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FRISCH'S ECONOMETRIC LABORATORY AND THE RISE OF TRYGVE HAAVELMO'S PROBABILITY APPROACH

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The paper traces Trygve Haavelmo's training and early career as an econometrician from graduation in economics at the University of Oslo in 1933 until his departure for the United States in 1939. The overwhelming influence on Haavelmo in this period was Ragnar Frisch, whose econometric laboratory at the University of Oslo was Haavelmo's workplace and training ground. In the latter part of the period Haavelmo traveled in Europe, mostly within the network of econometricians Frisch had been instrumental in establishing. Haavelmo's work with Frisch, his interaction with other econometricians and statisticians, and his own scholarly work are set out in some detail, allowing assessment to be made of the development of Haavelmo's econometric ideas. Of particular interest is how far his ideas had evolved by 1939. This paper deals with Frisch and his research program in the early 1930s. Haavelmo's activities are narrated by and large chronologically. A sequel to this paper will deal with Haavelmo's scientific activities while in the United States from 1939 to 1944.

1. INTRODUCTION

This paper aims at providing an account of Trygve Haavelmo's training and experience as a budding econometrician from when he was hired at 21 years of age after graduation in 1933 as an assistant by Ragnar Frisch, research director of the newly established University Institute of Economics in Oslo, until

I have benefited greatly from extensive, generous, and very insightful advice—and prodding—from three anonymous referees on earlier drafts. I am also most grateful for advice and encouragement from the editor. To one of the anonymous referees I also owe the title. I absolve the referees and take full responsibility for all remaining errors and shortcomings. The paper draws on correspondence and documents from the Rockefeller Archive Center, Tarrytown, New York, and the Frisch Correspondence Files at the National Library of Norway and in addition from the Frisch and Haavelmo archives, currently at the Department of Economics, University of Oslo. I am most grateful to professor emeritus Tore S. Thonstad, who has done a great job of organizing the Frisch and Haavelmo archives. I have also benefited from the comprehensive Frisch bibliography prepared by professor emeritus Kåre N. Edvardsen. I thank J.J. Polak for reminiscences, Yngve Willassen for information, E.S. Jansen and J. Kiviet for a push to submit, and Hilde Bojer and Inger Bjerkodden for encouragement. Address correspondence to Olav Bjerkholt, University of Oslo/Statistics Norway; e-mail: olav.bjerkholt@econ.uio.no.

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he left Norway for the United States in June 1939. The interest in Haavelmo's early career is his contribution toward establishing a probability foundation for econometrics. Thus the emphasis is on experiences and events that may be presumed to have had an impact on the formation of Haavelmo's thinking en route toward his 1944 treatise or, more precisely, toward the 1941 version of the treatise.[1]

Frisch's institute was a laboratory for mathematical-statistical experiments and empirical studies. It was a workshop run perhaps more similarly to the other workshop Frisch managed, the family jeweler's business, than to a modern research and postgraduate facility. The relationship between Haavelmo and Frisch is of prime concern here. Haavelmo was Frisch's most promising student before the war, and it is not much of an exaggeration to state that Haavelmo was his *only* gifted student in that period. In describing the relationship between the two men it may be apt to denote Haavelmo as apprenticed in the master's workshop to become an econometrician, achieving journeyman status in 1936, marked by membership in the Econometric Society. By then he had become a most valuable co-worker to Frisch. As if adhering to an old European journeyman tradition Haavelmo sought opportunities for enhancing his education through *Wanderjahre*, which eventually left him stranded in the United States.

An assertion in this paper is that Haavelmo's pathbreaking 1944 treatise, most of which had been completed by 1941, to a large extent was rooted in his early work as Frisch's assistant and co-worker. Haavelmo became deeply involved in Frisch's original and energetic efforts at developing methods for confronting theory with data and at explaining business cycles. The experiences in Frisch's laboratory and the views of other practitioners whom Haavelmo met in the 1930s provided the background from which Haavelmo developed the ideas pursued in his later endeavor. He may be deemed to have become quite prepared for the probability approach by 1939. Also, other parts of the research agenda Haavelmo pursued later in life can be traced back to his work with Frisch.

It is generally recognized that Haavelmo's work owed much to Frisch's influence. It is less obvious whether, as Qin (1993, p. 19) states, Haavelmo was spurred on in his probability quest by Frisch's "antiprobability position," implying that the two men were at odds over the appropriateness of applying probability reasoning in economics. A close reading of the evidence may suggest that Frisch's "antiprobability position" may have been somewhat overstated.

The next three sections deal with the setting and scientific pursuits of the institute that was Haavelmo's environment as an apprentice. After some background on Frisch and the establishment of the Institute of Economics there follows an outline of Frisch's research program with analysis of time series/business cycles and econometric methods as key areas. Then Frisch's confluence analysis approach is dealt with. The ensuing four sections deal with Haavelmo's experience roughly in a chronological sequence, and the final section concludes.

2. FRISCH AND THE INSTITUTE OF ECONOMICS

The University of Oslo was at the time the only university in Norway. There were in 1930 two professors in economics but no department of economics. Economics belonged to the faculty of law. A separate economics study had been established in 1908 as a two-year study, primarily meant as a complementary study for law graduates and others. Economics study alone did not give access to higher positions in the government administration. Doctoral degrees were awarded but not many. From when the university was founded in 1811 until 1930 there had only been six or seven doctoral dissertations on economic topics and only one in statistics, namely, that of Frisch in 1926.

Until Ragnar Frisch appeared on the scene, Norway did not have economists and statisticians of international renown such as Sweden's Knut Wicksell and Gustav Cassel and Denmark's Harald Westergaard. There was a national tradition in economics; academic economists had played an important role in economic policy, in social issues, and in promoting statistics. It was a small but influential profession. The economics study around 1930 does not in retrospect seem to have attracted many talented students.[2] The early 1930s were the depth of the depression in Norway, and the university was enduring hard times.

Frisch, born in 1895, had graduated in economics in 1919 while being apprenticed to become a silversmith. After completing both educations he spent two to three years abroad studying mathematics and statistics in Paris, also visiting for study purposes other European countries. The object of his studies was, as he had told an acquaintance in 1926, to find "methods of submitting the laws of pure economic theory to a numerical verification by an intensive study of economic statistics."[3] In 1927 Frisch visited the United States for the first time on a Rockefeller fellowship. While in the United States he sought out practically everyone he could locate with similar interests, not finding very many. After his return he became an associate professor ("docent") at the University of Oslo in 1928.

Even after such preparations for a career in economics Frisch may, however, as late as 1929 have been in a dilemma as to whether it was possible for him to pursue his scientific interests in economics and statistics, in view of his responsibility toward his family, which meant taking over the family jeweler's business. A point of no return came when in the same year Irving Fisher instigated an invitation for Frisch to spend one year at Yale. The year and a half he spent in the United States in 1930–1931 gave him the opportunity to develop all his scientific ideas and prepare a comprehensive research program. At the University of Oslo the visit created the imminent fear that Frisch would accept an offer from Yale for a permanent position. While Frisch was in New Haven, the two professors in economics maneuvered shrewdly to have the Norwegian Storting (parliament) by an unusual act appoint Frisch as full professor at the University of Oslo, effective from July 1, 1931.

After his return to Norway in June 1931 Frisch negotiated with the Rockefeller Foundation about funding a research institute at the university as it had done in Denmark and Sweden. The Rockefeller Foundation was impressed with Frisch's plans and after a new round of negotiations in September the same year donated funds to support an empirical research institute.[4] The professorship and the prospect for resources for doing research in Norway sufficed for Frisch, who turned down Yale's offer and decided to stay in Norway. The University Institute of Economics was established from the beginning of 1932 with the Rockefeller grant as the only funding at the outset, with Frisch and Ingvar Wedervang as co-directors.[5] Frisch's part of the institute he referred to as his statistical or econometric laboratory.

Frisch had a lifelong passion for numerical calculations and was exceedingly adept at developing efficient algorithms and organizing large-scale numerical work. The availability of computational tools was limited; the most expensive equipment was beyond what Frisch could afford. But simple tools used by assistants adhering strictly to meticulous computing schemes masterminded by Frisch, usually with specially designed calculation sheets, went a long way toward satisfying the need. Frisch even invented some computation equipment and had it built to order. The process of numerical analysis and experimentation was a sine qua non in Frisch's laboratory; it was crucial for his most ambitious projects and often included stochastic elements, using random numbers from lotteries.

Frisch's position had since 1931 been that of a university professor. His main responsibilities were teaching and other academic duties. The institute was a separate responsibility, an institute at the university but not part of it. Somewhat separate from both university and institute was Frisch's active international role. He had played a key role in the establishment of the Econometric Society and in organizing the first two European meetings of the society in 1931 and 1932. He became the first editor of *Econometrica* in 1933. Of course, to Frisch his different roles were quite intertwined.

3. FRISCH'S RESEARCH PROGRAM FOR THE INSTITUTE OF ECONOMICS

The research program for the institute in the early years comprised problems and research areas rooted in Frisch's work in the 1920s, on which he had worked intensively while in the United States from 1930 to 1931. They were all *econometric* problems in the sense of the word Frisch had written into the constitution of the Econometric Society.

In 1926 Frisch had demonstrated how marginal utility, and more particularly the income elasticity of the marginal utility of income, could be "quantified" by making the theoretical assumptions precise by means of mathematical formulation and then confronting them with statistical data, adapted and interpreted by economic theory. In this work he expressed one of his key tenets in empirical analysis, namely, the primacy of theory, and demonstrated how that would lead the way for numerical estimation. At Yale he reworked his ideas and extended the range of applications of his approach to estimate marginal utility in the *New Methods* monograph.[6] This was his first internationally launched research theme.

A second theme was to establish an appropriate framework for data analysis as a foundation for econometric analysis. This was really Frisch's motivation and rationale for pursuing economics, inspired not least by the lack of rigor he found in others.[7] Frisch's theoretical work sometimes took its starting point from faults he found with others, but nowhere was this more true than with regard to uncritical use of regressions and partial correlations, which he vehemently attacked.[8] Frisch's own attempt at providing an appropriate foundation was to develop a geometric framework for studying linear dependencies in economic variables and the introduction of "diagonal regression."[9] Within this framework, which he elaborated during the 1927 visit to the United States, the importance of multicollinearity and of simultaneous equations came to the fore.

Frisch had also in the 1920s put much effort into the key research topic in economics at the time, the analysis of business cycles. He was familiar with Wesley Mitchell's work and had become acquainted with him during his visit in 1927. To Frisch the mechanical periodogram analysis of early researchers was much too rigid and simplistic. He first aimed at developing more flexible methods allowing for cycles of different lengths and had during his first stay in the United States written a methodological paper, which Wesley Mitchell showed interest in.[10]

By 1930 Frisch's ambition in business cycle analysis had advanced far beyond his 1927 paper, which he also recognized had shortcomings. He had been in touch since 1925 with Eugen Slutsky, who alerted him about his 1927 contribution as soon as it appeared. Slutsky also made Frisch aware of Yule's article the same year.[11] Slutsky had shown that mechanical smoothing processes applied to a random series might create more or less regular cycles. This result raised the question as to whether the search for cycles sometimes resulted in cycles being produced rather than identified. Frisch had, however, with brilliant insight recognized that if smoothing processes could have this effect on a series of random numbers, then random shocks somehow entering a macrodynamic system represented as a set of structural equations might likewise result in cycles of the variables in the model, with cycle lengths determined by the model structure. Wicksell's rocking-horse simile gave Frisch's idea an intuitive appeal.

To corroborate, explore, and extend the idea Frisch drew up a comprehensive research program. While he was in the United States in 1930–1931, he had engaged students at Yale and the University of Minnesota in numerical exercises, e.g., identifying cycles in constructed data sets with superimposed cycles and random disturbances. He also collected various long time series for experimental purposes and offered his time series methods for use in other fields. The institute gave him the chance to embark on the research program he envisioned.[12]

Yet another topic at the institute rooted in the 1920s and worked on at Yale was production theory, or "productivity theory" as Frisch called it at the time. Frisch had taught production theory for the first time in 1926, found flaws in earlier presentations, and decided to incorporate production theory as one of his econometric topics. That implied "quantification," i.e., mathematization of the theoretical concepts as the basis for confrontation with data. Frisch had attended the American Economic Association meeting at which Paul Douglas presented the first empirical application of the Cobb–Douglas function. Frisch's enthusiasm about the topic was mixed with dismay over shortcomings in the Cobb–Douglas article.[13] This may have spurred Frisch on in continuing his mathematization of production theory, which he did, aiming at publishing a monograph establishing it as another field of quantified theory.

