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TRYGVE HAAVELMO AT THE COWLES COMMISSION

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The article reviews the early history of the Cowles Commission (CC), its close and intertwined relations with the Econometric Society (ES), and the influence and guidance of Ragnar Frisch. It provides detail on three rounds of choosing a research director for CC in 1937–38, in 1939 and, particularly, at the end of 1942. Haavelmo's work in the early 1940s came to play a major role for the econometric research at the Cowles Commission under Jacob Marschak as research director 1943–48. The article points to the importance of Abraham Wald and Jacob Marschak for the success of Haavelmo's venture and its influence and tells the story of how it came about that Haavelmo's ideas were adopted, applied, and disseminated by the Cowles Commission. Thus the mention of Trygve Haavelmo in the title is referring also to his econometric ideas. The ideas themselves and their further evolvement at the CC have been a dominating theme in the history of econometrics literature, e.g., Hildreth (1986), Epstein (1987), Morgan (1990), Qin (1993), and Christ (1994). The article discusses the recruitment, the inner workings, and various other concerns of the Cowles econometricians, from Marxism to Black Magic. It recounts at some length the efforts made by Marschak to recruit Abraham Wald to the University of Chicago and the Cowles Commission. The article can be read as a sequel to Bjerkholt (2005, 2007).

1. INTRODUCTION: TRYGVE HAAVELMO, ABRAHAM WALD, AND JAKOB MARSCHAK

Trygve Haavelmo arrived in the U.S. at the end of June 1939. He was on a study trip with funds for about nine months, although he hoped in some way to stretch his stay to last for year or even a little bit longer. He traveled directly to Colorado

I owe sincere thanks to the Editor, Peter Phillips, for suggesting this paper and providing most valuable advice in the process. I acknowledge valuable suggestions from an anonymous referee, including advice on how to structure the paper. The paper draws on archival source material from the Frisch and Haavelmo Archives, Department of Economics, University of Oslo; the Ragnar Frisch Correspondence, National Library of Norway; the Jacob Marschak Papers, Young's Research Library, UCLA; and Special Collections, UChicago Library. I am grateful for exquisite assistance from Brandon E. Barton of UCLA, Christine Colborn of UChicago Library, Monica S. Blank of Rockefeller Archive Center, and staff at the National Library of Norway. I have benefited from personal communication with T.W. Anderson, Dorothy F. Anderson, John Aldrich, Kenneth J. Arrow, Selma Arrow, John Chipman, the late Leonid Hurwicz, the late Lawrence R. Klein, Sonia Klein, Mary Morgan, Duo Qin, the late Stein Rossen, and Stephen Stigler. Thanks are due to research assistant Ge Ge for assistance with the Haavelmo Archive and to Inger Bjerkodden for invaluable encouragement all along. The responsibility for all remaining errors is entirely my own. Address correspondence to Olav Bjerkholt, University of Oslo, PO Box 1095, 0317 Oslo, Norway; e-mail: olav.bjerkholt@econ.uio.no.

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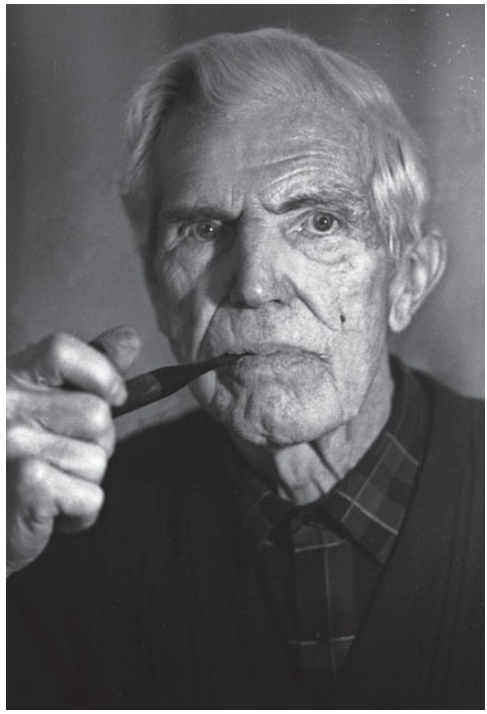


