (7) Fundamental inversion problems: (a) to determine the system of weights from a given time-shape; (b) to determine the shocks.

(8) Attempt a forecast using the weights determined by the inversion, and assuming – in the absence of better information – the future shocks to be zero.

(*Econometrica* 5, 365-366). Frisch’s “ideal programme” is discussed in Aldrich (1989).

⁴⁰ The results were hardly new to Frisch and Haavelmo. Allen reworked and published his paper in *Economica*, May 1939, just prior to Wald (1939)’s solution of the problem.

⁴¹ Most of the summary related to part III on testing had according to the report been prepared by Frisch (*Econometrica* 5 367-371). It seems that Frisch during the meeting wrote down an extensive note on Neyman-Pearson’s theory for his own purpose and later decided to include it in the report.

⁴² *Econometrica* 5, No. 4, Oct. 1937, 373-374. Haavelmo’s contribution was later extended with an empirical application published as Haavelmo (1938).

⁴³ Frisch also presented a second paper at the meeting on the determination of the “money flexibility”. It was an attempt at countering the severe criticism that Abram Burk (Bergson) had raised against *New Methods of Measuring Marginal Utility* (Frisch, 1932) in *Review of Economic Studies* 4, 33-52.

⁴⁴ Frisch could also update Haavelmo on the work going on in Oslo where Reiersøl had continued working as Frisch’s assistant: “You will be interested to know that we are now working out a complete technique giving upper and lower limits for the structural coefficients in the case $n=4$. In principle the technique is general but will involve very heavy complications for large values of n ”. Frisch/Haavelmo 23 Oct. 1936.

limited by the spread of the beams in the bunch map. Haavelmo could also report back to Jackson that Koopmans' doctoral dissertation which was forthcoming as a book, was, indeed, "dealing just with our confluence problem".⁴⁵

How close was the interaction with Neyman and how much impact did the visit have on Haavelmo? Shortly after his return Haavelmo wrote to Egon Pearson that he was about to use sampling theory on certain problems in the analysis of business cycles generated by erratic shock. Three years later Haavelmo chose to visit Neyman in Berkeley before he went to other universities.⁴⁶

While staying in London Haavelmo also frequented Friedrich Hayek's weekly seminar on business cycle problems at London School of Economics, invited by Hayek.⁴⁷

The first trip abroad must have been a great experience and inspiration to Haavelmo.⁴⁸ He had met leading scholars in both economics and statistics and become acquainted with a large number of researchers in the field.⁴⁹

On the road: Berlin, Geneva, Paris and Oxford 1937/38

After the first trip abroad Haavelmo worked another year at the Institute before he went abroad again. While the Rockefeller fellowship, and hence a visit to the U.S., still was nothing more than a remote possibility, Haavelmo chose to use his Norwegian grant to visit several institutions in Europe. He did not heed Frisch's advice about going back to Neyman, but included a visit to Marschak at Oxford in his plan. The departure was put off until the beginning of December 1937.

The first month and a half was spent in Berlin as a visitor to the well known Institut für Konjunkturforschung, directed by Ernst Wagemann. The Berlin visit seems partly to have been an assignment by Frisch. Most of the time in Berlin Haavelmo spent at the Meteorologisches Institut's department for 'Periodenforschung', i.e. time series analysis. It was directed by Professor Karl Stumpff and had a range of advanced equipment for harmonic analysis. Frisch did not know either Wagemann or Stumpff very well. He sent with Haavelmo introduction letters to both of them emphasizing Haavelmo's "thorough founding in economics and statistics" as well as his "energy and interest". Frisch let the addressees know that he was convinced Haavelmo also "in the future [will] do excellent work in his chosen field", adding "that Mr. Haavelmo is a perfect gentleman, whom I have always trusted in all matters."⁵⁰

⁴⁵ Haavelmo/Jackson 23 Dec. 1936. Koopmans dissertation had been accepted and a copy had presumably arrived at the Institute.

⁴⁶ Haavelmo is not mentioned in Neyman's biography by Constance Reid, neither is the Oxford meeting. The biography appears to have been written in close interaction with Neyman.

⁴⁷ Hayek was apparently not present at the Oxford meeting, but his business cycle theory figured prominently in Tinbergen's discussion. Frisch had been in touch with Hayek since 1928, while still at the Österreichisches Institut für Konjunkturforschung, and had published an article by him in *Econometrica* in 1934.

⁴⁸ Haavelmo had received a small grant from his own University as well as from the British Council. During the stay at London University College he also gathered material on the system of teaching statistics in England, apparently with an aim of becoming a teacher in statistics, which he indeed became.

⁴⁹ Shortly after Haavelmo's return Frisch proposed him as a member of the Econometric Society. Haavelmo was informed in June 1937, gratefully accepted and became a member from 1937. His fellowship followed immediately upon the publication of the *Probability Approach*.

⁵⁰ Haavelmo got during his short stay also an impression of life under Hitler's Nazi regime, reporting home about the high work intensity everywhere and overfilled theatres, cinemas and restaurants. He also noted the more somber mood at the University with constant fear of losing positions "for political reasons". On entering a room with a "Guten Tag", he would often be met with fearful faces and a loud and clear "Heil Hitler".

Haavelmo was given access to Stumpff's equipment and tried it out on data that had been analysed in Oslo by Frisch's methods to compare the efficiency.⁵¹ Frisch had worked with Haavelmo and Reiersøl since his lectures on time series in 1934 on these problems and conducted a large number of numerical experiments, not least to solve the "inversion problem".

Stumpff's methods for harmonic analysis were described in a note by Haavelmo.⁵² The harmonic analysers were based on light interference. The results came out as photographs and punched cards which then had to be interpreted. Haavelmo worked closely with Stumpff who showed great interest. Haavelmo was not impressed with the results, they were hardly as accurate as the results achieved in Oslo.

On the eve of his departure from Berlin he sent home his report. Haavelmo concluded that the methods were useful as they required little work, even with several components included in the series, but they were not able to solve Frisch's inversion problem. Frisch studied the results sent home by Haavelmo and concurred.⁵³ While he was in Berlin Haavelmo also found time to heed Frisch's advice and read mathematics.

From Berlin Haavelmo travelled to Geneva in mid-January 1938, primarily to work with Tinbergen at the League of Nations' Financial Section.⁵⁴ Geneva was in the mid-1930s a beehive of economists. At the League were beside Tinbergen also James Meade, Marcus Fleming and Ragnar Nurkse. J. J. Polak worked as Tinbergen's assistant. At the ILO worked Hans Staehle. Several economists worked also at the Institut des Hautes Etudes, also one of the European institutions supported by Rockefeller Foundations, inter alia Lionel Robbins and Abraham Wald.

Wald had, however, left before Haavelmo arrived. He had been in Geneva from September to December 1937 and during that time he had collaborated with Tinbergen in establishing a system of equations reflecting the chief forces acting in business cycles. From Geneva Wald went back to his position at the Institut für Konjunkturforschung in Vienna, but fled from Austria after Anschluss in the spring of 1938 and came to the United States in the summer of 1938.⁵⁵

(Haavelmo/Wedervang 19 Feb. 1938)

⁵¹ Frisch had constructed test data sets from drawings of a Norwegian lottery ("Pengelotteriet") and sent data to Haavelmo by mail. The data were held up for a while by the German censorship suspecting that the data were ciphered messages!