As the Institute of Economics at the outset had only one source for its funds, the research program of the institute coincided with the research program agreed upon in the contract with the Rockefeller Foundation. Frisch and his colleague Wedervang had negotiated with John Van Sickle from the New York office of the Rockefeller Foundation and committed themselves to letting the institute serve "as a tool of advancing scientific economic research in Norway, as means of making the teaching of economics more effective, . . . [and] as a means of organizing cooperation between Norwegian industry and commerce and the economic research work at the University."[14] They furthermore committed themselves to letting the research "have a concrete character, based on thorough factual studies." This expressed an important goal for the social science grants of the foundation. Frisch may have felt some ambivalence; it was a worthy goal but perhaps too narrow. He put into the document the caveat that "factual studies alone can never lead to real understanding of economic phenomena. In our opinion accumulated observations get their full scientific usefulness only when they are interpreted in terms of a broad synthetic theory. Therefore theoretical investigations cannot be eliminated from a research work of the kind we are aiming at."[15]

Frisch had his research agenda already set. He coped with Rockefeller's requirements by emphasizing only the more appealing parts of his planned research in the proposal. The reports he gave about research done, included, however, all activities.

In 1936 Frisch reported on the work of the first four years of the institute using the following subdivisions:

I. Time Series and Business Cycle Analysis. Economic Dynamics.
II. Productivity Studies
III. Demand and Utility Analysis
IV. Statistical-Technical Studies for Developing Tools Necessary in the Above Analysis

These headings covered the portfolio of projects at the institute, including those financed from other sources. Projects under II and III had figured prominently in the original proposal submitted to Rockefeller, whereas I had been somewhat disguised as time series analysis of long series of price and wage data and IV not mentioned at all.

Frisch's high-flying methodological research ambitions, pertaining particularly to I and IV, were thus a bit camouflaged in his communication with the Rockefeller officials but not for long. John Van Sickle's and the Rockefeller Foundation's high enthusiasm at the outset, influenced by Schumpeter's praise of Frisch's talent, waned quickly. After one year Van Sickle found to his dismay that Frisch devoted himself to "highly abstract mathematical theory."[16]

Shortly after the institute was founded Frisch had hit upon the idea of national accounting and in 1934 asked the Rockefeller Foundation's Paris office for an additional grant to pursue national accounts for Norway it. It was a pity, perhaps, that he did not get it, as this could have become a pioneering effort of great usefulness and helped to advance macroeconomic research. But Frisch had already ruined his chances.[17]

In 1936 Van Sickle referred to what went on in Oslo as "abstruse mathematical theoretical work" in an internal memo on how to disengage from further support.[18] In the end the Rockefeller Foundation dealt gently with Frisch, who got a tapering grant until 1940, and also supported his research after World War II.

Of particular interest for the theme of this paper is Frisch's thinking at the time about the role of probability in econometrics. His two major research ideas to be pursued at the institute comprised on the one hand the impact of random shocks on a dynamic economic system and on the other how to identify (simultaneous) relationships among variables with errors of measurement. In both he may be said to have come near to considering probability theory at center stage. He expressed at the same time a very general view of the role of probability in economic relationships, as can be gauged from a letter Frisch wrote from Yale to Joseph Schumpeter at Harvard in December 1930 a few days before they would both meet in Cleveland at the foundation of the Econometric Society:[19]

> . . . 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.
>
> Let $x_1, x_2, \ldots x_n$ 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:
>
> $$\begin{aligned} &F_1(x_1, x_2, \ldots x_n) = 0 \\ (1) \qquad &F_2(x_1, x_2, \ldots x_n) = 0 \\ &\ldots\ldots\ldots\ldots\ldots\ldots \\ &F_n(x_1, x_2, \ldots x_n) = 0 \end{aligned}$$
>
> equal to the number of variables, thus making the system determinate.

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This involves, of course, the further assumption that the n relations considered really give a determinate solution for the n quantities $x_1, x_2, \ldots x_n$, 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 $F_1, F_2 \ldots$ will contain a number of constant parameters that characterize the shape of the function. We may indicate this explicitly by writing the functions,

$$
\begin{aligned}
&F_1(x_1, x_2, \ldots x_n, a_{11}, a_{12}, \ldots) = 0\\
&F_2(x_1, x_2, \ldots x_n, a_{21}, a_{22}, \ldots) = 0\\
&\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\\
&F_n(x_1, x_2, \ldots x_n, a_{n1}, a_{n2}, \ldots) = 0
\end{aligned}
\tag{2}
$$

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, $F_2 \ldots$ contained also another set of variables, $\xi_1, \xi_2, \ldots \xi_m$, m being at least equal to 1. Our set of structural relations the will take on the form

$$
\begin{aligned}
&F_1(x_1, x_2, \ldots x_n, a_{11}, a_{12}, \ldots, \xi_1, \xi_2, \ldots \xi_m) = 0\\
&F_2(x_1, x_2, \ldots x_n, a_{21}, a_{22}, \ldots, \xi_1, \xi_2, \ldots \xi_m) = 0\\
&\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\\
&F_n(x_1, x_2, \ldots x_n, a_{n1}, a_{n2}, \ldots, \xi_1, \xi_2, \ldots \xi_m) = 0
\end{aligned}
\tag{3}
$$

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

Now it is quite clear that the relation between the distribution Ω and the distribution w depends in a characteristic way on the magnitude of the parameters a_{ij}. Further, by expressing the fact that the x distribution which is deduced from the ξ distribution (that is to say, from the function Ω) is identical with the actually observed x distribution (that is, with the function w), we obtain a system of equations in a_{ij} which will furnish a solution of the set a_{ij} provided the equations in question are theoretically and practically solvable.

Thus in point of principle, the constant a_{ij} may be determined if we know Ω. Actually, of course, we do not know Ω but we may perhaps make some more or less plausible assumptions about it. To every such assumption corresponds a set a_{ij}. In an actual case, the formulae connecting w and Ω would give an exact expression for the effect of a change in the assumptions regarding Ω. Such a formula, it

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> seems to me that will be of some value as a check on the perfectly gratuitous assumptions which are sometimes made in this field.

Here Frisch thus viewed economic relations as having deeply embedded stochastic elements. The formulation allowed different interpretations as to the source of the stochastic variation. His very general formulation of the role of stochastic influences in economic relations seems in retrospect to have been an excellent starting point for elaborating a probability approach to the estimation of the parameters. It is almost, but not quite, something that can be imputed to Frisch on the basis of the last paragraph quoted here. Frisch did not choose that route, and that in itself is worthy of a question why. Instead he returned to the framework of his "Correlation and Scatter" essay and adapted it to become the confluence analysis cum bunch maps.

4. CONFLUENCE ANALYSIS

Frisch's *Confluence Analysis* aimed at dealing with regressions when there was more than one (linear) relationship connecting the variables under consideration. With no errors, either in equations or in observed variables, an attempt to determine regression coefficients when there were multiple relationships would give 0/0 expressions. When the relations or observations were contaminated by random errors, the same expressions would be determinate but really without meaning. To use a favorite expression of Frisch, one would have "fictitious determinateness created by random errors."[20]

Frisch assumed that the observed variables generally were measured with errors but did not include error terms in the equations. His analytic framework was computation intensive as the "complete tilling" of the data that he firmly recommended at the outset comprised finding the regression slopes of all subsets of the variables in all minimizing directions. Then his highly inventive graphical diagnostic tool, the "bunch map," would display the computation results in a way that revealed simultaneity problems caused by confluent relationships and multicollinearity and pave the way for the selection of regression equations to be included in a model.[21]

The confluence analysis was rooted in the cluster theory set out in the "Correlation and Scatter" essay.[22] This theory focused on the geometric structure of the set of points in the sample space using the matrix of correlation coefficients of the observed variables to classify different types of clustering, i.e., of deviations from a random scatter, as data analysis prior to doing regressions, not least to prevent the misinterpretation of regression and correlation results due to the neglect of confluent relationships.

The first long-term visitor at the new institute was Frederick V. Waugh, who stayed in Oslo most of the academic year 1932–1933, working closely with Frisch.[23] Waugh had been impressed with 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, including the use of bunch maps, in a study of the interconnection between the price and quality indicators of potatoes.[24] After Waugh Maurice H. Belz came from Australia on a Rockefeller fellowship. He came to learn and was at the institute in 1933–1934. Belz followed Frisch's statistical lectures and worked with him on a number of confluence analytic investigations.[25]

The *Confluence Analysis* book, rooted in Frisch's 1920s work and practical experiences during the visits of Waugh and Belz, was written over the course of three months in the spring of 1934. It was not premeditated as a monograph, and its content partly gives the impression of being a laboratory progress report.[26] The book had a mathematical superstructure, drawing on characteristic roots and polynomials of correlation determinants and a key empirical example about the relationship between potato price and seven quality variables. In addition it used a number of constructed data sets to demonstrate the usefulness of bunch map analysis. The book summarized research work achieved over about two years and was quite detailed about how the calculations ought to be organized. It reflected Frisch's high proficiency in relevant mathematics, making it difficult to read for many.[27]

Frisch tackled head on his abstention from probability considerations in his methodological approach, as if the issue had been discussed between him and others, which rather likely may have occurred. He argued that "sampling theory" (i.e., probability theory in inference) did not work when a regression equation included variables fulfilling two simultaneous equations. An attempted regression would have indeterminate parameters, but the standard formulas could very well give fictitious determinateness created by random errors.[28] No doubt Frisch regarded his effort at overcoming this roadblock for empirical analysis as a major contribution.

In the introduction to *Confluence Analysis* Frisch set out the nature of his investigation, referring to the weakness of the method in his earlier work as its lack of criteria for judging the significance of the "scatterances," as defined in "Correlation and Scatter":

> 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 suggests itself from the viewpoint 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)

He might well have had in mind here the "general set of assumptions" in the letter to Schumpeter. Instead of basing his approach on an explicit consideration of the probability aspect Frisch offered his method as an "attack on the problem more from the experimental side, working out numerically—on actual

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economic data as well as on constructed examples—various other types of criteria which intuitively and heuristically may suggest themselves. These experiments converged toward a definite method which after applications to various kinds of data was found to give satisfactory and plausible results" (pp. 7–8).

After these initial remarks he did not discuss probability further, except in a passage in the middle of the treatise where he defended his decision not to give a rigorous definition of probability by referring to the arbitrariness of choosing one specific probability scheme, adding, "I believe it will be a better application of time and energy to work experimentally with the method and rely on one's intuitive judgement of whether a given spread in the various determinations of a given regression coefficient is reasonable or not."[29] This was scarcely a very convincing justification, nor did it pretend to be. But the tentative formulation undermines somewhat the picture of Frisch's staunch antiprobability position.

The confluence analysis attracted, as the *New Methods* monograph had done, both students and scholars to Frisch as a generator of new and fruitful ideas, but his approach also met with resistance. His presentation was criticized for being too hard to penetrate and his methods for being too computation intensive and—more important—also for the lack of exact criteria. Frisch was not moved by the computation argument; it was only a question of organizing the calculations in an efficient way, and in this he was right.[30] On the much more important point of criteria, Frisch did not have much to say, but what he said in the final paragraph refrained from exaggerated claims (which he often was prone to) and summed up the main conclusion in the following passage:

> I do not claim that the technique developed in the present paper will, like a stone of the wise, solve all the problems of testing "significance" with which the economic statistician is confronted. No statistical technique, however refined, will ever be able to do such a thing. *The ultimate test of significance must consist in a network of conclusions and cross checks where theoretical economic considerations, intimate and realistic knowledge of the data and a refined statistical technique concur*. But I do claim that the technique here presented will in a great number of cases be very helpful. (p. 192, my emphasis)

This and other passages thus show Frisch shying away from adopting an overall probability framework but not as the outcome of a decidedly antiprobabilistic stand, more as a temporary position of pursuing one research idea at a time. Frisch's rationale was influenced on the one hand by the urgency he felt to provide tools that could be helpful and on the other hand by the conviction that there did not exist an all-encompassing probability framework as an operational tool for the problems Frisch had chosen to deal with.[31]

5. APPRENTICED IN THE ECONOMETRIC WORKSHOP, 1933–1936

Trygve Haavelmo, born in 1911, had begun his study of economics in 1930. He said in an interview after being awarded the Nobel Prize that his study incli-

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nations had really been toward engineering or languages. Economics was chosen because only such a short study could be afforded. Frisch was in the United States in the first year of his study but was present and teaching after that. Haavelmo graduated in June 1933 and was hired by Frisch as an assistant at the one-year-old institute, still in its establishing phase, and which, despite its name, was not part of the university.

Frisch had hired a number of assistants, students and young graduates, and acquired the computational equipment he could afford. He stretched the Rockefeller means by offering relatively low pay. The assistants were primarily "computers," and much of the work can surely be described as endless and tedious calculations. Perhaps the laboratory was run with some similarities to the silversmith's workshop that Frisch also managed.