FIGURE 1. Trygve Haavelmo at ease in his home in Oslo after the 1989 Nobel celebrations had been endured. (Copyright: Olav Johannes Bøthun)

Springs to attend the Fifth Cowles Commission Research Conference in Economics, July 3–28, 1939. Just as Haavelmo was about to leave Oslo, his teacher and mentor Ragnar Frisch sent a letter to Alfred Cowles 3rd to say that Haavelmo was practically on his way. Haavelmo's visit had been mentioned between them already. With uncanny foresight Frisch wrote to Cowles as follows: '[Haavelmo] is a constructive mind and may be able to give you worthwhile ideas in connection with the research program of the Cowles Commission.'[1] Worthwhile, indeed!

Among the more than 100 visitors attending the conference in 1939 Haavelmo soon met Jakob Marschak and Abraham Wald. It was a reunion with Marschak whom he had met twice in Oxford, in 1936 and 1938, and with whose work in the Econometric Society he was quite familiar. But Haavelmo had never met Wald, although their paths had very nearly crossed in Europe. It was a fortuitous encounter which turned out to have a significant impact on the corroboration and dissemination of the methodological essay that eventually would result in 1941 from Haavelmo's grappling with the problem of statistical testing and measurement of structural relations in economic theory with due attention to their

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probabilistic character. Haavelmo was the youngster in this trio, 27 years old, while Wald was 36, and Marschak 41.

A few years later Marschak was research director of the Cowles Commission in Chicago and made repeated and largely unsuccessful attempts at recruiting Wald and Haavelmo, both of whom he came to rate very highly, into his research team. Marschak's research program at CC came to be very much based on ideas developed from Haavelmo's work. Thus the name of Trygve Haavelmo in the title of this article alludes to Haavelmo's work inspiring and influencing the research program of the Cowles Commission 1943–48, as much as to his presence in the events recounted (which is less than that of Marschak and Wald). The accidental meeting of Marschak, Wald, and Haavelmo at Colorado Springs in 1939 took place when Haavelmo's ideas for his work in the U.S still were at an embryonic stage. Marschak and Wald were his first discussion partners and both came to play important role for the success of Haavelmo's work.

Haavelmo is sometimes depicted as if he was a taciturn, shadowy figure. Taciturn he may have appeared but not shadowy; he had a marked presence in the company of econometric colleagues, always prepared to rise to any challenge. Even his taciturnity was telling. When Marschak turned 60 in 1958, he wrote to Haavelmo to acknowledge greetings, adding: 'You are not sixty yet, but I may tell you right now how much I owe you! I learned from your silence as much as from your words.'[2] Then he went on to state how the 'stochastic view' had dawned upon him at Haavelmo's hospital bed in 1942 (see Section 4).

A shared experience for this trio was that they had all visited or worked with Tinbergen in Geneva. Marschak had been there in April 1937 while Wald worked in Tinbergen's group the entire autumn of 1937. Haavelmo had spent some months in Tinbergen's group in the spring of 1938 while the work approached completion.[3] Marschak was, however, the only one of them who had attended the Business Cycle Conference at Cambridge, July 18–20, 1938 to discuss Tinbergen's work for the League of Nations.[4] Shortly before the Cambridge conference Haavelmo visited Marschak in Oxford and Tinbergen's work was surely discussed among them. Haavelmo gave a couple of staff seminars at Marschak's institute.[5]

Immediately after the CC Research Conference Haavelmo sent Frisch an eyewitness report:

> 'I have been here all four weeks and I don't think I could have used the time better. … Among the people here I rank A. Wald and Marschak on top. Marschak is even more all-round than I had realized and has exerted a very propitious influence on the discussions.[6] Wald is really an exceptional mathematician and statistician. I have a lot to learn from him. (I haven't known much about his work until now. He will take over Hotelling's position at Columbia.) Wald has developed a new regression method (so far for two variables), as he presented in his paper [published as Wald 1940b]. He assumed "errors" in both variables.… We shall still have a couple of weeks

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> together out here after the conference. Marschak, Wald, [Gerhard] Tintner and a couple more will remain here, and we have planned an informal colloquium to discuss statistical testing and estimation. I think it will be quite interesting.' (Haavelmo to Frisch, July 28, 1939, transl. by the author; cf. Bjerkholt, 2007, pp. 783–784.)