⁵² Stumpff's methods had been set out in his book *Grundlagen und Methoden der Periodenforschung*, to which Haavelmo made page references.

⁵³ Stumpff's equipment was also tried out on data Frisch and Haavelmo referred to as the "Yale Data", a constructed data set with four sinusoidal components and one erratic component, which Frisch had analysed and used as teaching material at Yale and Minnesota in 1930/31. He concluded that the 20 year wave in the Yale data had after all been more precisely determined by himself using linear operations than by means of Stumpff's labour saving, but expensive equipment.

⁵⁴ The League of Nation's Economic Intelligence Service had for several years, supported by grant from the Rockefeller Foundation, been engaged in an inquiry into the causes of the recurrence of depressions. The outcome of the first phase in this inquiry was Gottfried Haberler (1937)'s *Prosperity and Depression: A Theoretical Analysis of Cyclical Movements*. The second phase was the statistical verification and mathematical testing of the alternative explanations. The investigation was led by Tinbergen since 1936. Alexander Loveday was in charge of the Financial Section and the Economic Intelligence Service, see M. Hill: *The Economic and Financial Organization of the League of Nations*, Washington: Carnegie Endowment for International Peace, 1946.

⁵⁵ Wald had been a student and later a close collaborator of Karl Menger, who had left for the United States. Wald's sojourn in Geneva may have been mentioned to Haavelmo by Tinbergen, but Wald had not yet done much work on statistical problems. Frisch had had scientific contact with Wald and in connection with price index problems and encouraged him to write two articles on the topic for *Econometrica*. He had probably

Haavelmo found Geneva a much more advantageous place to continue his studies than Berlin, in fact. He was taken care of by Tinbergen who gave him a thorough introduction to the ongoing work and arranged for him an opportunity to work in the splendid offices of the League with access to the Financial section's computer equipment. Tinbergen's office faced South towards the park of the *Palais des Nations*, the lake, and on clear days the majesty of Mont Blanc. Haavelmo sent home an impression that there could hardly be a more ideal place for him to visit, he regarded clearly Geneva at the time as a leading centre in modern economic research.⁵⁶

Tinbergen was close to completion of his first League volume when Haavelmo arrived and was working hard on the second volume to be ready for a special conference to be convened in Cambridge in July 1938. Tinbergen applied Frisch's confluence analytic methods intensively for this purpose and may have found it convenient to have Haavelmo around.⁵⁷

Tinbergen had brought Haavelmo into a little informal group of 6-7 "econometricians" some of whom he met with almost daily. One of the members of the group was Hans Staehle with whom Haavelmo discussed the measurement of the propensity to consume.⁵⁸ In a letter home to Frisch Haavelmo could report that *Confluence Analysis* had "gained firm ground" in Geneva. Terms like "bunch map" and "multicollinearity" were used in daily communication without explanations. Haavelmo rewrote his paper from the Oxford conference, applying the theoretical ideas empirically to stock market data in close contact with Tinbergen. On receiving the manuscript Frisch sent him encouraging words, introducing a new shorthand code: "Keep on working! And remember what I mean about Carthago (i.e. that you do not know enough mathematics)!"⁵⁹ The paper resulted in Haavelmo's first *Econometrica* article.⁶⁰

From April 1 Haavelmo was in Paris. Frisch had, of course, put him in touch with his old friend François Divisia. Frisch had sent with him introduction letters also to Philippe Le Corbeiller and to the statistician Georges Darmois. Haavelmo visited the statistical department of Institut H. Poincaré in Paris, where he got opportunity to follow at close range a major study of production efficiency in French manufacturing.⁶¹

also been instrumental in getting Wald the offer as a research associate of the Cowles Commission, that gave him the opportunity to leave for the U.S. 1938. Frisch had persuaded Wald to visit him in Oslo and leave for the U.S. from a Scandinavian port, but Wald was in too much of a hurry to make the detour.

⁵⁶ Haavelmo's enthusiasm about the research atmosphere was certainly shared by Jaques J. Polak, one of Tinbergen's assistants: "Sharing an office with Tinbergen, I had the opportunity to absorb his method of work as if by osmosis. I learned more mathematics and even, I believe, more economics in that office than during my entire studies. The kind of work done in Geneva was at the very front line of economics and econometrics. Hardly a week passed by that we did not chance upon new and unexpected linkages – new statistical approximations, new variables that deserved a place in the model. Subjects suitable for journal papers were as easy to find as coloured eggs on Easter morning" (Polak, 1994, p.xiv).

⁵⁷ Tinbergen returned to Rotterdam in late 1938 and the business cycle studies was taken over for some months by Tjalling Koopmans. After the outbreak of the Second World War preparations started for moving the entire group to the United States. The move took place comprising the Director Alexander Loveday and about ten senior officials and their families in Aug-Sept. 1940 via Spain and Portugal to the Institute for Advanced Studies, Princeton, N.J. (Polak, 1994, p.xiv). Koopmans whose contract had expired left in June 1940, also went to Princeton as an assistant to Samuel Wilks (Scarf, 1992).

⁵⁸ Hans Staehle had published three articles in *Econometrica* on the issue and had recently estimated the propensity to consume for Germany in *Review of Economic Statistics* 1937. Haavelmo and Staehle would meet again in USA, and Haavelmo would later have a go at the propensity to consume too.

⁵⁹ Frisch/Haavelmo 1 March 1938 (transl. ob).

⁶⁰ Haavelmo sent the manuscript to Frisch at the end of February. Frisch glanced at it, wrote back immediately, promising comments later, expressing his belief it would attract attention, and indicating publication in the July issue (where it also appeared, prominently placed before Hotelling's famous taxation article in the same issue). MORE

⁶¹ Institut H. Poincaré was one of Frisch's strongholds in Paris, he had been there in the spring of 1933 to give

While he was in Paris Haavelmo completed a paper on “The seasonal movements considered as a periodic acting force” and sent it on to Frisch who found that the idea in the paper was a fruitful approach, but not corroborated enough for publication. The presentation was too “staccato”, but with a better grip on the numerical technique and applied to statistical data it might become a “beautiful” article in *Econometrica*. Frisch observed that there now was more of a “mathematical slant” in Haavelmo work, but he reminded nevertheless about Carthago.⁶²

After one month in Paris Haavelmo moved on to spend the last couple of weeks at the Institute of Statistics in Oxford. The Institute had been directed by Jacob Marschak since it was established in 1935.⁶³ He took part in colloquia organized by Marschak, who also invited him to give some lectures on confluence analysis. The modest amount of Nkr 2000 he had received for his European journey must have vanished long before he returned to Oslo on 19 May 1938.

Haavelmo spent the summer in Norway preparing to leave for Denmark before the autumn term started. Frisch recuperated in the mountains as he, indeed, did every summer, also when abroad, hiking in the daytime, working at night. He had been invited to the conference convened in Cambridge to discuss Tinbergen’s League of Nation’s work. There was no question of going to Cambridge in mid-July, but Frisch had brought the two volumes with him and intended to submit a memorandum to the conference. But, alas, time dragged out and the memorandum written at candle light at Eidsbugarden in the central Norwegian mountain massif was not sent off till the day the conference commenced.⁶⁴ Haavelmo presumably got a copy before he left for Denmark and although the ideas were not new to him, Frisch had introduced new terms not used in writing before, such as *autonomy*, that Haavelmo incorporated in his own conceptual and theoretical framework.