Thus Haavelmo was thus hired to be a "computer." It may seem likely that Frisch had discovered Haavelmo's suitability for laboratory work before he graduated, as he had taken part in Frisch's statistical seminar, but he was not offered particularly favorable conditions.

Frisch was 38 years old and Haavelmo 21 when they entered into the master-apprentice relation in 1933. The distance between the two men in terms of social and cultural background, and also cumulated intellectual capital, was at the outset large and remained large throughout the 1930s. Frisch was from a well-established liberal-bourgeois background with a forefather who, according to the family tradition, at the king's invitation came to Norway from Germany as a mining specialist in the seventeenth century. Haavelmo had rural roots; his father had chosen a teachers' college rather than emigration to escape the impoverished overpopulated countryside.[32] Haavelmo was at the time by any measure a novice in econometrics, whereas Frisch may well be deemed as having been at the crowning peak of his scientific power, influence, and creativity. His *New Methods of Measuring Marginal Utility* was published in 1932; the *Confluence Analysis* would appear in 1934.[33] Early in 1933 he published the "Pitfalls" essay.[34] In March–April 1933 Frisch gave a series of eight lectures on the problems and methods of econometrics at the University of Paris.[35] Later in the year the "Propagation and Impulse" essay appeared.[36] The year 1933 was also the first year of *Econometrica* with Frisch as editor. Also in his fourth field, productivity studies, Frisch had something to show for himself, including the impressive but little-known rent article Frisch (1932b) and the mimeographed monograph, *Marginal and Limitational Productivity*, he had written in the United States (Frisch, 1930). The latter he had announced as a forthcoming book on several occasions, but for reasons not easily understandable it did not appear in English until 1965! In 1933 he furthermore, and unrelated to institute work, published "Monopole—Polypole—La notion de force dans l'économie" (Frisch, 1933c), which secured him a place in the history of game theory. Among other contributions in 1933 Frisch launched in the Norwegian economic-political debate the idea of national accounts.[37]

Haavelmo's work assignment in the first period at the institute, in addition to his tasks as Frisch's personal assistant and secretary, included calculations, much of them related to confluence analytic problems, which meant "tilling" of data and construction of bunch maps. Haavelmo was involved in the efforts undertaken in Frisch's work with Maurice Belz in 1933–1934. Confluence analytic computations thus became Haavelmo's first introduction to econometric problems. Haavelmo became extremely proficient in the kind of calculations that the bunch map analysis required. He was actually the first person to read the *Confluence Analysis* monograph from beginning to end, as Frisch passed the responsibility for proofreading on to Haavelmo as he rushed off to visit the Cowles Commission at Colorado Springs in July 1934.[38]

Haavelmo's apprenticeship with Frisch has left few documentary traces of Haavelmo's activities at the institute in the period 1933–1935. He did not publish anything, nor has there survived much in terms of internal documents with Haavelmo's name on them. Any assistance that he extended to Frisch with regard to his publications, such as, e.g., *Confluence Analysis*, was not acknowledged. Why was this so? Haavelmo's intellect at the age of 21 to 23 must have been as bright as it turned out to be later, although his fund of knowledge naturally was smaller at the outset. With an element of pure speculation, one might surmise that it was due to Haavelmo's time being fully absorbed in computing and studying or due to Frisch not fully having recognized Haavelmo's potential. A third reason could be a lack of a definite commitment on Haavelmo's part to become a laboratory-trained econometrician, as the institute neither offered career prospects nor much pay.[39]

The evidence on Haavelmo's motivation and commitment is that in the spring of 1935 he applied for a job as clerical assistant in the social security administration.[40] The requirements were secondary school and some office work practice. Higher education was not required. It was thus quite a low position, but yet it paid more than Frisch's "computer" jobs. Haavelmo was offered the job he applied for in July 1935 but then chose to decline the offer.[41]

Very shortly after Haavelmo got this offer Frisch redefined Haavelmo's position to become "chief computer" and doubled his pay. It could be tempting to read a game play into this outcome. Although Haavelmo is known to have enjoyed poker games with other assistants, this seems out of character. The pay raise (with a transfer of some administrative duties) perhaps reflected a (belated) recognition on Frisch's part that Haavelmo was as good an apprentice as he could ever hope for. Most important is the impression one has that from 1935 on Haavelmo's position relative to Frisch had moved at least a little in the direction of becoming more that of a co-worker rather than a lowly assistant.

Haavelmo's motivation and commitment did not seem to falter after 1935, but what made that year a point of no return for Haavelmo may well have been something else, such as the visit by Tjalling C. Koopmans. In March 1935 Koopmans wrote to Frisch, whom he had never met, and asked to come to stay for the autumn term. He indicated 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."[42]

Frisch gave an elaborate and somewhat critical answer to Koopmans's suggestions:

> 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.

Then Frisch's current concern with confluence analytic problems came to the forefront, as well as an outright invitation to Koopmans to become a student of confluence analysis:

> 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. . . . [H]ere 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. Maybe you would like to devote some energy to these kinds of questions.[43]

Thus it happened that Tjalling Koopmans spent the autumn of 1935 at the institute, where he gave a series of lectures under the title *On Modern Sampling Theory*.[44] More important to Koopmans was the underlying purpose for visiting Oslo, namely, to study whether Frisch's views could be incorporated in a more formalized probabilistic framework.

Frisch's skepticism toward probability reasoning as a fruitful approach in practical econometric work thus must have come under scrutiny during the visit. Further testimony on Frisch's view during Koopmans's stay can be gathered from a letter he sent to an American statistician who wanted to visit Oslo.[45] Frisch informed the statistician about the ongoing activities, with Koopmans's work described as an attempt to build a bridge between confluence analysis and Fisher's sampling approach, summarizing the main ideas discussed including his own view 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 have 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.[46]

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

Koopmans's doctoral dissertation thesis gave a thorough discussion of Frisch's confluence analytic approach.[47] He went a long way in accepting Frisch's views on economic data and his criticism of others, but he also pointed out shortcomings of confluence analysis. His incorporation of R.A. Fisher's theories made his thesis volume the first major work in econometrics that explicitly accepted probability theory, but as Qin (1993) points out, his probability message was fragmentary and rather technical.[48] Koopmans's explicitly stated aim was to combine the assumptions of Fisher and Frisch. He succeeded in that, but to put two bulls in the same pen does not necessarily promote procreation. Perhaps it could be said that Koopmans in his dissertation *adopted* probability theory in econometric estimation, in contrast to Haavelmo's all-out effort a few years later to *adapt* probability theory to econometrics.[49]

Koopmans's lectures were attended by Frisch, Haavelmo, and Reiersøl.[50] Koopmans's visit provided Haavelmo's introduction to the recently developed Neyman–Pearson theory.[51] Until 1935 Haavelmo's studies, when he was not toiling with computations, may have been primarily oriented toward conquering Frisch's scientific universe, which was large by any measure. Koopmans was the first visitor who did not come just to learn from and work with Frisch. To Haavelmo Koopmans's visit may have opened a window and ignited an urge to learn what there was to learn about probability theory and statistics.

One should perhaps also consider the effect that Koopmans's visit had upon Frisch's thinking. First, he was clearly very enthusiastic about Koopmans's lectures and insisted on having them mimeographed and distributed to various of his acquaintances.[52] Frisch continued to try to attract statisticians' attention to his confluence analysis, and to one of these he admitted in 1937 that the way he had tried to tackle the confluence problems was to a large extent based on intuition and that he had not reached as far as "to build a bridge between the confluence approach and the sampling approach."[53]

Frisch involved Haavelmo in his most ambitious and certainly most time and effort consuming project, namely, the study of time series and business cycles. It was the "economic dynamics" project, in the institute jargon just "shock theory." Frisch had completely reoriented his original time series analysis after studying Slutsky and Yule, and he utilized their ideas in his propagation-impulse cycle-generating model of the macroeconomy. The macrodynamic structural model of the economy worked according to Frisch's idea as a linear operator of the random disturbances to which it was exposed and produced cycles through the mechanism that Slutsky had described.[54]

Frisch's method in the economic dynamics project was mathematical analysis and numerical simulations, endless numerical simulations.[55] The work went on in bouts of high intensity from 1933 to 1937. Another person who was working at Frisch's institute from 1936 on was Olav Reiersøl, who had graduated in mathematics the previous year. Apart from these two few if any of Frisch's assistants would have been able to follow Frisch's heavy use of mathematics in this project.

Haavelmo and Reiersøl were thus Frisch's two key operators in trying out a large number of hypotheses and conducting enormously extensive calculations. To show that the Slutsky–Yule–Wicksell mechanism could produce cycles that simulated observed ones, as shown in the propagation-impulse model, was by itself an achievement but only the first step on the road. Frisch aimed at establishing a general theory for determining "the exact nature of the cycles which are created when a linear operator is applied to a random series" and professed on occasion to have found the general solution to that. Further along the road was the "inversion problem," i.e., to determine from a given time series produced by such a mechanism the weight curves by which the random disturbances have been accumulated.[56] This implied retrieving the macrodynamic mechanism from the shock-disturbed observations. Then one could also find the individual random disturbances themselves. Frisch described the method of attack as a "combination of theoretical analysis and the construction of numerical models," adding, "It goes without saying that a number of the ideas thus suggested have turned out to be valueless." Finally, at the far end came the methods for the ultimate goal, "structural forecasting." After solving the inversion problem an observed system could be forecasted on the assumption that future random disturbances would be zero.[57] Given the institute's equipment at the time these tasks called for enormous human efforts.[58]

In addition to working within Frisch's two main scientific paradigms, Haavelmo took part in a number of empirical studies falling under demand and production studies. He was also Frisch's teaching assistant, drafting and editing lecture notes. Parts of Frisch's lecturing were on his research frontier, such as *Macrodynamics* (1933–1934) and *Time Series Analysis* (1934–1935).[59] Haavelmo assisted in these and others of Frisch's lecture series, including *Statistical Theory* and *Monetary Theory*, in the years 1933–1936.[60]

In the autumn of 1935 Frisch would lecture again on monetary theory. From his visit to Cambridge in 1934 and contacts with Cambridge economists he was well aware that Keynes was working on a new theory. Motivated by a wish to give his students the most updated theory, he had the following brief letter exchange with Keynes:

> *Frisch to Keynes, September 18, 1935*
>
> "This semester I am lecturing on your monetary theory to the students in Oslo. I know of course your treatise on money in two volumes (1930). I also know that you have been working on a new book on the subject, but I do not know whether it has been published. . . . If you would care to suggest in a few words what you think are the essential features that I ought particularly to stress, I should appreciate it very much. I frequently find that a few words directly from the author may be more helpful in a matter like this than many days of careful scrutiny of printed material."
>
> *Keynes to Frisch, October 1, 1935*
>
> "My new book will be entitled *The General Theory of Employment, Interest and Money*, and has not yet been published. . . . I would very much rather, if it is possible, that you should wait until my new book is out before you inflict my opinions on your students. The new book makes a considerable difference, and I think they might lose their time if they were to go in any great detail into my previously published theory."

Keynes left Frisch with no choice. He decided not to heed Keynes's admonition and with Haavelmo as teaching assistant dissected the *Treatise of Money* once again.[61]

Frisch solicited financial support from Norwegian sources, offering statistical and economic analyses using the new tool of econometrics, partly for financial reasons but also, and perhaps more important, to prove to the Rockefeller Foundation and also to the Norwegian public and authorities the social usefulness of econometrics. Haavelmo was the key investigator in several of the empirical studies that Frisch contracted to do at the institute.