Wald and Marschak were also recent arrivals. Wald had arrived exactly one year earlier, also directly to Colorado Springs for the Fourth CC Research Conference, and with little hope (and perhaps no wish) of returning to Vienna where he had studied and worked since the early 1920s. Wald had, however, got a short-term Carnegie fellowship (arranged by Hotelling) soon after his arrival and left Colorado Springs for Columbia University already at the end of September 1938. He retained his research fellow status with CC but never returned. From the spring of 1939 he held a teaching position at Columbia.[7] Marschak had arrived later, at the end of 1938, with a Rockefeller fellowship, on leave from his directorship of the Institute of Statistics at Oxford University. The nine months fellowship was by August 1939 about to expire and at the outbreak of WWII Marschak surrendered his Oxford position and sought refuge at the New School of Social Research in New York with no intension of returning to Europe.

Haavelmo learnt soon after arrival in Colorado Springs that the Cowles Commission would move to Chicago shortly after the conference. He had heard about the Cowles Commission almost since its inception from Ragnar Frisch, who in 1932 had been instrumental in establishing the close, symbiotic relationship between the CC and the Econometric Society. Frisch had spent much of the summer of 1934 in Colorado Springs writing research notes and advising Cowles on a number of issues, while Haavelmo as assistant at Frisch's Institute of Economics in Oslo slaved away at proofreading the *Confluence Analysis* (Frisch, 1934).[8] To Haavelmo the Cowles Commission was not so much a research institute as the center and heart of the Econometric Society.

At the time of his arrival in the U.S. Haavelmo's research plans comprised what he had stated in his application letter of April 1939 to the Rockefeller Foundation, in which he had characterized himself as 'especially interested in economic studies along the econometric line' and stated three research topics as 'the general problem of connecting economic theory and statistical observations,… some special oscillation problems in economics dynamics, [and] a study of individuals economic behaviour, particularly dealing with the problems of individuals planning over time.' The application ran into problems. T.B. Kittredge at the Paris office of the Foundation insisted upon an interview, a proper CV, a more detailed study plan and corroboration of Haavelmo's future prospects at the university. Haavelmo returned the filled-in application form that came with Kittredge's letter shortly before he left for the U.S. in June 1939. In entry for 'subject of study' he restated his topics slightly but significantly as 'statistical testing and measurement of structural relations in economic theory, and individual economic behavior in planning over time' with some additional specifications on a separate sheet. He dropped out the 'oscillation problems' (which can have been nothing but problems

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within Frisch's propagation and impulse paradigm). As for why he had chosen the stated topics he wrote, 'because they are central problems of econometrics, which is my chosen field, and because I expect to obtain results which will be of importance for further research work.'[9]

In August 1939 while still in Colorado Springs he heard from the Rockefeller Foundation that a fellowship might be possible but required clarification of additional details. When inquiring friends and acquaintances he was told his chances were poor, it would be an exception from usual practice. Hence, in the middle of November he wrote to Frisch that he had more or less given up and decided to stretch the means he had as far as possible before returning home. Only two days later he was notified that he had indeed got the Rockefeller fellowship from 1940.[10] A counterfactual to consider here is that if Haavelmo had returned to Norway when his original funding expired he would have been back before the German attack on Norway in April 1940 and it may seem utterly unlikely that his name would have figured in the history of econometrics.

Did Haavelmo during his long stay in 1939 in Colorado Springs envision completing a long and penetrating essay on the theory and measurement of economic relations, as he eventually did in Haavelmo (1941), the early version of the *Probability Approach* (Haavelmo, 1944)? Haavelmo was determined to come back to Frisch with something to show for himself on his stated study topics but it was hardly likely that he imagined completing a monograph. His intention was to return to Oslo, complete a doctorate, and get a university position (as stated in his Rockefeller application). His other stated study topics, on individuals planning over time and 'oscillation problems', also comprising stochastic ideas, never really took off. They got crowded out when Haavelmo gave full attention to the need for probability in econometrics.