Teaching and pork econometrics: Aarhus 1938/39

While Haavelmo was in Oxford in May 1938 Frisch wrote to him to say that Jørgen Pedersen at the newly established Department of Economics of Aarhus University needed a statistics teacher for the forthcoming autumn term and Frisch had suggested that he ask Haavelmo. Frisch indicated that this might be a quite suitable assignment before he went abroad again. He was thus called to the position rather than applying for it. That is how it came about that Haavelmo in 1938/39 was a teacher of statistics in Aarhus.⁶⁵ Apart from the chairman professor Jørgen Pedersen the academic staff of the department comprised professor Erich

the first ever announced series of eight *econometric* lectures. Haavelmo also spent time with one of René Roy’s student of demand relationships, E. Morice.

⁶² Frisch/Haavelmo 5 May 1938, quotes transl. by ob.

⁶³ Jakob Marschak was born in the Ukraine. He studied in Vienna and was an active member in the Econometric Society since the beginning. Working in Heidelberg shortly before Hitler’s accession to power, Marschak contacted Frisch about moving to Norway. Frisch was positive about helping him to a position at the University, emphasizing the need to learn Norwegian. Marschak chose, however, England and attempted to make the Institute of Statistics a laboratory similar to that of Frisch.

⁶⁴ Dennis Robertson who organized the conference received the dispatch from Frisch three days after the conference ended: “Dear Professor Frisch, Your memorandum for the Tinbergen Conference has safely arrived here, but alas! too late for the conference, which dispersed on Wednesday evening. Making excuse that the envelope was hardly fit to stand another continental journey, I have taken the liberty to open it, and I thought it would save time for me to look at it now. It is, alas! far above my head; but I felt dimly that in the concluding pages you were expressing in scientific language the same kind of criticisms or warnings as some of us have felt impelled to lisp in crude and ignorant terms. I am sending on the memorandum to Tinbergen ... I thank you in the interim ... for taking so much trouble to write what is, I am sure, a most valuable commentary on the whole enterprise.” Robertson/Frisch, 23 July 1938.

⁶⁵ Haavelmo’s stay in Denmark has been discussed in Andersen & Kærgård (2000).

Schneider and professor Torkil Kristensen. As assistants worked Kjeld Philip, Jørgen Gelting and Orla Strange Petersen.⁶⁶

In Aarhus Haavelmo gave a course on statistical theory in the autumn of 1938, accompanied by mimeographed lecture notes.⁶⁷ Haavelmo was influential in choosing Davis & Nelson's textbook rather than the Danish alternative by Westergaard & Nybølle.⁶⁸ Haavelmo found the emphasis on philosophical foundations in Westergaard's book highly commendable and often missing from other textbooks, but as a textbook for economists in 1938 it was insufficient. Davis & Nelson on the other hand conveyed the impression that statistics was a "laboratory science", although it could be criticized for being too crowded with formulae and too scarce on the underlying philosophical aspects.⁶⁹

Shortly after the department was established Jørgen Pedersen initiated a series of empirical studies related to the Danish economy, foremost the agricultural sector. Pedersen had in the first of these studied the British butter market 1923-37. Soon after Haavelmo's arrival Pedersen suggested he conduct some empirical study of the Danish pig market. Haavelmo rose to the challenge and embarked on two projects, one on pig production in Denmark and one on the demand for pork. Both were completed in the spring of 1939.⁷⁰

Haavelmo seems to have started out with the demand study which due to lack of data was limited to the Copenhagen market. In the beginning of October 1939 he sent Frisch 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".⁷¹

The demand study was written in Danish and avoided technicalities. It had a discussion about how to make sure that the "functional covariation" represented a demand rather than a supply relationship. The confluence analysis figured prominently in the investigation, the key tool was "the modern form of regression analysis called 'Bunch Analysis'". Haavelmo declined to give estimates of standard deviations errors as such "are of doubtful value with short time series".⁷²

Before he left Denmark Haavelmo took part in the Third Nordic Meeting for Younger Economists with a paper on the statistical testing of hypotheses in economic theory.⁷³ The

⁶⁶ Schneider, whom Haavelmo knew from Oslo, may well have stimulated and influenced his interest in investment theory. Both Kristensen and Philip served as cabinet members in Denmark after the war, Kristensen was for many years Secretary General of OECD. Gelting's claim to fame is his discovery of the balanced budget multiplier, published in Danish while Haavelmo was in the U.S., but prior to Haavelmo (1945), hence a case of "who-influenced-whom", see Andersen & Kærgård (2000).

⁶⁷ Haavelmo (1939a). The sixth and last chapter in the lecture notes on "sampling problems" included a brief introduction to the testing statistical hypotheses, but without mentioning Neyman-Pearson.

⁶⁸ Davis & Nelson (1937), Westergaard & Nybølle (1928).

⁶⁹ H. T. Davis and W. F. C. Nelson were both associated with the Cowles Commission. Haavelmo's views were set out in a note he sent Frisch in January 1939 recommending Davis & Nelson to be used also in Oslo, in spite of some shortcomings Haavelmo and Reiersøl had found with the book. Also considered was F. C. Mills: *Statistical Methods Applied to Economics and Business*, New York: Henry Holt & Company, Revised Edition 1938. Frisch thanked for the viewpoints, reminding again about Carthago. Frisch/Haavelmo 30 Jan. 1939.

⁷⁰ Haavelmo's studies were no. 4 and 5 in the series, Haavelmo (1939b, 1939c). Pedersen's butter market analysis was, despite its being written in Danish, reviewed in *Econometrica* by one of Hotelling's pupils and received severe criticism for its econometric aspects, see I. Hilfer: Differential Effect in the Butter Market, *Econometrica* 6, 270-284.

⁷¹ The memorandum has been lost. Haavelmo/Frisch 10 Oct 1939, transl. ob.

⁷² The pig production study, written in English, foreshadowed in its detailed treatment of agricultural production Haavelmo's postwar work at the University of Chicago. Both studies are discussed in Andersen & Kærgård (2000).

⁷³ Haavelmo (1939d).

technical level of the presentation was elementary and the audience perhaps not exactly erudite in modern statistical theory, but Haavelmo aimed nevertheless at presenting an early version of his ideas of what a hypothesis in economic theory means and how it can be tested. The paper is important in an assessment of how much of the Probability Approach Haavelmo brought with him from Europe. He used results from the pork demand study to exemplify. He discussed inter alia the nature of economic hypotheses, simultaneity and the ‘ceteris paribus’ clause as a statistical problem and gave a sophisticated discussion of the trend elimination issue and the interpretation of regression results.

In the spring of 1939 Haavelmo got confirmed that he with Frisch’s help had been granted some means from the Norway-America Foundation and thus could plan a departure for USA. But the means would not suffice for more than a stay for some months. About the Rockefeller fellowship that Frisch had indicated he could help him with, Haavelmo drafted an application to Rockefeller, describing his research interest 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.⁷⁴

Haavelmo also indicated the places he wanted to go, adhering to Frisch’s advice. But the reaction from Rockefeller Foundation’s office in Paris which came very quickly, was rather cool. The application was too late, the study plan too vague and Haavelmo without a university position, did not fit into the Foundation’s institution building policy. Frisch rose to the occasion and did his utmost to convince the Foundation officials. He expressed his conviction that Haavelmo would have a future at the University of Oslo and gave him 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 a unusual degree the qualities of an economic theorist and a statistician. He is very energetic.⁷⁵

The outcome was that Haavelmo got the fellowship for 1940. It was the crucial decision for the rest of this story. Frisch also made an immediate effort to get Haavelmo a formal affiliation with the University of Oslo.