One study was undertaken in 1934 for the breweries' association, whose concern was that deflation under nominal taxation had caused a doubling of the real price of beer whereas the consumption was halved. The breweries wanted corroborated evidence to convince the government that lower taxes would increase tax revenues. The task seemed simple, a question of determining the price elasticity. Direct regressions gave highly uncertain estimates around -1 and revealed high multicollinearity. Frisch used his connections with the statistical bureau and borrowed the entire data files for the most recent household surveys, from which Haavelmo estimated a short- and long-term price elasticity of −1.74 and −1.55, respectively. But Frisch was still not satisfied; he produced a questionnaire and sent students out to interview acquaintances about their reactions to hypothetical changes in the price of beer.[62] Analysis of the polled answers gave an aggregate estimate equal to −1.65, corroborating the earlier result. The two future Nobel laureates sat together at the end of June 1936, drafting the report to the breweries' association.[63]

The report to the Rockefeller Foundation on findings resulting from a study of the demand for eggs in Oslo in 1935 may have been written by Frisch tongue in cheek to suit the foundation's request for "inductive research of a more realistic nature":

> The data were collected by one of our assistants, who paid a personal visit to a number of retail grocers in various parts of Oslo and asked for their cooperation. As a result monthly data on quantities of eggs sold and prices were made available over some years. . . . From the data collected we were able to construct rather definite demand curves for eggs. . . . [A] seasonal variation was found in the sense that the demand was smaller during the summer months, this effect being the most pronounced in the most well-to-do parts of the city. The effect is very likely due to people going away for the summer.[64]

On his appointment to "chief computer" in 1935 Haavelmo had bought a used Harley-Davidson motorbike, an unusual means of transportation among the institute employees. It saved time going to work, Haavelmo's home was about 20 kilometers from Oslo. He was fond then, as during the rest of his life, of the wilderness, as, indeed, was Frisch. But it was not really a common interest. Whereas Haavelmo's ideal pastime was fishing and pipe smoking miles away from other human beings, Frisch's outdoor activity was hiking and climbing mountains. Hours of solitude at a lake may have been when Haavelmo conceived his original ideas, whereas Frisch seemed to derive from hard physical exercise in mountain air his legendary ability to work on a problem for days on end. (Cross-country skiing was, however, an activity they both enjoyed into old age.) Haavelmo was highly regarded by the other staff at the institute in the supervisor position in which Frisch had placed him. All of them would surely have concurred in what Frisch wrote in introduction letters in 1937—that he was convinced Haavelmo "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."

6. PROBATION WORK COMPLETED, 1936

As apprenticed to become an econometrician, Haavelmo was in a very young profession! The mark of completed apprenticeship was, naturally, membership in the Econometric Society.[65] Frisch was practically the only Norwegian mem-

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ber of the Econometric Society who took part in the society's activities. His position as editor of *Econometrica* naturally gave Haavelmo good opportunities to follow the activities of the Econometric Society. Frisch had arranged for membership of several Norwegian members; they were by and large supporting members, rather than econometricians. Haavelmo had not been offered membership. Reasonably, Frisch would find Haavelmo suitable for being proposed for membership whenever he had something to present at an Econometric Society meeting.

Early in 1936 Haavelmo must have worked on the paper "Confluent Relations as Means of Connecting a Macrodynamic Subsystem with the Total System," to be presented at the sixth European meeting of the Econometric Society at Oxford in September 1936. Frisch and Haavelmo shared a cabin on the voyage across the North Sea on their way to Oxford. Frisch had been prominently present at all but one of the five previous meetings. Haavelmo had never been outside Scandinavia. The Oxford meeting was the largest meeting so far, with 64 participants. Among participants Haavelmo would have more contact with later in his career were only a few apart from Frisch: J. Marschak and J. Neyman, primarily the Geneva group of H. Mendershausen, H. Staehle, and J. Tinbergen. The meeting opened on Saturday, September 26.

Top billing on the agenda was the discussion of Keynes' *General Theory*, a topic of great interest to Haavelmo.[66] Saturday afternoon continued with Frisch's presentation "Macrodynamic Systems leading to Permanent Unemployment."[67] Frisch concluded, according to the report from the meeting, by stating that "the task was not so much to develop new systems as to test different systems against the facts." This was even more the concern of the next speaker, Jan Tinbergen, whose paper "Dynamic Equations Underlying Modern Trade Cycle Theories" dealt with the League of Nations project he had recently embarked upon. At a colloquium Saturday night Frisch presented an "ideal programme for macrodynamic studies."[68]

On Sunday morning R.G.D. Allen presented "The Assumptions of Linear Regression," a paper in a Frischian vein (even Frischian notation!) on the determination of limits for the true regression coefficient between two variables with measurement errors.[69] Afterward Jerzy Neyman presented a paper entitled "Survey of Recent Work on Correlation and Covariation," which naturally comprised a presentation of the Neyman–Pearson theory of testing hypotheses. By way of introduction Neyman compared the situation in economics with that in astronomy after Copernicus but before Newton. Kepler guessed the right formulas and adjusted the numerical coefficients. That was what was being done in economics according to Neyman. Newton's success was due to the existence of calculus! The success in economic dynamics likewise required new tools: stochastic calculus! Neyman's eloquent and imaginative presentation can hardly have left Haavelmo unaware of future tasks.[70]

On Monday morning it was Haavelmo's turn. The topic was chosen entirely within Frisch's paradigm and more precisely was meant to show the applicability of Frisch's macrodynamic modeling scheme. The problem Haavelmo posed was what to do when the structural equations representing the issue under consideration were a subsystem and mathematically underdetermined. Haavelmo discarded the alternatives of adding enough structural equations ("press more theory into the subsystem") and letting endogenous variables become exogenous, which meant they had to be specified rather arbitrarily as time functions. Instead he argued for adding sufficient "confluent relations" that fitted the data reasonably well. The underlying rationale was that the macrodynamic methodology called for eliminating all but one variable to get to the "final confluent relation" and only then to estimate. The numerical values of the estimates would reveal the dynamic properties of the model. In an empirical illustration Haavelmo also employed bunch maps.[71] 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 meaning and implication of *autonomy* but without introducing the term.

Before Frisch left England he spent an evening at Neyman's home. Frisch showed a great interest in the Neyman–Pearson theory of testing hypotheses and had drafted a note on it to make sure he had got it right.[72] He involved Neyman in a discussion of the theoretical part of confluence analysis, including its common points with Spearman's and Thurstone's work.

After the 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. In the report he submitted to the university after his return he stated the motivation for the study visit as his being "specially interested in the problem of using sampling theory in economic statistics." He followed lectures by Egon S. Pearson on general statistical theory and by Jerzy Neyman on the topic "Testing Statistical Hypotheses" and on orthogonal polynomials. In his report to the university Haavelmo also mentioned that the library facilities and document collections at the London School of Economics had given him the opportunity during the visit to go through the most important parts of the modern literature on statistics and probability theory, which presumably also included the first volume of Neyman's *Statistical Research Memoirs*, just published.

Haavelmo did some numerical work with Pearson, who also gave him referee assignments for *Biometrika*. He worked with Robert Jackson, a research worker at the department, with some participation also by Neyman on numerical tests for regression coefficients in confluence analytic problems. Jackson and Haavelmo planned a joint paper, but the only result from the stay seems to be a memorandum by Haavelmo entitled "Standard Errors on Regression Coefficients in Multivariate Sets."[73] While staying in London Haavelmo frequented at Friedrich Hayek's invitation his weekly seminar on business cycle problems at the London School of Economics.[74]

Shortly after Haavelmo's return Frisch proposed him as a member of the Econometric Society. The apprentice had come to a new stage; surely the meeting and the two months in London must have whetted Haavelmo's appetite for spending time with other econometricians, not least people such as Tinbergen and Marschak.

From 1937 on the institute had become involved in a government-financed project to explore Norway's production possibilities. Frisch had offered to participate and had gotten approved as part of the project, an attempt to develop national accounts, which ranked high on his own agenda. Haavelmo did not get very deeply involved but also had his share of the initial stage of that project.

Frisch's fourth group of research topics, productivity studies, had been relatively neglected. Frisch had demonstrated his ideas when he pioneered an engineering production function approach in a study of chocolate production in 1935. Estimation of production functions became, however, a topic in the laboratory in 1937. Oslo's biggest bakery commissioned a project to investigate the quality of its bread; the econometric analysis revealed that the dough needed more water![75]

The Cobb–Douglas production function also turned up on the agenda. When Douglas reminisced toward the end of his life about his 1928 article with Cobb, he mentioned "such critics of the production analysis as Horst Mendershausen and his mentor, Ragnar Frisch." Without reference he quoted the two critics as holding that Douglas's study had so few observations that any mathematical relationship was purely accidental and not causal and that all past work should be torn up and consigned to the wastepaper basket![76] One source for Douglas's remarks must have been the 1938 *Econometrica* article "On the Significance of Professor Douglas' Production Function" by Mendershausen, strikingly missing in Douglas's list of references. The article was written at the institute while Mendershausen visited in autumn 1937. He acknowledged suggestions and assistance from Frisch, Haavelmo, and Reiersøl. Haavelmo is likely to have been responsible for the bunch maps and perhaps also for the highly illuminative geometric drawing of the sample points, very much in the spirit of the "Correlation and Scatter" essay. The highly critical article reiterated Frisch's view that Douglas had neglected the multicollinearity in the data and the possibility of nonconstant returns to scale. The article concluded that the Cobb–Douglas coefficient found was merely "an expression of the trend in the technical development."[77]

A study of the demand for milk was published as a joint paper.[78] It was a government assignment that aimed at estimating price and income elasticities as functions of household income. It was a strenuous project using a number of different data sets on which Haavelmo toiled until the end of 1937. Also in that project the data sources were topped off by interview data, an inquiry among housewives. The overwhelming part of the joint work naturally fell on Haavelmo. It was his last work in the master's workshop.

7. JOURNEYMAN TRAVELS, 1937–1939

Haavelmo had expressed to Frisch his interest in a longer study visit abroad. Frisch advised a stay of at least two years. In the spring of 1937 Haavelmo was awarded a Norwegian grant for "further study of statistical theory and techniques" abroad. The amount was small, hardly sufficient for one year. Frisch hoped to get him a Rockefeller fellowship also.

While attending the Third Cowles Commission Research Conference in July 1937 Frisch gave some thought to Haavelmo's plan for studying abroad. His advice was to "read a considerable amount of mathematics before leaving Norway" and then to concentrate in the first part of the stay on mathematical statistics by going back to work with Neyman in London ("Stay there as long as you think is necessary in order to get a good foundation in sampling theory").[79] Frisch advised after that that he take up very thoroughly "the construction of demand and supply curves and similar investigations" by studying the rest of the first year with Jakob Marschak in Oxford. For the second year Frisch strongly advised going to the United States. He mentioned Louis H. Bean's work on demand and supply curves for agricultural commodities and furthermore advised Haavelmo to see Theodore Yntema, Harold Hotelling, and Charles F. Roos.[80] Finally, Haavelmo ought to get out to Colorado Springs and "see the work of the Cowles Commission." Haavelmo would adhere rather closely to Frisch's advice. By 1939, when Haavelmo finally crossed the Atlantic, both Neyman and Marschak had moved permanently to the United States as part of the rising flow of scientists that would make some of the American universities vastly more exciting places for econometrics than when Frisch made his suggestions.

In the autumn of 1937 Tinbergen visited the institute again. Tinbergen was learning to speak Norwegian; he had a gift for languages and also an interest in being updated with work notes on the progress in the laboratory. The topic for discussion would naturally be Tinbergen's project at the League of Nations, which had progressed much since Oxford. Tinbergen was the world's foremost macroeconomic model builder (in fact, he was still practically the only one) and used bunch maps intensively in the investigation.[81] Learning that Haavelmo planned to travel in Europe he extended an invitation to visit him in Geneva.[82]

A pertinent question with regard to Tinbergen's work, which prior to the League of Nations project had comprised a model of the Dutch economy, is why it did not ignite any interest in model building from Frisch's side. The macrodynamic model figured, indeed, prominently in Frisch's "ideal programme" as presented in Oxford, but it seemed as if Frisch was not interested in the model for its own sake but only in the transmission mechanism that explained the properties of data generated from the economy represented as a macrodynamic system of equations and random shocks. Even the propagation and impulse model, often celebrated as the first macro model, can be read as if Frisch's point is really only the exemplification it gives of the transmission mechanism.[83]

Although the Rockefeller fellowship and a visit to the United States still were pie in the sky, Haavelmo intended to use his Norwegian grant to visit several institutions in Europe. Tinbergen in Geneva was the prime target. He did not heed Frisch's advice about going back to Neyman, but he included a visit to Marschak in Oxford in his plan.[84] The busy schedule at the institute had put off the departure until the beginning of December 1937.