Haavelmo's encounter with Wald and Marschak in 1939 was fortunate and important as the interest that Wald and Marschak took in Haavelmo's ideas emanated from the exchange and discussion at that meeting. Their interest, particularly that of Wald, spurred Haavelmo on in his attempt to resolve the various issues involved in 'connecting economic theory and statistical observations' over two peripatetic years, until the completion of Haavelmo (1941). That the quality of the result was improved through the contact with Wald is obvious from Haavelmo's acknowledgement of debt to Wald:

> 'My most sincere thanks are due to Dr. A. Wald of Columbia University for numerous suggestions and for help on many points in preparing the manuscript. Upon his unique knowledge of modern statistical theory and his profound training as a mathematician I have drawn very heavily. Many of the statistical sections in this study have been formulated, and others have been reformulated, after discussions with him.' (Haavelmo, 1941, v.)

It was, indeed, most valuable help that Haavelmo got from Wald. They had a work session together at the 1939 conference (including postconference hiking

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and ad hoc discussion sessions), continued during the spring of 1940 in New York when Haavelmo attended Wald's lectures, and followed by more discussions at the 1940 CC Research Conference, which they both had joined mainly for the chance to spend time together. In the absence of a CC conference in 1941, Haavelmo persuaded Wald to come along with him for one month in a remote forest and lake fishing area in Maine in June 1941. In Maine, Wald helped Haavelmo out with the mathematical reasoning in a couple of intricate sections of the essay. Wald, who was not particularly keen on trout fishing, may have spent time on the manuscript while Haavelmo was fishing.

There was no acknowledgement of debt to Marschak in Haavelmo (1941); Haavelmo was not much for idle compliments.[11] But Marschak's influence on the dissemination of Haavelmo's innovative thinking through his instrumental help in getting the *Probability Approach* prominently published and, even more important, in propagating Haavelmo's ideas both into his own research and teaching, and into the research program of the Cowles Commission (see Section 4), was overwhelming.

There was substantial contact with Marschak during the period Haavelmo struggled with his 1941 essay. Haavelmo credited Marschak for having suggested at the 1939 CC conference the idea that led to Haavelmo (1940), a paper that relaxed the conditions Frisch (1933) had stated for random shocks to result in cycles.[12] Another instance is an unusual acknowledgement remark in Marschak's contribution to the Henry Schultz memorial volume: 'The article could not have been written without the stimulating influence of talks with T. Haavelmo' (Marschak, 1942, p. 135), dating the interaction very precisely to March 1940. In his contribution Marschak discussed a problem with links back to the *Pitfalls* controversy in 1933–34, see Morgan (1990, pp. 183–187), not entirely unrelated to what would appear (Haavelmo, 1943a).[13]

Haavelmo (1941) got poorly disseminated. Haavelmo distributed the hectographed document himself to friends and acquaintances, in total perhaps less than 100 copies. It was hardly cited more than 4–5 times in journal articles, among these are Klein (1943) and Marschak and Andrews (1944), before it was superseded by Haavelmo (1944).

The only other acknowledgement of importance in Haavelmo (1941) was of Ragnar Frisch of whom Haavelmo wrote that the reader would 'recognize many of Frisch's own ideas in the following, and indirectly, his influence can be traced at almost every point, in the formulation of problems and the methods of analysis' (Haavelmo, 1941, p. v). Indeed, Haavelmo was permeated by Frischian ideas and thinking. It happened more than once in the United States that he was sought by people because he was Frisch's student and thus presumed to carry a clue to some of Frisch's ideas and assertions. Both Marschak and Wald may also have looked at Haavelmo as a conduit of Frisch's ideas. They had both been under the spell and influence of Frisch, as had also others, both elder statesmen and younger – particularly European – members of the Econometric Society.