Cowles Commission Research Conference: Colorado Springs 1939 and 1940

Haavelmo left Norway for the United States in July 1939. He had planned a stay of 1-2 years, but did not return to Norway until February 1947. His first stop was in Colorado Springs, Colorado to attend the Fifth Cowles Commission Research Conference, 3-28 July, 1939.

Cowles Commission for Research in Economics was founded in 1932. Alfred Cowles who directed the Commission, had in 1931 visited Irving Fisher, the first President of The Econometric Society, and offered to bankroll a journal for the Society. He also offered to turn his consulting company into a foundation to support the aims of the Society, and that is what he subsequently did. The Cowles Commission was thus since its inception closely affiliated

⁷⁴ Haavelmo/Rockefeller Foundation, 15 April 1939.

⁷⁵ Frisch/Rockefeller Foundation, 25 May 1939.

with the Econometric Society, a bond which was strengthened by Cowles becoming Treasurer of the Society in 1932 and also Secretary from 1936.

The Advisory Council of the Commission appointed in 1932, comprised the first President of Econometric Society, Irving Fisher, as well as the Editor, Ragnar Frisch, and other prominent members, Arthur L. Bowley, University of London, Wesley C. Mitchell, NBER, and Carl Snyder, Federal Reserve Bank of New York. The Commission had its headquarters in Colorado Springs, with an affiliation with the Colorado College until September 1939 when it moved to Chicago and became closely affiliated with the University of Chicago.

Six Cowles Commission Research Conferences took place at Colorado Springs in the summers of 1935-40. The first conference was an unannounced and improvised event after the Econometric Society meeting in Colorado Springs in June 1935. For the second conference in July-August 1936 invitations had been sent out to the members of the Society and others and 50 persons attended. The third conference in June-July 1937 was even better prepared and held in conjunction with an Econometric Society Meeting in Denver.

The fourth conference was held in July 1938 to commemorate the hundredth anniversary of the publication of A.-A. Cournot's pioneering work in mathematical economics. The French Government sent René Roy as its official representative. The attendance was nearly 100. Frisch attended only one of the conferences, in 1937. Wald had come directly from Europe to take up his position as research fellow of the Commission and arrived just in time for the conference where he was scheduled to speak on indifference surfaces.

The fifth conference in July 1939 had an even higher attendance than the preceding ones. Among those present were Harold T. Davis, J. B. D. Derksen (Netherlands Bureau of Statistics), Mordecai Ezekiel (USDA), Abba Lerner, Jakob Marschak (Oxford), Horst Mendershausen (Colorado College), Charles F. Roos (Institute of Applied Econometrics), Gerhard Tintner (Iowa State College), Abraham Wald (Columbia University), Elmer J. Working (University of Illinois), Holbrook Working (Stanford University), and Theodore Yntema (University of Chicago).⁷⁶

Haavelmo presented a paper on statistical testing of dynamic systems.⁷⁷ It dealt with the "inversion problem" that Frisch, Haavelmo and Reiersøl had struggled with, how to derive the (damped) solution of the theoretical dynamic system from the observed shock-maintained series.

Abraham Wald's contribution at the conference was titled "The fitting of straight lines if both variables are subject to error." Wald showed that consistent estimates could be arrived at in the problem close to heart of the confluence analysis, although only two variables were dealt with. As Haavelmo expected, Frisch was enthusiastic when he was told about Wald's contribution, but not surprised. He had according to his own recollection recommended Wald to do exactly that. He also expressed his high expectations about Wald's further work.⁷⁸ And reminded about Carthago!

Haavelmo sent Frisch a letter on the last day of the conference. He was very satisfied with the outcome (although he could not suppress the impression that he found some of the participants too talkative). His own contribution had been well received. Of those present he

⁷⁶ Mendershausen and Wald had until shortly before the conference been research fellows of the Commission, Hotelling had been instrumental in offering Wald a position at Columbia where he eventually would succeed Hotelling. Yntema would take over as research director in September 1939 after the position had been vacant since Charles Roos left in January 1937.

⁷⁷ Haavelmo (1939e).

⁷⁸ Frisch/Haavelmo 17 Aug. 1939.

ranked Wald and Marschak on top. After the seminar he stayed on in Colorado Springs for a couple of weeks colloquizing further with Wald, Marschak, Tintner, Derksen and some others. He expressed to Frisch that he had already met practically all he wanted to meet in USA already. He decided that the next stop would be Berkeley to see Jerzy Neyman again.

The Fifth conference became the next to last one. Haavelmo and Wald attended also the Sixth Conference in July 1940. At that time the Commission had moved to Chicago, but the conference was still held in Colorado Springs. Among those who took part were again Davis, Mendershhausen, Wald and Yntema, and furthermore Oskar Lange, Wassily Leontief, Paul Samuelson and Holbrook Working. Haavelmo presented a paper on the problem of testing economic theories by means of passive observations.⁷⁹ Wald had by 1940 advanced deeply into in statistical theory and spoke on a new foundation of the method of maximum likelihood.⁸⁰ To Haavelmo's satisfaction Wald had succeeded in showing under quite general assumptions that all maximum-likelihood estimators tended towards a normal distribution. After the conference Haavelmo and Wald went hiking in Colorado.

Student and missionary: Berkeley, Chicago, New York and Harvard 1939-41

After the Cowles Commission Conference Haavelmo went to Berkeley at the end of August or early in September 1939. Neyman had made the big move across the Atlantic and settled in Berkeley in August 1938. Haavelmo stayed in Berkeley for around two months until the beginning of November. How much interaction he had with Neyman in this period is difficult to assess. There is no trace of Haavelmo in Constance Reid's biography of Neyman, written in cooperation with him about forty years later. One reason Neyman may have had other things on his mind than "converting Haavelmo to probability reasoning", if that carried any meaning to either of the two men, was the German attack on Poland 1 September 1939. Neyman was naturally highly concerned about his family and friends in Poland and the ruthless German extermination of Polish intellectuals. He started immediately to engage in activities to support his colleagues still in Poland and the refugees who arrived to the United States.

One of Neyman's collaborators in Berkeley, Francis Dresch, has recalled that Neyman gave a seminar in economics one autumn evening at Griffith C. Evans' home. Evans was chairman of the mathematics department, and among the others present was George Dantzig, head of the lab section and the mathematician Julia Robinson. Most likely Haavelmo must have been there too.⁸¹ Haavelmo also met with Harold Hotelling when he happened to pass through Berkeley.

Haavelmo gave a seminar at the end of his stay. From handwritten notes he has left behind it appears that he spoke with references to Koopmans' book and his main message in the seminar was summarized as follows:

- a) If the theory is a statement about the connection of observable variables, then, if no deviations are allowed, all theories will be deemed wrong.
- b) On the other hand, if the theorists have no preference for the different types of "deviations from theory" which may occur, one theory may be just as good as any other.
- c) Hence the problem of statistical testing is completely indeterminate unless the theory states explicitly what sort of errors are allowed to be in accordance with theory.

⁷⁹ Haavelmo (1940). Haavelmo was clearly working on sections of his forthcoming treatise.