Haavelmo's first stop was Berlin, where he spent a month and a half (including Christmas and New Year's) as a visitor at the well-known Institut für Konjunkturforschung, directed by Ernst Wagemann. Part of the purpose of the Berlin visit seems to have been to study the equipment and report back to Frisch. Haavelmo spent much of the time in Berlin at the Meteorologisches Institut's department for *Periodenforschung* (i.e., time series analysis), directed by Professor Karl Stumpff with a range of advanced equipment for harmonic analysis. Haavelmo was given access to Stumpff's equipment and tried it out on data already analyzed in Oslo by Frisch's methods to compare the efficiency.[85]

Stumpff's methods for harmonic analysis were described in a note by Haavelmo.[86] The harmonic analyzers were based on light interference. The results came out as photographs and punched cards that 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 Frisch his report, concluding that the methods were useful as they required little human effort, 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.[87] From a scientific point of view the Berlin visit was hardly of great interest, Haavelmo had shifted his focus away from computing equipment and toward the real econometric challenges. He fulfilled his duty for Frisch and perhaps enjoyed Berlin also.[88]

From Berlin Haavelmo traveled to Geneva in mid-January 1938, primarily to work with Tinbergen at the League of Nations' Financial Section.[89] Geneva was in the mid-1930s a beehive of economists. At the league also worked James Meade, Marcus Fleming, and Ragnar Nurkse. J.J. Polak worked as Tinbergen's assistant. Hans Staehle worked at the ILO. At the Institut des Hautes Etudes, supported by the Rockefeller Foundation, were Lionel Robbins, Abraham Wald, and Horst Mendershausen.[90] Wald had from September to December 1937 worked with Tinbergen in establishing a system of equations reflecting the chief forces acting in business cycles before he went back to his position at the Institut für Konjunkturforschung in Vienna. Thus Haavelmo narrowly missed meeting Wald in Europe.[91]

Haavelmo found Geneva a great place to continue his studies. He was taken care of by Tinbergen, who gave him a thorough introduction to the ongoing work and arranged office space for him with access to the financial section's computer equipment. Meetings were held in Tinbergen's office facing south toward the park of the Palais des Nations, the lake, and on clear days the majesty of Mont Blanc. Haavelmo conveyed the impression that there could hardly be a more ideal place for him to visit, and clearly he regarded Geneva at the time as the leading center in modern economic research.[92]

He was certainly right about that; no other place in the world had a project that could be of more interest to an econometrician just then than Tinbergen's League of Nations project.[93] Haavelmo already had learned much about the project from Tinbergen in Oslo half a year earlier, but the visit gave him a firsthand opportunity to get an inside look. Tinbergen applied bunch mapping intensively in his project (and for that reason may have appreciated having a bunch map expert around), but he also used a range of other techniques in his corroboration of model specifications and estimations.[94]

Being an observer and a participant in this landmark of an econometric project must have left an indelible impression upon Haavelmo. It was also great timing; Tinbergen was close to completion of his first league volume when Haavelmo arrived and was working hard on the second volume in preparation for the special conference to be convened in Cambridge in July 1938 to discuss the results. Tinbergen brought Haavelmo into a little informal group of six or seven "econometricians" he had gathered in Geneva and met with almost daily. In a letter home to Frisch Haavelmo could report that confluence analysis had gained firm ground in Geneva and terms such as *bunch map* and *multicollinearity* were used in daily communication without explanation. While he was in Geneva Haavelmo also found time to review for *Weltwirtschaftliches Archiv* Tinbergen's 1936 publication and Koopmans's dissertation.[95]

Frisch had surely encouraged Haavelmo to submit the Oxford paper to *Econometrica* after revision. A year and a half after the Oxford meeting it was still not completed. Haavelmo had brought it along to Geneva, and from there he submitted the finished paper to Frisch, acknowledging valuable suggestions from Tinbergen.[96] Why did it take so long? Haavelmo was usually fast and efficient in most things. Perhaps the completion of the paper had caused problems it took time to sort out. Shortly after his return from London, Haavelmo had written back to Pearson to express his thanks, mentioning that he worked on using sampling theory on certain problems in the analysis of business cycles generated by erratic shocks, referring perhaps to the same problems. In the published version of Haavelmo's paper one of the two numerical examples, using U.S. stock market data from 1903 to 1914, was a model that was solved to have dynamics given as damped exponentials. The simulation nevertheless generated cycles, as if a Wicksellian rocking-horse mechanism was not needed. Haavelmo attempted to resolve the puzzle, which may have delayed the article, by the assumption that all the coefficients were stochastic, causing the observed cycles. Although Haavelmo scored his point by showing a formal similarity between the effects of stochastic coefficients and random shocks, the solution may in retrospect seem to have been ad hoc and analytically awkward.[97]

From April 1, 1938, Haavelmo was in Paris, apparently without a very specific purpose. Frisch had put him in touch with his old friend François Divisia and also sent with him introduction letters to the mathematician/physicist Phillippe Le Corbeiller and the statistician Georges Darmois. Haavelmo visited the statistical department of Institut H. Poincaré in Paris, where he had the opportunity to follow at close range a major study of production efficiency in French manufacturing. While in Paris Haavelmo wrote a note entitled "The Seasonal Movements Considered as a Periodic Acting Force." In mathematical terms the idea was to start from a general dynamic equation of a system mobile around an axis with a stable equilibrium and add seasonal movements as a linear function of the state of the system. The economic inspiration was from having noted that the seasonal amplitudes of unemployment in Norway varied inversely with the cycle.[98] The problem led to a differential equation. Philippe Le Corbeiller showed interest but advised Haavelmo that he was getting into quite difficult mathematical territory.[99] Was it a whim or a well-considered idea? It took off from a physical analogy, like much of Frisch's modeling.

After one month in Paris Haavelmo spent the last couple of weeks of his trip abroad at the Institute of Statistics in Oxford, which had been directed by Jakob Marschak since it was established in 1935. He took part in colloquia organized by Marschak, who also invited him to lecture on confluence analysis. At the time Haavelmo may have considered Marschak primarily a demand and supply analyst. There is no further information about the interaction with Marschak. In retrospect, it could be viewed as an important meeting. Marschak was still only one-third into his 60-year-long remarkable career. The most colorful and dramatic events of his life were behind him. He was certainly one of the brightest and most inventive and enterprising persons in the entire econometric circuit. Frisch had been in close contact with Marschak since 1932 and valued him very highly as reflected in his advice to Haavelmo to spend half a year rather than two weeks with him.[100] Why did Haavelmo waste time in Berlin and Paris and spend so little time in Oxford? Haavelmo and Marschak would meet again at Colorado Springs little more than a year later and would have much contact throughout the ensuing years.

While Haavelmo was in Oxford in May 1938 Frisch was asked by the head of the newly established Department of Economics of Aarhus University to find a statistics teacher for the coming academic year. Without thinking twice Frisch offered Haavelmo, who was thus called to a position as a teacher of statistics in Aarhus and accepted without hesitation when he learned about it. We can hardly interpret this otherwise than that Frisch found it a good idea for Haavelmo to be away from Oslo for awhile. Haavelmo might even have expressed an interest in teaching statistics; in London he had made a point of studying the teaching programs in statistics. A sojourn at Aarhus University can hardly have been rated by Frisch as of much interest in itself. It gave Frisch more time to secure

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Rockefeller support for Haavelmo's visit to the United States, which he surely regarded as a necessity to complete Haavelmo's education.

In the summer of 1938 Frisch recuperated as usual in the mountains, hiking in the daytime, working at night. He had been invited by Alexander Loveday at the League of Nations to take part in the conference convened in Cambridge to discuss Tinbergen's work.[101] There was no question of going to Cambridge in mid-July, but Frisch intended to submit a paper to the conference. But, alas, time dragged on, and the memorandum written by candlelight at Eidsbugaren in the central Norwegian mountain massif was not sent off until the day the conference commenced.[102] Haavelmo spent the summer in Norway preparing to leave for Denmark in good time before the autumn term started.[103] The next summer spent in Norway would be nine years later.

Aarhus provided a nice break for Haavelmo. The teaching burden was light and gave him ample time for reflecting on the econometric problems he had struggled with for five years. The break came at a convenient time. He had behind him four years of experience with Frisch in the laboratory, the short autumn term with Neyman in London, and his recent Berlin–Geneva–Paris–Oxford tour. He had with him Frisch's 1938 memorandum for the Cambridge conference, which hardly contained anything that was new to Haavelmo. (Did he have the proof version of Tinbergen's two volumes too?) There still was a chance for a one-year visit to the United States. Then time would tell.

At the Department of Economics in Aarhus there were two members of the Econometric Society, the chairman Professor Jørgen Pedersen and Professor Erich Schneider, who was German.[104] Haavelmo gave a course on statistical theory in the autumn of 1938, accompanied by mimeographed lecture notes.[105] He was instrumental in choosing Davis and Nelson's textbook rather than the revised edition of Westergaard's 1890 textbook. Haavelmo found the emphasis on philosophical foundations in Westergaard's book commendable and often missing from other textbooks, but as a textbook for economists in 1938 it was insufficient. The Davis and Nelson work was praised for conveying the impression that statistics was a "laboratory science," emphasizing mathematical processing of data and comprehensive computations, but Haavelmo still criticized it for being too crowded with formulas and too scarce on the underlying philosophical aspects. Even this offhand remark in a note to Frisch may be read as an indication of the shift in Haavelmo's concern from algorithms to philosophy as what was in short supply for econometric progress.[106]

Jørgen Pedersen had initiated a series of empirical studies related to Denmark's most important industry, agriculture, and invited Haavelmo to contribute. Haavelmo rose to the challenge and embarked on two econometric studies, one on pig production in Denmark and one on the demand for pork, both completed in the spring of 1939.[107] By then Haavelmo had great experience for such tasks. On the study of demand for pork in Copenhagen he drafted in October 1939 a memorandum outlining his approach: first, to build a theory for the investigation and then to "statistically verify" the relationships rather than just

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choosing "a mechanical procedure that fits the market data," perhaps written for didactic purposes. The demand study was written in Danish and avoided technicalities. The key tool was "the modern form of regression analysis called 'Bunch Analysis.'" Haavelmo adhered to Frisch's maxims by declining to give estimates of standard deviations errors, as such "are of doubtful value with short time series."

The pig production paper was not so much about pig production as it was econometrics of regulation (for the first time?). Haavelmo apparently learned a lot about pigs in Denmark. He had determined the structure of pig production, summarized in one of the relations as the outflow of finished pigs per month as a lagged function (by 8.18 months!) of a linear combination of the stocks of first-time-breeding sows and other breeding sows. But what made the paper really interesting derived from the quota regulation, which implied a two-price system for pigs brought to the slaughterhouse, quota price and without-quota price. The quotas were transferable among farmers at market price. Haavelmo was on the frontier in discussing the interaction between production lags and the effects of the regulation with regard to how "shockproof" the system was, concluding that it was indeed not very shockproof. Shocks in the quota price affected the price of small pigs, resulting in shocks reverberating through the production system.[108]

8. GETTING READY FOR COLORADO SPRINGS

In the spring of 1939 Haavelmo received confirmation that he had been granted some means from the Norway-America Foundation and thus could plan a departure for the United States. The means would not suffice, however, for more than a few months. At Frisch's insistence Haavelmo drafted an application to the Rockefeller Foundation in which he described 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 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.[109]

Little can be read into the quite generally formulated first sentence with regard to how Haavelmo's thinking had progressed on the issue he had worked on since 1933. The second topic reflected his struggle with the problem that arose from his Oxford paper. The third topic was inspired by Keynes and was an attempt to develop a microeconomic underpinning of the consumption function.

The reaction from the Rockefeller Foundation's Paris office was rather cool. The application was too late; the study plan too vague; and, worst of all, Haavelmo, without a university position, did not fit into the foundation's institution-building policy. Frisch had good standing with the Paris office. He had been consultant to the Paris office on a number of European applicants for

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Rockefeller fellowships. He now rose to the occasion and did his utmost to convince the foundation officials, expressing his conviction that Haavelmo would have a future at the University of Oslo.[110]

The outcome was that Haavelmo got a fellowship for one year beginning in 1940. That would allow a total stay in the United States of a year and a half. Haavelmo had no wish to stay any longer. Relatively soon he would also obtain a formal affiliation with the University of Oslo.[111]

The topic for the paper Haavelmo prepared for his presentation at the Fifth Cowles Commission Research Conference in 1939, immediately after his arrival in the United States, was nothing less than the "inversion problem." Haavelmo dealt with "shock cumulants," i.e., observations generated by a dynamic model and contaminated by erratic shocks. Classical regression methods would then not give unbiased estimates of structural coefficients. As in his 1938 paper Haavelmo asserted the equivalence between erratic shocks and stochastic coefficients. It was thus a topic chosen from within Frisch's research agenda, but it also reflected the increasing interest Haavelmo had taken in the confrontation between observations and theory.[112]

Before leaving Denmark, Haavelmo took part in the Third Nordic Meeting for Younger Economists in May 1939 in Copenhagen, after being asked by colleagues in Oslo to contribute on behalf of the Norwegian association. He chose to present a paper, "On the Statistical Testing of Hypotheses in Economic Theory." The technical level of the presentation was quite elementary, and the audience was perhaps not exactly erudite in modern statistical theory. Haavelmo aimed nevertheless at presenting a highly sophisticated lecture on verification in economics. The section headings of the paper, which was only 18 pages long, were as follows:[113]

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

It was the experienced researcher who presented the modern theory of verification in economics. The opening section set the tone:

> Anyone who has worked in economic theory knows how it often is the case that several different "correct" theories can be put forward to explain the same phenomenon. The differences are in the choice of assumptions. One comes all the time to crossroads where one direction *a priori* seems as plausible as another. To avoid it all becoming just a logical game, one must at each step have these ques-

> tions clearly in view: Are there realistic elements in my reasoning, or do I operate in a one hundred percent model world? . . . It is here that the requirement of statistical verification comes to the rescue, prevents the reasoning from running astray and forces a sharp and precise formulation of the hypotheses. The statistical corroboration saves us from many empty theories at the same time as it gives the hypotheses verified by data so much more theoretical and practical value.