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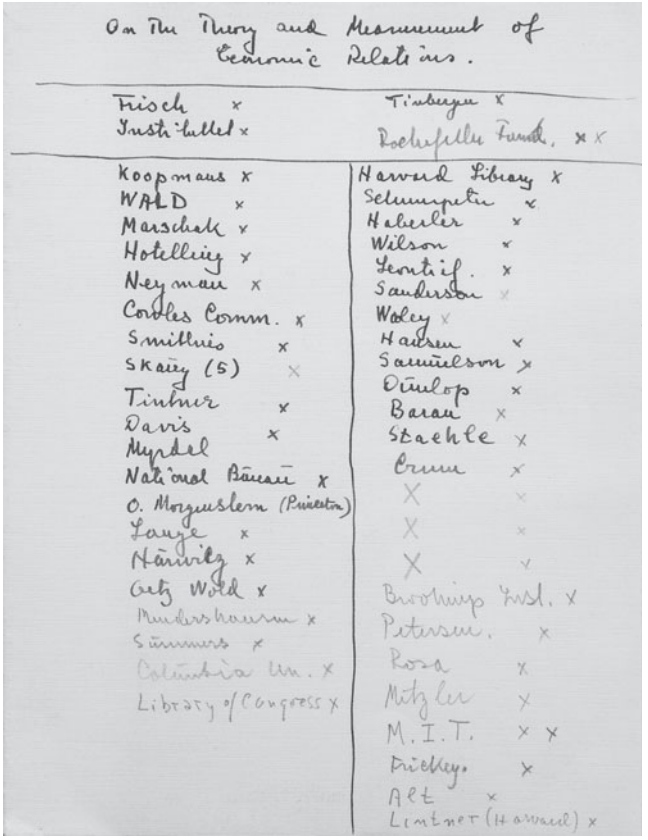


FIGURE 2. Trygve Haavelmo's notebook record of copies distributed of the hectographed *On the Theory and Measurement of Economic Relations* (1941). The two copies sent to Oslo never arrived.

Throughout Marschak's life, circumstances and choice took him steadily westwards from student days until his last move from Yale to UCLA in 1960.[14] He continued to take an interest in new topics and worked on them to achieve complete understanding. After arriving in Heidelberg in 1930 he became an eager member of the newly founded Econometric Society. He took part in all the European meetings until his departure for the United States in 1938. It is hardly much of an exaggeration to place Marschak next to Tinbergen among the most dedicated members of the Econometric Society in the 1930s in the pursuit of Frisch's vision of a new discipline. There is an interesting observation of Marschak by Henry Schultz at the Leyden meeting in 1933. In his diary, Schultz noted 'the quickness of apperception of J. Marschak' and further that 'M[arschak] impressed everybody with the way in which he "ate up" the lectures on linear transformations and

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quadratic forms, and in which he followed all other discussions. He has a first class mind.'[15]

In peaceful Heidelberg the political events in Germany in early 1933 caused immediate concern for people of Jewish extraction. In February 1933 Marschak confided to Frisch that he considered leaving Germany. Frisch raised the prospect of Oslo as a place to settle and Marschak responded that he had no higher wish in the world. Working with Frisch on econometric issues and perhaps teaching as well would be ample compensation for a possible decrease in income. To pick up another language Marschak did not seem to regard as much of a problem. Frisch was enthusiastic, to say the least. In the middle of March 1933 Marschak left Germany with his wife and two small children for Vienna, from where they moved to Paris at the end in April. Frisch was, however, disappointingly unsuccessful in persuading the Rockefeller Foundation to provide the necessary support.[16]

Marschak moved on to London in June 1933 and arrived, finally, in Oxford in July where he received help to establish a new existence and adapt to English university life.[17] Marschak and Frisch conducted an intensive correspondence throughout 1933. Even in that uprooting situation for Marschak the letters dealt mostly with Econometrica and Econometric Society matters down to subtle changes in the title and headings of Marschak's first paper submitted to Econometrica.[18] Although he had forewarned Frisch that he might have difficulties fulfilling his commitments in view of his somewhat harrowing circumstances, he fulfilled them, including writing the report from the Leyden meeting. By then Marschak was living in Oxford on a somewhat meager subsistence pay as a lecturer but from 1935 the situation improved when he became the director of a new Institute for Statistics.[19]

Abraham Wald joined the Econometric Society in 1937 but would have known about its activities from colleagues at the Austrian Business Cycle institute who were members, such as Oskar Morgenstern and Friedrich Hayek. Also the Hungarian-born banker Karl Schlesinger, who had directed Wald's interest toward the mathematics of general equilibrium, was a member. Wald was the kind of mathematician, who on being explained a problem, could immediately see through it to its inner mathematical structure and often from that provide a clue to how the problem could be resolved. Frisch had quite a bit of that ability himself and often "diagnosed" new acquaintances as whether they *really* knew mathematics, letting any doubt linger until it was completely resolved. No doubt lingered about Wald's mathematics.[20]