⁸⁰ See Report of Sixth Annual Conference on Economics and Statistics at Colorado Springs, 33-35. Wald had started on the enormous outpouring of theoretical statistical papers that in the period 1939-44 resulted in more than 20 articles in *Annals of Mathematical Statistics*, 3 in *Econometrica* and several in other journals.

⁸¹ Dresch is quoted in Reid (1982), p.168, but the dating of the seminar is uncertain. Dresch states that Lawrence Klein also was present, which he could not have been in 1939.

- d) The errors allowed may be of different kinds. Suppose the errors are specified as certain given functions. Then again in most cases data will contradict theory.
- e) It may be that the theory states that the deviations have a maximum value, and that the variation within the allowed range is simply unknown. That of course does not mean that the errors are random.
- f) Theory [is] unsatisfactory as long as deviations are of such regular character that it seems possible to explain them by one or a few regular factors.
- g) Therefore it is more or less natural to consider a theory about the connection between observational variables as “good” only if the errors made by using the theory are irregular.
- h) Taking irregular deviations to mean random variation the problem of comparing theory with observations becomes a statistical problem. The problem of statistical testing of economic theory has no meaning unless the errors are specified as random variations, and it must also be specified how great they are allowed to be before the theory is rejected.⁸²

From Berkeley Haavelmo moved to the Cowles Commission in Chicago. On the way he stopped for one week in Ames, Iowa to give a seminar invited by Gerhard Tintner. The seminar on 9 November 1939 was on “Problems in the Statistical Testing of Economic Relations”.⁸³

When he arrived in Chicago he was persuaded to give some lectures on confluence analysis. As he reported to Frisch: “I have been quite used to being a missionary for Confluence Analysis”, but his missionary activities had barely started.⁸⁴ Haavelmo’s Rockefeller fellowship for 1940 was confirmed and on the way to his next location, New York, he attended the Econometric Society meeting in Philadelphia in December 1939. Before he left Chicago he gave Frisch a progress report on his fellowship research plan:

From early on I had thought that I should go hunting for one or another new technical method (new regression or equation), but I have found it inevitable to take up the entire question on a broader basis. I shall try, at least for myself, to write out a survey of the terrain.

I think one of the most important questions is the following: Economic theory has now come quite far with regard to the construction of rational systems and in drawing the right conclusions from strictly defined assumptions. But very little has been done with regard to building rational theories about the gap between theory and observations. This means that one, - as soon as there is a discrepancy between theory and data (i.e. practically always), - is completely at a loss as to whether a theory is “good” or “bad”, whether the deviations are “large” or “small”, etc. One can hold a personal opinion about this and one can reach valuable conclusions that way, but it is useless to convince others on that basis. (This is an argument frequently used here against Tinbergen’s work). There are on the other hand, many dubious aspects connected with the introduction of probability statements. Nevertheless, I think this is the only possible, rational way out. It is scarcely fruitful to do this in the way that one, when considering macro variables, adds more or less arbitrary assumptions about the nature of the random deviations. One does not then know which ‘fundamental probability set’ (Neyman) one deals with and the probability concepts are floating in the air. I have worked on a different way of formulating it which I think is promising in many cases: One can take into consideration each macro variable’s character of being an aggregate or average of individual actions, and then make some assumptions about random variations in the individuals’ behaviour. These assumptions can be done in a very little restrictive way when the purpose just is to deduce the nature of the random variations that will result about the average of a large group. In this way one can give a concrete interpretation of ‘errors’. If one does not start here, but just keeps the average (at every point in time) as one observation, one will lose the information inherent in each such observation being a sum (average) of a known number of

⁸² From handwritten note for seminar lecture 2 Nov. 1939 at Statistical Lab., University of California, Berkeley. Haavelmo Archive.

⁸³ A handwritten note for the seminar, Haavelmo Archive.

⁸⁴ Haavelmo/Frisch 17 Nov. 1939, transl. by ob.

random variables. The assumptions then made about random movements in the macro series, can be inconsistent with the concrete information one has about the possible variations in the single individuals' behaviour.⁸⁵

After the long statement Haavelmo added obediently, but jokingly, that he had, indeed, struggled to destroy Carthago!

In New York he wanted primarily to continue the contact with Abraham Wald. Apparently the two had liked each other very much from the first meeting in Colorado Springs and Haavelmo surely felt he had much to learn from Wald.

Haavelmo and Wald worked on the determination of coefficients in simultaneous structural relations. This was the "reducibility" problems in Frisch's 1938 memorandum. Haavelmo wrote to Frisch in March 1940 that Wald seemed quite certain that there were no theorems in the general mathematical literature that would be of any help. The problem was to ascertain the rank of matrices where the unknown coefficients themselves entered. The rank could thus be insufficient for "critical" values of the coefficients. It was, according to Haavelmo, "not always possible to give rules – in the form of a finite number of steps – wherewith one can verify if just these critical values of the coefficients are possible values with regard to a set of observations constrained by the simultaneously fulfilled relations."

Jakob Marschak seems to have had no intention of leaving England, but was in the U.S. on a Rockefeller fellowship at the outbreak of World War II and decided to stay when offered a position at the New School for Social Research. In New York he organized a seminar in mathematical economics and econometrics under the auspices of the National Bureau of Economic Research. Haavelmo took part when he had opportunity and would also present his ideas to this seminar. Koopmans also attended this seminar on a regular basis in 1940-41.

Haavelmo also reported to Frisch that he had continued to work on the idea he had set out in his letter from November 1939. He now nicknamed this idea "Distribution-Dynamics" and wanted to apply it in a dynamic analysis of consumption and saving behaviour.⁸⁶ He also worked on other theoretical ideas inspired by the discussions in Colorado Springs the previous summer.

From the autumn term 1940 Haavelmo was at Harvard, where he more or less remained until 1942. At Harvard he had frequent contact with J. A. Schumpeter who read much of his work and advised on publication. While at Harvard Haavelmo wrote a short review for Social Research of a new book by Gerhard Tintner on his "variate difference method" of analysing of economic time series. Thinking deeper about the issues involved he wrote a longer and more technical note which was published in *Econometrica*.⁸⁷ Haavelmo took issue with Tintner on an important point. Tintner assumed that a time series could be modelled as $w_i = m_i + x_i$, with m_i deterministic and the x_i 's independently and identically distributed, but Haavelmo found this unsatisfactory:

In modern economic dynamics a simple scheme of additive random elements, like the x 's above, takes a secondary place as compared with the schemes where the random elements form an integrating part of the fundamental system of dynamic relations. Random events, whether they be "from outside" or resulting from characteristic random spreads in the behavior of different individuals, firms, or groups, usually strike deep into the very structure of economic movements, they change velocities, accelerations, and so forth; they create new

⁸⁵ Haavelmo/Frisch 15 Nov. 1939, transl. by ob.

⁸⁶ Haavelmo/Frisch 27 March 1940, transl. by ob.

⁸⁷ Haavelmo (1941a), Tintner's book was *The Variate Difference Method*, Cowles Commission Monograph No. 5, Bloomington, Indiana, 1940.

initial conditions. Only in very particular schemes would the result be additive – independent random errors “pasted” on the top of some “true” smooth curve.⁸⁸

Haavelmo’s Rockefeller fellowship was extended for 1941, in view of the war situation. In 1941 Haavelmo got his first formal appointment at the University of Oslo. His long time as assistant for Frisch had not been a university appointment. Haavelmo now became “universitetsstipendiat”, a kind of research fellowship, comprising quite limited teaching duties, and would keep this position which had teaching duties that could be suspended until the end of the war.⁸⁹ Frisch sent congratulations when he in January 1941 found Haavelmo’s name in the official lecture catalogue, adding: “Study mathematics. It is more necessary now than ever.”