He moved on to assert the statistical nature of the hypotheses of economic theory, emphasizing that testing was not an easy task:

> The circuit of problems relating to the testing of hypotheses is not exhausted by the question of the *degree of precision* in the agreement between data and a certain hypothesis. The key problems in the hypothesis testing actually lie prior to that stage in the analysis. It turns out—as we shall see—that many hypotheses cannot at all be verified by data, even if they are quantitatively well defined and realistic enough. Yes, we can be led astray if we try a direct quantification.

He dealt briefly with the principles of statistical testing, not even mentioning Neyman–Pearson, and explained "free" and "system bound," using formulations similar to those he would later use in the "Autonomy" chapter of the *Probability Approach*, forewarning that "[t]his is precisely one of the main reasons why refined techniques must receive such a prominent position in modern economic research. Here, there is no use to come with 'sledgehammer' methods; we need the statistical technique's finest tools to come to grips with the problems."

Section 7 on the trend problem touched upon the often-mentioned barrier for probability in economics, that economic time series are not recurrent events to which probability laws apply, but Haavelmo did not bring up probability explicitly. The question of trend elimination, he began, is often conceived as a purely technical-statistical problem but is in reality of far more profound character:

> In our formulations of theoretical laws we operate always with things of such nature that they *can be thought of as repeating themselves*. This holds both for static and dynamic formulations of laws. The most important economic data are given as time series, thus a quite particular series of successive events. Is it possible to test laws for recurrent events on the basis of such time bound variations?
> . . .
>
> Economic time series usually have two features that strike the eye: one is the one-sided straight development, the trend, the other is certain variations *around* the trend. Often we can track the cause of the trend back to certain slowly, changing things (e.g., changes in population size or structure), things that are outside the range of entities included in our hypotheses and also seem to be independent of the variations we wish to study. In such case it is natural to take the trend as a *datum* in the analysis and consider the things that happen *apart from* the trend. This is the rational basis for a statistical elimination of trend in our observations. It is unacceptable to make a purely mechanical trend elimination without a concrete interpretation of the trend's emergence. It could be that an observed trend has its explanation in the relations between the things that *are* included in our hypotheses.
> . . .

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> Assume that we have arrived at a determined dynamic system, such that we can solve the system, i.e., find the time paths of the variables under consideration. It might then be the case that the observed trend movements are just the possible solutions of this system. In other words the trend movement can arise as a confluent form of the dynamic system of structural equations. The observed trends can thus be taken as a statistical verification of our system of hypotheses. . . .
>
> When our test data are series with marked trend movements, it could be asserted that the hypotheses we can get verified will not be laws for recurrent events, but only a description of a historical path. If that viewpoint had to be accepted *in general*, it would be a severe blow for the attempt of establishing economic laws. But we don't have to accept this negative position. The cause of the trend is either outside our system of hypotheses, and if we can state the causes, we are allowed to eliminate the trend and consider only the residual variation, which has the character of recurrence. Or, the trend derives from the structure of the system under consideration, it is the outcome of an analysis of free variations and has its explanation by the *same* system of hypotheses which led to variations of recurrent nature.

It was not the probability approach; neither was it the occasion for it. Haavelmo's journey had not yet brought him to that stage, but he was getting closer. His experiences and studies since 1933 had advanced his thinking in leaps and bounds and prepared him for further achievement. His carefully phrased and pedagogic sentences in Copenhagen did not fully do justice to his scholarly level; he was after all not addressing econometricians. His presentation would even today serve its purpose as an excellent introduction to the fundamental problems of econometrics. Unlike much of Frisch's work it was imbued with the spirit of the experienced empirical researcher. Less than half a year later he would write home to Frisch about the need for making probability considerations about the deviations between theory and data to decide ultimately whether a theory was "good" or "bad" but still with mixed feelings about the range of applicability of this idea. It was an idea for which he on the eve of his departure was well prepared.

Haavelmo left Denmark in June 1939. His next stop was the Cowles Commission Research Conference at Colorado Springs, where Haavelmo would rejoin Jakob Marschak, who had moved to the United States at the end of 1938, and for the first time meet with Abraham Wald and also with Gerhard Tintner, M. Ezekiel, H.T. Davis, the Working brothers, Charles F. Roos, and others.

9. CONCLUSION

Trygve Haavelmo could hardly have had a more flying start on his way to becoming an econometrician than being hired, as he was, to work as an assistant to Ragnar Frisch in 1933 at the peak of Frisch's creativity and power. Frisch's laboratory provided a most valuable training ground. But in 1939 Frisch's original research program for the institute was in shambles, as his high-profile business cycles/time series project had fallen apart. The promised publications with theoretical results supporting the "ideal programme" never appeared.[114] Frisch's "new methods" in marginal utility analysis had run aground too; the revolution in demand theory left them by the wayside.[115] Confluence analysis and bunch maps had never become quite the success Frisch had hoped for, and there had been limited further development of the methods. The new approach and ideas in production theory had never taken off, as a result of Frisch's work not being translated, and empirical applications had been quite limited too. For Frisch it was defeat. Haavelmo escaped unscathed and equipped for bigger tasks.

Haavelmo's education had started firmly within Frisch's paradigms, which clearly encompassed some of the most challenging ideas for the development of econometrics, launched in the 1930s. Haavelmo's conception of econometric problems was firmly anchored in Frisch's dynamic structural equations and in the implications of confluence with regard to the identification and estimation of relationships. Although Frisch may be viewed as an inductively oriented "methods man," concerned above all about developing effective methods for confronting theory and data, Haavelmo's perspective was different. Like Tinbergen he had accumulated a big stake in empirical investigations, which naturally implied an interest in ascertaining that the empirical results were meaningful, even "true," and further an interest in the question as to what constitutes a test of a theory being valid, which was precisely Tinbergen's mandate. Frisch showed a very marked interest in promoting Haavelmo's further education. There was certainly no evidence of any cleavage between the two over the role of probability by 1939.

The lack of exact criteria in confluence analysis was pointed out by Koopmans, whose Oslo visit must have encouraged Haavelmo to penetrate more deeply into the contributions of both R.A. Fisher and Neyman and Pearson. The idea of spending two months with Neyman in 1936 was Haavelmo's own but surely influenced by Koopmans. How close was the interaction with Neyman, and how much impact did the visit have on Haavelmo? It does not seem that Haavelmo and Neyman came to be very close, nor was it necessary.[116] Haavelmo learned statistical testing from Neyman. He was, as we may assume, inspired while in London to take a deeper look into probability theory. Haavelmo may even have been as much attracted to studying probability theory by the importance for economic theory of taking stochastic influences into consideration, as he did indeed later in his career, as by the use he made of it in his probability approach. There is no evidence that Haavelmo had conceived the core of the probability approach before he crossed the Atlantic, but on the other hand, the elements in the universe he structured in his 1941 treatise were to a large extent in his baggage.

There is a kind of retrospective paradox for the many who knew Haavelmo in his post–World War II career as a serene theorist and philosopher-economist that he once was a painstaking worker in a numerical laboratory. His fame derives mainly from his econometric contributions. But was he after all a theorist and policy-oriented economist more than an econometric methodologist?

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Were the econometric puzzles he toiled on in these years, which eventually led to the "Haavelmo revolution," only obstacles that had to be solved for economic theorizing to become fully meaningful and the results applied for the betterment of society?

Haavelmo's departure for the United States in 1939 was expected both by Frisch and Haavelmo to imply a separation for one and a half or at most two years. Frisch had shifted ground and was enthusiastically working with a new breed of students on his national accounting project. Haavelmo could not have known much about what he would experience in the United States. In cooperation with Frisch he had planned the destinations for the first few months in the United States to be Colorado Springs for the Cowles Commission Research Conference, the University of Chicago, Columbia University, and the USDA in Washington, D.C.[117] There can hardly be much doubt about Frisch's desire to get Haavelmo a position in Oslo or, indeed, about Haavelmo's interest in the same. It would take nine years, however. Halfway through that period, Haavelmo's "Probability Approach" appeared as a supplement in an otherwise meagre *Econometrica* while the journal's editor was in German imprisonment in Norway. The world was in upheaval.

NOTES

1. Haavelmo (1941b, 1944). The 1944 version was a reedited, retitled, improved, and (slightly) extended version of the 1941 treatise, which was distributed in mimeographed form. The correct date for Haavelmo's "probability approach" is thus, arguably, 1941.

2. The situation changed in 1935, when economics became a full five-year study and changes in economic conditions and political winds made economics a much more attractive option.

3. Ragnar Frisch/Allyn Young, July 14, 1926. Using different words four years later he wrote a similar objective into the constitution of the Econometric Society as the definition of econometrics.

4. The grant came from the Laura Spelman Rockefeller Memorial, which under Beardsley Ruml financed empirical research facilities in several countries. The grant was $5,000 each year for a five-year period, with an additional amount of up to $5,000 each year forthcoming if the institute succeeded in soliciting support from Norwegian sources. The Oslo grant was small compared to others. The events prior to the appointment of Frisch as professor and the establishment of the Institute of Economics are set out in Bjerkholt (2000).

5. Ingvar Wedervang was professor of economics and co-applicant with Frisch for Rockefeller support. In practice the two colleagues agreed to divide the institute and the grant between themselves and not to interfere in each other's activities. Wedervang spent much of his share of the grant in collecting long time series for prices and wages in Norway; other research in his part of the institute can hardly be called anything but mediocre.

6. Frisch (1926a, 1932a). Frisch's assumptions were in 1936 shown by Abram Burk to be more confining than Frisch had been aware of, and that stopped Frisch's progress on that front.

7. Hence, the characterization of Frisch in Epstein (1987, p. 36) is to the point: "Ragnar Frisch was drawn into econometrics not so much out of interest in policy or economics reform but a curiosity to test empirically the fundamental postulates of neoclassical utility theory. He had little patience with economists less mathematically trained than himself and he gloried in exposing errors of his intellectual competitors."

8. Frisch's attitude toward the quality of statistical methods used in economics at the time became a permanent feature with him. It was reiterated in Frisch (1934a, p. 6) with 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." Even the gently phrased criticism of Tinbergen in Frisch (1938) may be viewed as fitting into this pattern.

9. Frisch (1929). In this treatise Frisch pioneered the use of matrix algebra in econometrics but discovered that few of those he tried to reach could understand.

10. Frisch (1927). Frisch's early time series work is discussed in Morgan (1990, pp. 83–89).

11. Frisch contacted Yule shortly after reading his article and discussed various issues also with him.

12. Frisch had all the ideas he later developed as the propagation-impulse model present in a lecture he gave in Stockholm immediately after returning from the United States in June 1931; cf. Bjerkholt and Lie (2003).

13. While at Yale in 1930 Frisch sent Charles W. Cobb a note with proposals for improvement of the analysis and put Cobb to work on calculating the results of some of these proposals, suggesting that they should write a common paper on the estimation of production functions.

14. These were uncontroversial goals. The third point on cooperation with industry and business seems to have been Wedervang's idea and to have received less enthusiasm from Frisch.

15. All quotes from "Memorandum to Dr. van Sickle," September 15, 1931, Rockefeller Archive Center.

16. Van Sickle still defended the support, quoting a passage from Schumpeter (1933) (written as if in support of Frisch's caveat quoted in the text): "Economic problems have most of the time been approached in practical spirit, either indifferent or hostile to the claims of scientific habits of thought. No science thrives, however, in the atmosphere of direct practical aim, and even practical results are but the by-products of disinterested work at the problem for the problem's sake" (*Econometrica* 1, 6).

17. Van Sickle in New York rebuffed the Paris office in no uncertain words: "I wondered why the funds now made available to the Institute should not be sufficient to finance the new venture. If the funds are insufficient, it is probably because a larger proportion of our funds than we had originally anticipated is being devoted to the highly abstract mathematical investigations of Frisch. In making our grant originally we had expected that the Institute would go into inductive research of a more realistic nature." Proposals for financing studies of national policies designed to promote recovery, memo from J. Van Sickle to T.B. Kittredge (Paris office), Rockefeller Archive Center.