The contact between Frisch and Wald got established when Frisch wrote to Wald in June 1937 to comment upon Wald (1937) on price index problems. Frisch had read the 40-page article with 'extreme interest' and congratulated Wald on having 'made a remarkable contribution on the theory of index numbers.' Wald was his kind of a mathematician. In the letter Frisch expressed hope of meeting Wald at the forthcoming Econometric Society in Annecy in the middle of September as 'there are many further fascinating problems in this field which I would like to discuss with you.'[21] But Frisch and Wald did not meet at Annecy as

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Frisch cancelled his participation after having spent the best part of the summer of 1937 in Colorado Springs at Cowles' insistence (see Section 2). Shortly after the Annecy meeting, Tinbergen spent some days in Oslo and the topic of Wald came up. After his return to Geneva, Tinbergen wrote to Frisch that Wald had mentioned he would be happy if he could get something to do in the Cowles Commission. Frisch wrote right away to Cowles and proposed that he offered Wald a CC fellowship.[22] Cowles acted on it immediately and the offer was mediated to Wald by Tinbergen, not suppressing from whom the idea had originated. Wald wrote to Frisch in January 1938, thanked him sincerely, and stated that he was not sure he was able to accept the offer as he did not have the means for the travel expenses to Colorado Springs and even less for the return journey in case he did not find a position over there. He had written to Cowles, however, and asked for the offer to remain open until June (Wald to Frisch, January 16, 1938).

Frisch wrote back immediately to Wald with the suggestion that if Wald secured the necessary financial means for the travel to the U.S. he could leave via Oslo and give Frisch an opportunity to put him on the track of unsolved index number problems he was concerned with but had put aside due to other work.[23] Wald very much wanted to exploit this opportunity for discussing these issues with Frisch.

But it did not work out so smoothly. When Wald wrote again from Rumania at the end of June 1938 he referred to the *Ereignisse* (= events) in Vienna which had jeopardized his travel plans completely. The *Ereignisse* was the Anschluss in March 1938 and the uncertainty, violence, persecution, and Nazification that came with it. Wald had secured in Rumania all the travel documents he needed and was leaving the following day for Paris and onward to Cherbourg for boarding a French liner to New York. But the Oslo detour had really been on his mind. He had worked out that the first liner from Oslo he could possibly catch would arrive too late for him to be able to give his scheduled lecture on July 12 at the CC Research Conference, and furthermore, it would have been cumbersome to travel to Oslo as he was no longer eligible for a German transit visa.[24] Wald arrived as scheduled in Colorado Springs and initiated his fellowship. At the end of the summer Wald submitted two papers to *Econometrica*, both entirely within Frisch's field special field of interest. After some delay in Frisch's handling of queued up submissions they were published as Wald (1939a, 1940a).

Section 2 briefly reviews some aspects and events related to the history of the Cowles Commission before its move to Chicago in 1939, as complementary and additional to the account in Christ (1952, Ch. I–III). Section 3 on the Cowles Commission's move to Chicago introduces new material on the choice of Theodore O. Yntema as research director in 1939 and, particularly, on the choice of Jacob Marschak from 1943. The evolving relationship between the Cowles Commission and the University of Chicago is discussed briefly in Section 3 in connection with the move and for the ensuing years at the end of Section 5. Section 4 deals with the first year or so of the Marschak period as research director, when Marschak oriented his own thinking and research toward Haavelmo's probabilistic ideas, aired research program ideas, and had the choice of persons for the core CC research

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team incessantly on his mind. Section 5 sets out how the research team was gathered and kept reasonably intact. Section 6 is about the first milestone reached, the January 1945 conference. Section 7 brings Haavelmo's person into the picture at the end of 1945 and into 1946, the peak of the Cowles Commission's econometric endeavor. Section 8 has some brief notes on the dissolution of the team Marschak had compiled, and Section 9 concludes with some aftermath details.

Haavelmo has only a scanty appearance in the unfolding of events in Sections 3 to 6 but this apparently quiescent aspect of the Haavelmo-CC nexus is nonetheless buttressed by the overriding CC research agenda where Haavelmo's thinking and contributions as mediated by Marschak played a preeminent role. For more on Haavelmo's path toward completion of his fundamental works and his contact with Marschak and others are provided see Bjerkholt (2007, 2010).