Haavelmo continued as missionary for Confluence Analysis. In the spring of 1941 he visited the University of Michigan, Ann Arbor. Arthur Smithies initiated the invitation indicating to Haavelmo that he had heard from others that “your missionary zeal will lead you to accept such a suggestion”. Haavelmo suggested a visit of “a few days”, but Smithies pressed on for Haavelmo to stay “as long as you can and at least long enough to do justice to your subject”. In the end they settled for one week during which Haavelmo offered to “take part in as many group meetings as you could arrange”. He lectured on confluence analysis each day of the week 18-22 February 1941.

He also gave seminars at Harvard and a mimeographed booklet titled “The Elements of Frisch’s Confluence Analysis” appeared at Harvard some time in 1941 with Haavelmo and Staehle as authors.⁹⁰ In the spring of 1941 Haavelmo told Frisch that he had refined a technique for presenting the idea and methods of Confluence Analysis and thus been better able to kill misunderstandings of what it was about. He set out to Frisch that even Koopmans who ought to know better than most what Confluence Analysis was, was confused on the issue when he lectured on regression.⁹¹ Haavelmo added that he had nearly completed a manuscript on “statistical verification of economic relations”. Schumpeter was reading it and Haavelmo intended to let Wald look at the more technical sections.

The manuscript was *On the Theory and Measurement of Economic Relations*, the early version of *Probability Approach*. Haavelmo seems to have worked on it and typewritten a large part of the manuscript in 1940.⁹² The preface is dated April 1941, but it was not completed and mimeographed until after hiking with Wald in Maine in August 1941. Haavelmo was very keen to have another summer hike with Wald. The two earlier summers they had spent weeks together in Colorado Springs. Those had clearly been very fruitful periods for Haavelmo. As he told Wald: “I have learned more during our previous summers together than during all the time I have spent elsewhere in this country”.⁹³

Haavelmo’s manuscript was mimeographed at Harvard at the end of August 1941 and distributed to a number of economists and statisticians. Haavelmo sent copies to Frisch and to Reiersøl through the postal channels still believed to be open, but none arrived. In a letter sent to Frisch, which got through, Haavelmo gave a brief summary of the main ideas:

⁸⁸ Haavelmo (1941a), p.75.

⁸⁹ It did not pay very much and Haavelmo received nothing until he came to Oslo.

⁹⁰ There probably was not much of Staehle in the presentation apart from the examples related to the demand for butter in the USA 1923-38.

⁹¹ Haavelmo gave details of Koopmans confusion. Haavelmo/Frisch (undated) from spring 1941.

⁹² A handwritten and two successive typewritten versions exist, Haavelmo Archive.

⁹³ Wald was more inclined to have another hike in Colorado Springs, and Haavelmo put persuasive pressure on him, arguing that “Maine has got everything that Colo. Spr. has, and it has got more ... to choose Colo. Spr. instead of Maine would definitely be to give up something for something inferior”! (Haavelmo/Wald 5 Aug. 1941).

The more I have worked on the question of statistical verification of economic laws, the question of structural vs. confluent relationships etc., the more convinced I have become about the necessity and fruitfulness of studying these problems from the viewpoint of probability and random variables. In the above mentioned treatise I have thus gone the whole step and tried to put all problems on a probabilistic basis. The problems of statistical verification, measurement of structural coefficients etc. can then be formulated such that they fall under the Neyman-Pearson scheme for testing statistical hypotheses. I have tried to give the general principles for such a formulation. From the beginning I thought that this would cover only a very special group of econometric research problems, and that other kinds of apparatus would be needed for a whole group of problems “where probability considerations do not apply”. But this way of looking at it I have come to abandon. There might of course arise problems for which another technique is better, but with regard to the problems that are currently discussed in econometric research, they can all, as far as I can see, be formulated – and that pleasantly precisely and also enormously generally – as questions about testing statistical hypotheses in the Neyman-Pearson sense. Something about this I try to set out and argue for in the Ms. I send you.⁹⁴

The letter got through, but the manuscript never reached Oslo. The war now interrupted further correspondence between Frisch and Haavelmo until May 1945.

Shortly after the Maine expedition Haavelmo wrote to Reiersøl, congratulating him on the article on confluence analysis in *Econometrica*, adding that others he knew would find it “damned interesting if you could follow it up with another article on the sampling theoretic problems that arise and which are of decisive importance in almost all practical cases”.⁹⁵ Haavelmo also reminded Reiersøl of their conversations about Neyman’s and Person’s disregard for Fisher’s maximum-likelihood method as having no foundation with regard to most powerful tests, shortest confidence intervals etc. Wald had now shown in a series of articles that the maximum-likelihood method could be solidly founded on the basis of Neyman-Pearson’s scheme and thus Fisher was right after all.

In Haavelmo’s last letter to Frisch during the war he also reflected on the wartime situation:

It can’t be denied that one asks oneself these days about the worthwhileness of such purely theoretical work, or one is asked by others. There are so many other more pertinent tasks one could do, and perhaps more appreciated for the moment. I have nevertheless, until now at least, tried to keep on with the work I have started, in case anyone would be interested when the world stands on its feet again. There are many now who think that this war and what comes afterwards, will bring the “practical problems” up front, do away with purely theoretical economic research, etc. I have rather the opposite view, that the time ahead will place greater demands on theoretical economic research than ever before. The collapse of various doctrines has nothing to do with the question of theory or no theory.⁹⁶

After the postal contact between Norway and U.S.A. was broken Oscar Lange was appointed Acting Editor of the *Econometrica*. He enlisted Haavelmo as referee.

Towards the end of 1941 Haavelmo was offered an appointment as lecturer in economics by the Dean Hans Simons at the Graduate Faculty of Political and Social Science, New School

⁹⁴ Haavelmo/Frisch 31 Aug. 1941 (transl. by ob). Frisch acknowledged receipt of Haavelmo’s letter in a postcard he sent back, and encouraged Haavelmo to submit the manuscript as a doctoral dissertation in the United States if he got an opportunity, even if “it would take away from me one of the pleasures I have looked forward to, namely, being your opponent in the university’s ‘gamle festsal’”. Then he added as his last words until 1945: “Keep on with the mathematics. And seriously. Without serious knowledge of functional theory and algebra you will experience disappointments. You will only be stuck half way where you ought to have come the entire distance and done really great” (Frisch/Haavelmo 23 Sept. 1941).

⁹⁵ Haavelmo/Reiersøl 8 Aug. 1941 (transl. by ob). The unnamed ‘others’ was Gerhard Tintner.