18. In 1934 Van Sickle sought a second opinion on the Oslo activity and consulted Friedrich von Hayek, who found Frisch "more of a mathematician than an economist and [was] not convinced of the soundness of his economics." But von Hayek admitted, according to Van Sickle's notes, that Frisch would "probably develop techniques that in another generation might prove highly useful." J. Van Sickle's conversation with F.A. von Hayek, July 24, 1934, Paris, internal note, Rockefeller Archive Center.

19. Frisch/Schumpeter, December 13, 1930. The letter was apparently motivated by Frisch's perusal of Leontief (1929) and Pigou (1930); both had in Frisch's view committed grave errors. Leontief had assumed that shifts in demand and supply curves were independent of each other and claimed on that basis to be able to estimate both supply and demand elasticities. Frisch later published a booklet dissecting Leontief's approach (Frisch, 1933a); the core of the criticism was stated in the letter to Schumpeter after the general introduction quoted in the text. Pigou (1930) is discussed in Morgan (1990, p. 176). Pigou was also criticized by John MacIntyre Cassels (1933), who sought out Frisch in Oslo in 1935.

20. Frisch (1934a, p. 6). All further quotes in Section 4 are from the same publication.

21. Confluence analysis and the bunch map technique are thoroughly discussed by Hendry and Morgan (1989), who discarded the bunch maps as an outmoded technique but emphasized the importance of confluence analysis for the ensuing development in econometric methods and suggested that its links to modern cointegration analysis make it worthy of further research; see also Epstein (1987, pp. 37–41). Frisch's approach also has links to principal component and factor analysis. Malinvaud (1966) applied a simplified version of Frisch's confluence analytic approach in the intro-

ductory chapter on econometrics without stochastic models, but his book is exceptional among textbooks in this regard.

22. Frisch (1929); see also Frisch and Mudgett (1931).

23. Waugh belonged to the U.S. agricultural economists, whom Karl Fox has characterized as "world leaders in applied econometrics during 1917–33," with Waugh and M. Ezekiel as the foremost representatives; see Fox (1986). 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 visit three of his fellow econometricians in Europe, F. Divisia, E. Schneider, and J. Tinbergen, before returning to the United States.

24. A result of Waugh's work was Waugh (1935); another outcome of their cooperation during that year was Frisch and Waugh (1933).

25. M.H. Belz (1897–1975) from Australia was trained in mathematics and after teaching for some years at the University of Melbourne decided to study the application of mathematics to economics at Frisch's institute and at the London School of Economics. After teaching mathematical economics for 10 years at the University of Melbourne he became in 1948 the head of the first autonomous department of statistics in Australia.

26. Frisch wrote on the spur of the moment in April 1934 to the editor/publisher of the *Nordic Statistical Journal*, Thor Andersson, and offered an article to be sent within one week. A few days later he wrote again to say that the article might come to more than 25 pages (the usual limit). In June he indicated that the article would come to 125 pages. Frisch submitted the manuscript, which in the end came to 192 pages, shortly afterward. The "Confluence Analysis" was not published in the journal after all, as the journal folded when Andersson became ill and died. The 1,000 reprints that Frisch ordered were delivered. Reprints from a journal issue that never appeared!

27. Waugh figures prominently in the book; Belz is also mentioned. Haavelmo is only present as one of the "trained staff of computers now working at the University Institute of Economics" and had carried out the numerical work, which, Frisch admitted, had been "extraordinarily great" (Frisch, 1934a, p. 9).

28. Frisch drove the point home in chapter 33 of the book, showing by a constructed example of 100 observations of four variables, each one constructed as a linear function of two variables (random drawings). Regressions among the four variables delivered coefficients, seemingly significant. To prevent misunderstanding he emphasized the value of sampling theory in controlled experiments, referring to the works of R.A. Fisher and Wishart.

29. Frisch (1934a, p. 88). The entire passage is quoted in Epstein (1987, p. 39).

30. Frisch's efficient computation schemes based on a detailed breakdown in elementary operations, special computational sheets, etc., may be considered a step in the direction of computer efficiency, although such inventive practices had surely been around since before Babbage. He boasted of his computational techniques to the Rockefeller Foundation and could in 1936 report that methods superior to those in Frisch (1934a) had been developed.

31. Frisch had epistemological concerns about the foundation of our knowledge about the outer world that seldom came to the surface. This may or may not have been of importance for his probability position.

32. In letters exchanged between them until 1945, they addressed each other by (the slightly disrespectful) "Haavel" or merely "Håv" and "Professor Frisch," respectively, both using the polite second-person pronoun ("De").

33. Frisch (1932a); see discussion in Chipman (1998). Frisch (1934a); see discussion in Morgan (1990, pp. 207–212).

34. Frisch (1933a); see Hendry and Morgan (1995, pp. 38–40, 257–270).

35. It must have been the first-ever lectures series announced as "econometrics," given in French at Institut Henri Poincaré. More important, it was Frisch's lengthy presentation of what econometrics was about, at the height of his econometric performance. Would the lectures have become pathbreaking, like B. de Finetti's Poincaré lectures, if they had been published?

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36. Frisch (1933b) published in a festschrift in honor of G. Cassel; see Morgan (1990, pp. 90–100).

37. Frisch's publications and activities at the time are discussed by Chipman (1998) and Klein (1998) and in other contributions in Strøm (1998).

38. Frisch was a research consultant for the Cowles Commission, which from its origin had a symbiotic relationship with the Econometric Society; see Cowles Commission (1952).

39. The daily personal contact between Frisch and Haavelmo was less the first year or two as the laboratory assistants were located in the loft of the first building erected at the new Blindern campus of the university, far from Frisch's office, but this was hardly a factor of much importance. Later the contact was very frequent, Haavelmo spent much of his time at the large conference table in Frisch's office in the main university building in downtown Oslo.

40. He may have applied for other positions earlier. Haavelmo kept a copy of a reference letter written by Frisch, dated November 1934, in which Frisch described Haavelmo's work as secretarial, comprising typewriting of manuscripts, proofreading, and filing scientific notes but also checking mathematical formulas, numerical checking of statistical and other tables, providing numerical examples, etc. Frisch praised Haavelmo as nimble, energetic, discreet, and pleasant and as someone who had his unconditional trust, adding that he would much regret if Haavelmo took another position but found it reasonable that he sooner or later would do just that, as the University Institute of Economics regrettably had no opportunity to offer its employees much in terms of salary.

41. Haavelmo was not employed by the university and thus was not on a career path. The university had recruiting positions ("universitetstipendiat"), but no vacancy was in sight. Haavelmo may at the time have underestimated his future possibilities. He would surely have known that the Rockefeller grant was given for five years and thus was due to expire in 1936 but perhaps not that feelers were out for extended Rockefeller support, and also for government research contracts, which eventually saved the institute. From what he told a younger colleague many years later his application was a question of job security and he did not really mean to take the position.

42. Koopmans/Frisch, March 25, 1935. Koopmans had studied mathematics and theoretical physics before he became one of Tinbergen's students in 1934 and embarked on writing a doctoral thesis in mathematical statistics.

43. Frisch/Koopmans, April 11, 1935.

44. Koopmans left densely written lecture notes; Koopmans (1935). The lectures were divided into three parts: fundamental concepts, Fisher's theory of estimation, and Neyman and Pearson's theory on hypothesis testing. Koopmans came to Oslo from London, where he was in touch with R.A. Fisher, J. Neyman, and E. Pearson. He also returned to London from Oslo.

45. Paul G. Hoel, of Norwegian extraction, had followed Frisch's lectures on time series analysis in Minnesota in the spring of 1931 and later completed his doctorate. Hoel authored a textbook in statistics that was used in Oslo after World War II until it was replaced by Frisch's and Haavelmo's chapters and sections for a textbook that never was completed.

46. Frisch/Hoel, October 15, 1935.

47. Koopmans's dissertation was jointly supervised by Tinbergen and by Hans Kramers, a leading theoretical physicist in Netherlands, published as Koopmans (1937), and presented to fellow econometricians at the Econometric Society meeting in Annecy, September 1937.

48. Qin (1993, pp. 16–18); cf. Morgan (1990, pp. 238–241).

49. I owe this neatly expressed way of relating the endeavors of Koopmans and Haavelmo to an anonymous referee.

50. The lectures were also followed by foreign visitors Georg Rasch (Denmark), Georges Lutfalla (France), John M. Cassels (England), and some Norwegian actuarial students.

51. The author once queried Haavelmo about his impressions from Koopmans's visit. Haavelmo stated merely that at the time his position was such that he was not invited to take part in the real discussions. Thus there may not have been much personal contact between the two at the time. Haavelmo is rumored to have found Koopmans a somewhat ascetic "carrot eater."

52. One of those who received Koopmans's lectures in December 1935 and was invited to give comments was Samuel S. Wilks, with whom Frisch had been in touch for some years. Wilks commended Koopmans on his presentation, pointing out the need for Koopmans to incorporate new results that Neyman and Pearson were publishing on the testing of hypotheses and statistical estimation (Frisch/Wilks, December 14, 1935; Wilks/Frisch, January 21, 1936).

53. He characterized the building of such a bridge as one of the urgent tasks in mathematical statistics at the moment. Koopmans had—in Frisch's words—made a first attempt at supplying "the missing link" between the confluence and sampling approaches, and he encouraged Samuel Wilks to build "a more embracing theory in this field" (Frisch/Wilks, February 1, 1937).

54. Haavelmo came to the institute too late to assist Frisch with the Cassels festschrift article (Frisch, 1933b) but surely must have studied it at the time of publication.

55. Haavelmo reminisced occasionally in his later years about the all the wasted time spent in calculations for Frisch.

56. Frisch (1926b), in his doctoral thesis on a topic of mathematical statistics, pointed in the concluding paragraph toward the deeper aspects of the problem of reconstructing from data the probabilistic scheme that had created them: "The inverse problem: how to reconstruct from an empirical distribution the scheme which has given birth to the observed distribution is a problem of a rather different kind. To deal with it in depth one cannot avoid entering into philosophical issues and in particular into the theory of knowledge. It seems to us that too often the scholars in statistics and mathematics have refused to enter into these philosophical issues, instead confining themselves only to deal with technical questions. That is the reason in our opinion why the critical interpretation of the foundation and the methods of statistics have not kept in step with the development of techniques and the increasing range of applications of our discipline in the social as well as in the natural sciences." Which problem he had in mind at the time of writing is not easy to say, but "the inversion problem" in "shock theory" became a nut too hard to crack in the laboratory and led almost to the despair of his assistants.

57. The quotes are from Frisch's report to the Rockefeller Foundation in 1936.

58. Frisch also continued the project after Haavelmo had left for the United States. The announced forthcoming publications by Frisch about the project never appeared; only Frisch (1933b), downplayed by Frisch as only a small result, was published. A theoretical treatise on the project came close to being published both as a long *Econometrica* article and as a Cowles Commission monograph in the early 1930s. Eventually the war buried the project. The Frisch archive comprises large files of notes and computations from the project. It is thus only scantily dealt with in the history of econometrics. As nothing was published, Frisch's ambition has never been given a thorough assessment. Thus Morgan's excellent treatment (1990) of Frisch's early work suffers from this. Andvig (1986) is more comprehensive as he also draws on unpublished archive material. Haavelmo published two *Econometrica* articles as spinoffs from his work with Frisch on the project (Haavelmo, 1938, 1940).

59. Frisch (1934b, 1935).

60. The lectures on monetary theory dealt extensively with Wicksell (and his Swedish successors E. Lindahl and G. Myrdal) and Keynes but also discussed a number of other contributors.

61. Haavelmo may seem to have adopted from Frisch his enormous admiration for Wicksell. On Keynes (after 1936) Haavelmo clearly had a much more positive opinion than Frisch.

62. As the interviews were entirely hypothetical, the survey was perhaps an early example of a "stated preference" approach, a kind of empirical experiment. Frisch had throughout his life a firm belief in the use of structured interviews as a source for hard to get information. The idea can be found in his works from 1926 until 1970, including Frisch (1938).

63. The report from the project was not released for publication.

64. Report of the work done under the direction of Professor Ragnar Frisch at the University Institute of Economics, Oslo, January 1932–June 1936 (pp. 23–24), Rockefeller Archive Center.

65. Several leading members in the Econometric Society strongly held the opinion that new members had to fulfill requirements, proving that they were worthy of membership; see Bjerkholt (1998, pp. 39–41).

66. About the discussion of Keynes's new theory at the meeting, see Young (1987).

67. "Macrodynamics" had been a hot topic at European Econometric Society meetings since Frisch presented the "propagation and impulse" model at the Leyden meeting three years earlier (see Frisch 1934c).