2. THE COWLES COMMISSION 1932–39

The Cowles Commission had been founded in 1932 as an institution devoted to supporting the newly founded Econometric Society. Alfred Cowles 3rd, who for about ten years had run an investment advisory service in Colorado Springs, Colorado, had come to the conclusion that firms offering such services had failed to be of benefit to the investor in predicting stock prices. He heard about the Econometric Society and decided to contact its President, Professor Irving Fisher.[25] At a meeting in Fisher's spacious premises on Prospect Avenue near Yale University on October 18, 1931 with Charles Roos present Cowles stated that instead of conducting his counseling service on the basis of inadequate knowledge, he wanted to convert it into a nonprofit research foundation. He offered the Econometric Society a deal which appealed immediately to Fisher. Cowles subsequently put the offer in writing. In a letter written in Fisher's office Cowles stated that he wanted to establish a foundation for research 'guided very largely in its research policy by the advice which would naturally come to me from the Econometric Society', and further was prepared to 'make up any deficit' of the – not yet established – journal *Econometrica.* (Alfred Cowles 3rd to Irving Fisher, October 18, 1931.)[26]

The leading European members of the Econometric Society were somewhat less enthusiastic about Cowles' offer than Fisher but voted to accept it. Cowles' proposed name for the research foundation, *The Econometric Foundation*, did not win approval, however, from the European Council members; instead it became the *Cowles Commission*. In February 1932, the Council of the Econometric Society elected Cowles as Treasurer of the Society and also appointed him as the Circulation Manager of *Econometrica*. At the request of Cowles, the Council selected five persons to serve as an Advisory Council for the Cowles Commission: A.L. Bowley, Irving Fisher, Ragnar Frisch, Carl Snyder, and W.C. Mitchell.

In the beginning the small staff comprised William F.C. Nelson, an economist, and Forrest Danson, a statistician, who had both worked with Cowles in his investment service firm before 1932. Cowles hired Harold T. Davis, a professor of

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mathematics at Indiana University to be in charge of the statistical work on a part time basis. In lieu of a research director, Cowles engaged Ragnar Frisch as a non-resident research consultant. Frisch, who by then had been elected as Editor of Econometrica, came to Colorado Springs for the first time in June 1932 before the CC was incorporated. In the summer of 1934 he spent another long visit at the Cowles Commission, during which he proposed the Monograph series as the 'Cowles Commission Monographs in Econometrics'.[27]

A commendable but almost forgotten effort by the Cowles Commission was to provide textbooks for students interested in econometrics. This had also been Frisch's idea. Two volumes were planned under the common title *Introduction to the Statistical Theory of Econometrics*, to be written jointly by Nelson and Davis. The ambition of the first volume was later lowered to be a textbook in elementary statistics (Davis and Nelson, 1935) but then Nelson died at only 36 years of age in 1936.[28] Nelson had also served as the assistant editor of *Econometrica* since 1932.

In the early years of the Cowles Commission its most important activity was the annual Cowles Commission Research Conference, initiated in 1935, see Christ (1952, pp. 14–15). The conferences were drawn out events lasting 3–4 weeks mostly in July, allowing ample time for the participants to spend time together at 'hikes, drives and picnics in the inspiring mountains' (Christ, 1952). The attendance increased from year to year, comprising key senior figures in the Econometric Society such as Irving Fisher, Joseph Schumpeter, and Ragnar Frisch, famous European scholars, e.g., R.A. Fisher, René Roy, and Corrado Gini, as well as young, budding econometricians.

In 1935 Cowles invited the secretary of the Econometric Society, Charles F. Roos, to become the first research director. He served from September 1935 until January 1937.[29] When Roos stepped down little had been achieved toward making the Cowles Commission a versatile research organization. From 1938 Cowles was elected to succeed Roos as Secretary of the Econometric Society in addition to being Treasurer and Circulation Manager. The symbiotic relationship between the Econometric Society and the Cowles Commission became even more intertwined.