⁹⁶ Haavelmo/Frisch 31 Aug. 1941 (transl. by ob).

for Social Research. The offer was surely engineered by Jakob Marschak with whom Haavelmo had maintained close contact. Haavelmo indicated he might accept the offer, pending further negotiations with the Rockefeller Foundation on extension of his fellowship. Haavelmo's Rockefeller fellowship was extended for another half year into 1942, but Haavelmo turned it down and started to work for the Norwegian government in New York, or as he expressed it: "I have been drafted for civil service at the Norwegian Government's Shipping Ministry in New York". The nature of the "draft" is not quite clear, most likely Haavelmo felt an obligation to do war duty when this was suggested to him. A position in New York would also give excellent possibilities for continuing the cooperation with Marschak. He took part in the Econometric Society meeting in New York in December 1941 with a paper on Wicksell.⁹⁷

War activities: New York 1942-44

Haavelmo had been employed as statistician at Nortraship, which was a government administered management of the Norwegian merchant fleet. The work load was not very heavy and Haavelmo got involved in discussions of postwar preparations. Even though he had left academia for a while the period in New York gave relatively good opportunities for keeping in touch with fellow econometricians, Marschak in particular, and continue his research.

In February 1942 Haavelmo addressed Marschak's seminar at the Hillside office of the National Bureau of Economic Research. According to the written invitation sent out the seminar would address

... the validity of some current objections to the use of statistical inference in economics research. The main emphasis will be centered on the following questions, with an attempt to define them precisely and to clarify their content:

1. Is there any danger in considering economic data as stochastic variables? Is there any use in so doing?
2. Is it always objectionable to use free-hand methods of curve-fitting?
3. (a) What is the meaning of a "spurious result"?
(b) What are "economically meaningful" results as contrasted with "statistically significant"?
4. Is it more dangerous, in general, to draw inference from economic time series than from other kinds of statistical information?⁹⁸

Kenneth Arrow many years later wrote an eyewitness report:

I can still recall the meeting of the seminar where Haavelmo presented his new methods: Marschak in the chair, probing, questioning, and stimulating; Koopmans, even more ascetic-looking and soft-spoken than he is today, obviously understanding the issues better than anyone else; and Schumpeter, somehow esconced more comfortably than the rest and treating the whole matter with the benevolent condescension of a lord among well-meaning and deserving but necessarily limited peasants. Many of us knew that an important turning-point had been reached, and Marschak saw the need for effective leadership.⁹⁹

⁹⁷ Haavelmo (1942). Only 20 papers were presented at the meeting.

⁹⁸ Handwritten notes in the Haavelmo Archive. The invitation, signed by Sidney S. Alexander, gave no title, Haavelmo's notes were titled "The Nature and Logic of Econometric Inference".

⁹⁹ Arrow, K.: Jacob Marschak, *Challenge*, March-April 1978, 70-71.

During the spring of 1942 Haavelmo drafted the “Statistical Implications”-article. He completed it in June 1942 and sent it to Marschak for comments. It was published in *Econometrica*, No. 1, 1943.¹⁰⁰

In 1943 Haavelmo reworked the 1941 treatise. The revised version eventually became the Probability Approach. Haavelmo reshuffled the sections in the early part of the thesis and created a new chapter (III) on “Stochastical Schemes as Basis for Econometrics”. He also added a wholly new chapter (VI) on prediction and a short conclusion. The somewhat defensive statements in the conclusion reflected perhaps criticism he had met:

The patient reader, now at the end of our analysis, might well be left with the feeling that the approach we have outlined, although simple in point of principle, in most cases would involve a tremendous amount of work. He might remark sarcastically, that “it would take him a lifetime to obtain one single demand elasticity”. And he might be inclined to wonder: Is it worth while? Can we not get along, for practical purposes, by the usual short-cut methods

It would be arrogant and, indeed, unjustified to condemn all the short-cut methods and the practical guesswork which thousands of economists rely upon in their daily work as administrators or as advisers to those who run the economy. In fact, what we have attempted to show is that this kind of inference actually is based, implicitly and perhaps subconsciously, upon the same principles as those we have tried to describe with more precision in our analysis.¹⁰¹

The preface and acknowledgements were practically unchanged in the revised version, but clearly the impact of Abraham Wald had increased. Haavelmo referred in the acknowledgement part of the preface to recent work by Wald having given “a more explicit statistical treatment of problems that in the present study have only been mentioned or dealt with in general terms”¹⁰².

Haavelmo sent the revised version to Chicago.

Gottfried Haberler had taken upon himself to edit a special issue of Harvard’s *Review of Economic Statistics* to honour Schumpeter at sixty. He invited Haavelmo in October 1942 to contribute. Haavelmo answered yes right away, stating that had planned for some time to write a simple, non-mathematical exposition of problems in the statistical testing of business cycle theories. To some extent this would be an outgrowth of discussions he had had with Schumpeter while at Harvard.¹⁰³

Haavelmo also gave another presentation to Marschak’s seminar at the NBER in New York in December 1942 on “Problems of estimation and prediction in economic dynamics”.

In March 1943 Haavelmo gave a seminar at M.I.T. invited by Lawrence Klein. He gave it the title “Some Problems of Statistical Inference in Relation to Econometrics”. The opening paragraphs ran as follows:

The aim of econometrics, as you may read it on the cover of every issue of *Econometrica*, is “the Advancement of economic theory in its relation to Statistics and Mathematics”. That is, econometrics should be an attempt, not only towards more precision in the formulation of economic theories, but perhaps still more an attempt to reach such formulations that the theories lend themselves to testing against actual observations.

So far, however, it seems that most of the energy has been spent on constructing rational models, involving exact relationships that are much too rigid if we would try to identify the theoretical variables involved with some actually observable economic quantities.

¹⁰⁰ Haavelmo (1943a).

¹⁰¹ Haavelmo (1944, p.114).

¹⁰² Haavelmo (1944, p.vi). The reference was to Mann & Wald (1943).

¹⁰³ Haavelmo/Haberler 22 Oct. 1942. The contribution was Haavelmo (1943b).

The relation between such exact economic models and economic reality is, of course, in point of principle similar to the relation between rational mechanics and the bodies and motions observed in the real physical world. But the difference in the degree of precision is and is probably always going to be tremendous. In economics it is therefore not sufficient first to set up a system of exact relationships and then allow for certain small deviations in the applications to facts. We shall have to start out with a probabilistic formulation of our models from the beginning, otherwise we shall either have to call all our theories wrong or we may call almost any theory right by allowing for sufficiently large discrepancies (in a subjective manner).¹⁰⁴

Towards the end of 1943 news came from Norway that the University of Oslo had been closed by the Nazi authorities and Frisch imprisoned with a number of other professors and also several of the assistants and associates of the Institute.

From March 1944 Haavelmo was transferred to the Norwegian Legation in Washington D.C. with an assignment to work for the Royal Norwegian Purchasing Company which was an agency directly under the Norwegian government-in-exile in London. The work gave few opportunities to continue academic activities and lasted until after liberation. One of the few things he produced in this period was the balanced budget article.¹⁰⁵

Not long after Haavelmo moved to Washington the *Probability Approach* was published. An editorial note by the acting editor in the April issue announced that “[i]n view of the deficiency of material, it has been decided to publish, instead of the July issue, a special supplement containing an extensive monograph by Trygve Haavelmo, ‘The Probability Approach in Econometrics’”. A preliminary version of this was privately circulated by the author.”¹⁰⁶

An immediate impact of Haavelmo’s wartime contributions to econometrics was the role they came to play in the Cowles Commission’s research program. A conference was convened to discuss the adaptations of statistical tools to economic analysis based on the contributions by Haavelmo and others 28 January – 1 February 1945. Although the Cowles Commission announced already in the same year that the proceedings would be published as a monograph, and later reported the editing as completed by the end of 1946, it did not appear until 1950, the famous Cowles Commission Monograph No. 10 *Statistical Inference in Dynamic Economic Models*.¹⁰⁷ Haavelmo contributed the paper ‘Some remarks on Frisch’s ‘Confluence Analysis’ and its use in Econometrics’. The contribution differed from the “missionary” presentations of confluence analysis by being more of a critical review and had suggestions of how the confluence analysis techniques could be incorporated in a probabilistic framework.¹⁰⁸

Looking back at Haavelmo’s trail from Oslo to almost every other place where contributions to the breakthrough of modern econometrics took place, and back to Oslo again, one cannot avoid being struck by the impact that the World War II had on who were gathered where. Another, less visible, factor was Rockefeller money. Not only for the many fellowships,

¹⁰⁴ Handwritten note, Haavelmo Archive. L. R. Klein and P. A. Samuelson reminisced about Haavelmo’s and others contributions in this seminar series in *Statistical Science* 6, 320-338.