68. Frisch's "ideal programme" (*Econometrica* 5, 365–366) is quoted and discussed in Aldrich (1989) and Qin (1993, p. 48). The "ideal programme," which included confluence analysis as estimation tool, presupposed the solution to the "inversion problem." Frisch also had a third presentation at the meeting, a paper that tried to cope with the criticism raised by Abram Burk against his marginal utility measurement.

69. The problem was well known to Frisch and Haavelmo, who had given thought to how to solve the problem in more variables. Allen reworked and published the paper as Allen (1939), just prior to Wald's solution of the problem (Wald, 1939).

70. The title of Neyman's lecture is from the report and differs from that of the program for the meeting sent out by E.H. Phelps Brown at the beginning of September, which stated the title of the lecture as "Statistical Studies of Economic Relations, with Particular Reference to Covariation in Time." The report on Neyman's lecture, part III on testing, was unusually detailed and was prepared by the editor (*Econometrica* 5, 367–371).

71. Haavelmo (1937). The original paper has been lost. It was eventually published, under a different title, as Haavelmo (1938); see the discussion that follows in the text.

72. After having his note approved by Neyman, Frisch had it inserted into the *Econometrica* report from the Oxford meeting. This suggests that Neyman and Pearson's work was new to Frisch and that he found it of great interest for the *Econometrica* readers.

73. Reiersøl in Oslo had in the meantime cracked the problem of finding the limits of the true regression coefficient from the spread of the beams in the bunch map for at least four variables.

74. Even Hayek belonged to Frisch's network. They had been in touch since 1928, while Hayek still was at the Institut für Konjunkturforschung in Vienna.

75. The report was not published until 1945; cf. "En statistisk analyse av bakeprøver" in *Kristiania Brødfabrikk A/S og Rosenborgkomplekset gjennom 25 år*, Oslo, pp. 172–181.

76. Douglas (1976, p. 905), posthumously published. Frisch may have heard Douglas boasting at the 1927 meeting or later about the high correlation achieved in the original Cobb–Douglas estimation, as if it was proof of a valid specification and correct estimation, thus confirming Frisch's view of widespread ignorance about the meaning of regression results. Horst Mendershausen, of German origin, obtained his doctorate in Geneva. He held a Rockefeller fellowship in 1937–1938, spent a short time on the staff of the Cowles Commission, and then joined the faculty of Colorado College. He left academia after the war.

77. Mendershausen (1938). Douglas's conviction that $R = 0.97$ was the proof of the pudding seems, however, to have been unshaken; cf. Samuelson (1979).

78. Frisch and Haavelmo (1938). The article was close to 100 pages. It is the only joint paper of Frisch and Haavelmo.

79. The advice to study mathematics as a prerequisite seems curious as Haavelmo 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 had acquired matrix algebra, harmonic analysis, and much more and on top of that a whole battery of methods of numerical analysis. His mathematical skills, of course, were far less than those of Frisch.

80. Frisch's advice may have been influenced by whom he was with at the time. C.F. Roos, T. Yntema, and J. Marschak attended the Colorado Springs conference, as also did Louis Bean. Roos had become the first research director of the Cowles Commission in 1934, but in 1937 he had left to work at the Mercer-Allied Corporation, New York. Yntema, of the University of Chicago, would become the next research director after the move of the Cowles Commission to Chicago in 1939. Marschak succeeded Yntema as research director in 1943. Marschak, Yntema, and Frisch may all have been offered the position of research director during the conference that Frisch attended; see

Cowles Commission (1952, p. 18). Bean was at the time statistician to Secretary of Agriculture Henry A. Wallace.

81. Tinbergen talked about his work as "macro-economic" as proved by the occurrence of this term in Tinbergen (1939, vol. I, p. 10), which may well be the first occurrence ever of this term in print in English. It should really be dated to 1938, as the book was distributed as a proof copy one year before it was officially published. Tinbergen put the term in quotes; his formulation suggested that it was in colloquial use, presumably in Geneva, but surely in Oslo also. Frisch had after all introduced it in Norwegian in Frisch (1934b). Tinbergen included a brief introduction to confluence analysis in vol. I, pp. 26–30.

82. Shortly after Tinbergen's visit a vacant position, "Membre de Section à la Section Financière et Service d'etudes économiques du Secretariat de la Société des Nations," was announced. Haavelmo became interested, as it would mean working close to Tinbergen. He was encouraged to apply by Frisch and got recommendation letters from E. Pearson and J. Tinbergen but did not succeed in getting any offer.

83. Morgan (1990, p. 99) also seems to lean toward this interpretation.

84. Neyman accepted an offer from the University of California at Berkeley in April 1938 and left soon afterward.

85. 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 awhile by German censors, who suspected that the data were ciphered messages!

86. Stumpff's *Grundlagen und Methoden der Periodenforschung* was studied by Haavelmo during the visit.

87. Stumpff's equipment was also tried out on data referred to at the institute as the "Yale data," a constructed data set with four sinusoidal components and one erratic component, which Frisch had used as teaching material at Yale and Minnesota in 1930–1931. Frisch 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 labor saving, but expensive, equipment.

88. Haavelmo also got an impression of life under Hitler's Nazi regime during his short stay. He reported home about the high work intensity everywhere and the overfilled theaters, cinemas, and restaurants. He also noted the somber mood at the university and the constant fear of losing positions "for political reasons." On entering a room with a "Guten Tag," he would be met by fearful faces and "Heil Hitler," loud and clear (Haavelmo/Wedervang, February 19, 1938).

89. The League of Nations' Economic Intelligence Service had for several years, supported by a 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 Haberler (1937). The second phase was the statistical verification and mathematical testing of the alternative explanations. The investigation had been led by Tinbergen since 1936.

90. At the 1937 meeting of the Econometric Society in Annecy one-third of the participants came from Geneva, but the total number of participants was much lower than in Oxford!

91. Wald fled Austria after Anschluss in the spring of 1938, headed for the United States. Frisch was in contact with Wald and encouraged him to come via Oslo and leave for the United States from a Scandinavian port. Wald was positive but in too much of a hurry to make the detour.

92. Haavelmo's enthusiasm about the research atmosphere was shared by Jaques J. Polak, who worked as Tinbergen's assistant: "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).

93. The project and the reactions to it are discussed at length in Morgan (1990, pp. 108–130). Haavelmo's close contact with Tinbergen's project may have been of importance for his later sup-

port of Tinbergen in the controversy with Keynes in Haavelmo (1943); see Morgan (1990, pp. 128–129). Haavelmo also commented upon econometric aspects of Tinbergen's work in Haavelmo (1941a).

94. Tinbergen returned to Rotterdam in late 1938, and the business cycle studies were taken over for some months by Tjalling Koopmans. The entire research unit, comprising the director, Alexander Loveday, and about 10 senior officials, moved to the Institute for Advanced Studies at Princeton in August–September 1940; see Polak (1994, p. xiv). Koopmans, whose contract had expired in June 1940, also went along to Princeton.

95. It was a joint review that also comprised a treatise on econometrics by J.J.J. Dalmulder from the Netherlands. Tinbergen's book on his first macrodynamic model (see Morgan, 1990, pp. 102–108) was reviewed in the English edition, published in Paris in 1937. It was probably Erich Schneider who offered Haavelmo review work for *Weltwirtschaftliches Archiv*.

96. Haavelmo/Frisch, February 27, 1938. 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)!" (Frisch/Haavelmo, March 3, 1938, translated by the author). Haavelmo's paper was published in the July 1938 issue of *Econometrica*, prominently placed by Frisch before Hotelling's famous taxation article in the same issue. Frisch may have changed the title, perhaps without consulting Haavelmo.

97. But it played right into Haavelmo's 1940 paper in *Econometrica* where he reconsidered the riddle and showed that a rocking horse, indeed, was *not* necessary for random shocks to create a cycle. Haavelmo had flirted with this idea since 1938 but did not resolve it until after arriving in the United States. The opening of the paper drew attention to a new focus: "The whole question is connected with the *type of errors* we have to introduce as a bridge between pure theory and actual observations" (Haavelmo, 1940, p. 312). Haavelmo showed that a macrodynamic model with coefficients that gave the propagation part a noncyclic character, say, damped exponentials, could still generate cycles when exposed to random shocks, invalidating the idea that had been promoted by Frisch that the deterministic part of the model had to have damped cycles for the models exposed to shocks to generate cycles.

98. Haavelmo had sent the note to Frisch, who returned comments to Haavelmo's next location in Oxford. Frisch found the idea in the paper fruitful but not well enough corroborated 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 remarked that there was more of a "mathematical slant" in Haavelmo's work but reminded him nevertheless about Carthago (Frisch/Haavelmo, May 5, 1938, quotes translated by the author).

99. In an interview on becoming professor at the institute in 1948, Haavelmo said he had wasted a month in Paris trying to solve a differential equation that did not have an analytic solution. He found out finally from Whittaker and Watson (1927) that he had hit upon Mathieu's differential equation. Thus Frisch was right; he did not know mathematics!

100. When Marschak prepared to leave Germany in 1933 he had queried Frisch about possibilities for settling in Oslo. Frisch was enthusiastic, but the Rockefeller Foundation refused to support Marschak in Oslo. Marschak fled, ending up in England. He apparently tried to model his institute to some extent on Frisch's. It focused on business cycle analysis and was also founded by the Rockefeller Foundation.

101. These were published as Tinbergen (1939) but were available for the conference participants in printed proof versions dated 1938. Volume II was missing chapter VII, which was sent to the participants in mimeographed form before the conference. The assertion by Morgan (1990, p. 125, n. 22) that only Tinbergen's volume I was discussed at Cambridge may seem inaccurate, as it is also obvious from Frisch (1938) that he commented upon volume II.

102. Frisch (1938). Dennis Robertson, who organized the conference, received the dispatch from Frisch three days after the conference ended and replied, "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, July 23, 1938).

103. The assertion by Epstein (1987, p. 57) that Haavelmo attended the 1938 conference at Cambridge is inaccurate.

104. Schneider, whom Haavelmo knew from visits to Oslo, seems to have stimulated and influenced his interest in investment theory. Among other staff were Professor Torkil Kristensen and lecturers Kjeld Philip and Jørgen Gelting. Both Kristensen and Philip served as cabinet members in Denmark after the war; Kristensen was for several years secretary general of OECD. Gelting's claim to fame is his discovery of the balanced budget multiplier, published in Danish after Haavelmo had left but prior to Haavelmo (1945), hence a case of "who influenced whom"; see Andersen and Kærgård (2000), who discuss Haavelmo's stay in Denmark in more detail.

105. Haavelmo (1939a).

106. It was Davis and Nelson (1937) vs. Westergaard and Nybølle (1927). H.T. Davis and W.F.C. Nelson were both associated with the Cowles Commission. Haavelmo's assessment was summed up in a note dated January 27, 1939, most likely written to Frisch. A loser in the competition was F.C. Mills, *Statistical Methods Applied to Economics and Business* (Henry Holt) as the latest 1938 edition was not available.

107. Haavelmo's publications (1939b, 1939c) were nos. 4 and 5 in the series; see discussion in Andersen and Kærgård (2000).

108. Haavelmo referred at the time to the paper as "A Dynamic Study of the *Regulated* Pig Production in Denmark" (author's emphasis).

109. Haavelmo/Rockefeller Foundation, April 15, 1939.

110. Frisch gave Haavelmo in this connection 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 framework. Indeed, he could probably be classified just as well, or even better, as a statistician. He combines in an unusual degree the qualities of an economic theorist and a statistician. He is very energetic" (Frisch/Rockefeller Foundation, May 25, 1939).

111. Haavelmo was not told until mid-November 1939 that a fellowship for 1940 would be granted. Haavelmo discovered in mid-1940 that one of the two recruiting positions in economics at the University of Oslo ("universitetstipendiat") had become vacant. He applied and got it beginning in 1941. The position had some, but limited, teaching duties.

112. Haavelmo (1939e). The paper also had references to the work of Herman Wold.

113. Haavelmo (1939d), title, section headings, and excerpts translated by the author.

114. Frisch promised a forthcoming publication for the last time in Frisch (1938).

115. The new demand theory built on Slutsky's 1915 paper. Frisch, ironically, was one of few economists, or even fewer demand theorists, in the world who was in possession of Slutsky (1915), received as a reprint from the author in 1926.

116. Haavelmo is not mentioned in Neyman's biography by Constance Reid (1982); neither is the Oxford meeting. The biography was written in close interaction with Neyman as a year-by-year account. The second half of 1936 was in Neyman's memory the time when he had his celebrated paper on confidence intervals rejected by *Biometrika*, edited by E. Pearson. A type I error on Pearson's part, one might surmise! Haavelmo was there but perhaps was not aware of this strain in the Neyman–Pearson relationship.

117. Cf. Haavelmo/Rockefeller Foundation, April 15, 1939.

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