¹⁰⁵ Haavelmo 1945). He sent a draft to Marschak in October 1944, adding: “My contact with professional economists has been very poor now for a long time, so I am not sure whether my remarks in the enclosed notes are trivial or perhaps wrong, or, if they are not, whether the matter then has not already been dealt with more adequately by others”. Haavelmo/Marschak, 28 Oct. 1944.

¹⁰⁶ *Econometrica* 12, 142. The note also impressed upon the reader that the *Probability Approach* together with Mann & Wald (1943) “gives the foundations for a rigorous statistical testing of economic theory applied to time series and should be very helpful to those working in the field.”

¹⁰⁷ Koopmans (1950).

¹⁰⁸ The original notes for Haavelmo’s paper exist in the Haavelmo archive.

Rockefeller Foundation financed institutions and projects relevant to the breakthrough of modern econometrics in many countries. Along Haavelmo's trail it financed Frisch's confluence analysis work in Oslo, Tinbergen's group in Geneva, Haavelmo's fellowship, the agricultural project he worked on with Meyer Girschick in Chicago 1946, and, indeed, postwar work at Frisch's Institute. London School of Economics, National Bureau of Economic Research, Brookings Institution, University of Chicago and many others were recipients of Rockefeller Foundation grants whose contribution to the development of modern econometrics seems never to have been properly accounted for.¹⁰⁹

Aftermath: why did Haavelmo return to Norway?

Frisch was released from prison in the autumn 1944, but there were no regular channels of communication between USA and Norway until liberation in May, 1945. In July, 1945 Haavelmo wrote to Frisch and gave a very brief summary of his exploits since 1941. He mentioned the two 1943 articles and the *Probability Approach*, adding with characteristic modesty:

I must have been quite lucky with [the *Statistical Implications*-article] as the Cowles Commission with Marschak in charge, took up the methods suggested in the article and now has a special group working on further corroboration (especially Koopmans). I attended a conference about this work in Chicago this winter. Hotelling and others of 'the big boys' were also invited, and it was thus quite fun.¹¹⁰

Remembering Frisch's many admonitions he also added, "I am not quite sure how it stands with Carthago just now. A year or so ago I had the impression that I had begun to get a hole in the wall, but with my current work it might easily seal up again".¹¹¹

Frisch received shortly afterwards the *Statistical Implications*-article and the *Probability Approach*. He wrote back immediately: "I have not read them properly yet, only sniffed at them, but it was a highly pleasant air that came up. Your 'Conclusion' p. 114-115 is as written out from my own heart. Why don't you submit this work as a doctoral thesis in Oslo? The number of pages ought to suffice (Abel became, as is well known, world famous on 16 printed pages)."¹¹² Then he again reminded about the need for destroying Carthago, but that was for the last time.

Haavelmo again took Frisch's advice and submitted the *Probability Approach* as a doctoral dissertation soon afterwards. He expressed to Frisch and to many others his eagerness to get back to Norway. In the short run he was obliged to continue his work for the Royal Norwegian Purchasing Company. In October-November 1945 with no immediate prospects at home Haavelmo decided to accept an offer from Theodore Schultz, Chairman of the Department of Economics at the University of Chicago, to work on a project concerning agricultural demand, as soon as he could be released from government service.¹¹³

Haavelmo moved to Chicago from the beginning of 1946 and could thus also be resident as research associate at the Cowles Commission, to which he had been appointed by Marschak already in 1943. In the autumn of 1946 he spent a few weeks in Oslo to defend his doctoral dissertation. Shortly afterwards he got to know that a new professorate in economics would be

¹⁰⁹ See references in Bjerkholt (2000a), for the support to Tinbergen's group, see Neil de Marchi: "League of Nations Economists and the Ideal of Peaceful Change in the Decade of the 'Thirties'" in C. D. Goodwin (ed.): *Economics and National Security*, Durham, N.C. & London: Duke University Press, 143-178.

¹¹⁰ Haavelmo/Frisch 24 July 1945 (transl. by ob).

¹¹¹ Ibidem.

¹¹² Frisch/Haavelmo 10 Sept. 1945 (transl. by ob).

¹¹³ More details are given in Bjerkholt (2000b), particularly about various job offers Haavelmo got from Norway.

erected for him. Frisch, while attending a UN meeting, came to Chicago in January 1947, apparently to make sure that Haavelmo booked a ticket for the return to Norway. Haavelmo returned in February 1947.¹¹⁴

But why did he return? Haavelmo was almost alone in the group of European trained economists and statisticians who had come to the United States in great numbers in the 1930s and been offered positions in the U. S., to return to his home country. In the econometric community Oscar Lange and also Michal Kalecki returned, but apart from the two Poles, it is difficult to think of any others. Many of those who had crossed the Atlantic had, of course, little to return to.

Schultz and Cowles Commission may have made a mistake in not offering Haavelmo a tenured - and better paid – position, but it would hardly have made much difference. Haavelmo cannot have been much in doubt about his “market value”. Needless to say the return to Norway meant postwar austerity, a heavy teaching burden and few attractions. Haavelmo may have felt an obligation to contribute to postwar duties in his home country after having, against his will, spent the war years in the U.S. The only evidence of Haavelmo’s motives are on an anecdotal level:

A Norwegian economist friend and former colleague as assistant for Frisch, was visited by Haavelmo in New York on his way home in early 1947. The friend who worked at the United Nations and knew Haavelmo’s reputation very well, expressed astonishment that Haavelmo with such prospects in the U.S. would leave to become professor in Oslo some time in the future. Haavelmo answered simply that he liked trout fishing. The friend pointed out the excellent prospects for trout fishing in the U.S., only to be told: “Yes, but that is not Norwegian trout”.

Lawrence Klein whom Haavelmo had met in the U.S. and worked closely with at Chicago in 1946 came to Oslo to spend one year at the Institute in 1947/48. Through the winter that year he spent much time with Haavelmo and made the following observation:

One night in the Oslo countryside, we had been drinking a lot, and Trygve took me out to see a "picture-postcard" view in the moonlight with deep snow all round. He said, "Look at this scene and you can understand why I wanted to return".

Acknowledgements

This essay draws on correspondence and documents from the Frisch Correspondence Files at the National Library of Norway and from the Frisch Archive and Haavelmo Archive at the Department of Economics, University of Oslo.

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¹¹⁴ On the eve of his stay in U.S.A Haavelmo was offered several positions, e.g. by IMF and University of Wisconsin.

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