As soon as Roos had decided to leave, Cowles renewed Frisch's initial role as research consultant of the Commission. Frisch passed on to Tinbergen that Roos was about to leave as research director; he regarded Tinbergen as the best person he could think of as taking over that position. Tinbergen expressed positive but not immediate interest. He was already involved in the League of Nations study. Frisch then advised Cowles to install Davis as research director temporarily.[30] Cowles adhered to Frisch's advice and appointed Davis but Davis had got a teaching position at Northwestern and left his temporary directorship in August 1937.

Cowles had decided to embark on creating a new deal for the CC to lift the research activity out of semiobscurity, helped by external funding.[31] As it turned out this would not be successful in the short run but a logical first step was to find a new research director fitting that role. The CC Twenty-Year Research Report, published 1952, gave the following account:

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> 'The summer conference of 1937 was used partly as a recruiting ground for a new director of research, and among the prospects invited were Frisch; Jacob Marschak, then director of the Institute of Statistics at Oxford University; and Theodore O. Yntema, then a Professor in the School of Business at the University of Chicago. None of them was inclined to accept the position, however, for Colorado Springs was too isolated from the large academic centers to be attractive.' (Christ, 1952, p. 18.)

That is about all Christ (1952) stated on the effort to recruit a new research director apart from a follow-up remark that the disadvantage of the Cowles Commission's geographical position with led to 'failure to secure as director of research any of the three men who ranked as first choice when Roos departed in 1937.' (Christ, 1952, p. 18). But Christ got it mixed-up. Frisch was not a candidate for the research directorship. Cowles needed his assistance in selecting a research director, and from a wider set of candidates than those mentioned.

Marschak and Yntema were thus invited to the CC Research Conference in 1937 because they were among the persons considered as candidates for the research director position. Cowles' invitation to Marschak to the CC Research Conference came with an offer of $400 to cover expenses for this first visit of Marschak to the United States. It is not clear, however, who were involved in discussions about the selection of research director during the conference. One may wonder, e.g., whether Joseph Schumpeter's presence was due to an invitation to give advice on candidates.[32]

Frisch may have been more concerned with what kind of research the new Cowles Commission research director ought to initiate than with who would be offered the position. Half way into the Conference Frisch wrote a memo to Cowles and had it typed up as *Memorandum to Alfred Cowles 3rd from Ragnar Frisch Regarding a Research Project on Economic Control*. The opening lines of the 9-page note set the scene:

> 'In connection with the appointment of a new research director of the Cowles Commission for Research in Economics, the plans of the future work of the Commission should be considered. At the present moment there is only one great research project which the Commission pursues in a systematic way … namely, the analysis of the behaviour of stock prices. The various other researches which are going on at present … are all of a more special sort, undertaken individually and independently by the research fellows and other members of the staff. I consider it important for the future development of the activities of the Cowles Commission that a coordinated attack is made also on some other broader economic research problem. I propose that the Commission embark upon a research project on economic control. I shall give a brief outline of the scope of such a research and the reasons why I consider it of particular importance at the present moment' (Frisch, 1937, p. 1).

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Frisch made it clear that the Commission needed a research program more than a research director and thus the search for a research director should be guided by the kind of research that ought to be pursued by the Commission. And on that Frisch had deep insight, with advice also on what kind of research to avoid. The memo amounted to nothing less than a blueprint for the research at the Commission as shown in the excerpt quoted below:

> 'In discussing any measure which aims at influencing the economic system as a whole it is therefore necessary to discuss the economic system as a closed system. Expressed mathematically, it is necessary to consider a system of simultaneous equations where all of a number of variables influence each other simultaneously. The "one thing at a time" analysis is wholly inadequate for the discussion of this type of problem. (…) There is a crying need for a type of systematic economic analysis which has as its explicit purpose to explain the effects – direct and remote – that are to be expected if certain specified measures are taken that will change the original conditions of the economic system. This is what I would call the analysis of economic control. (…) With few exceptions the problems in economic control are quantitative. … Indeed, when the direct and the indirect consequences of a given measure are to be compared, one must be able, at least approximately, to assign a magnitude to the various responses; it is only through this comparison of magnitude that a discussion of the final result can be made …. it is impossible to carry through any conclusive analysis of such composite results without formulating the problems and the discussion in mathematical terms. Into this mathematical framework must be thrown all the resources of economic theory and of factual economic analysis. This means that the problems of economic control are essentially econometric. (…)
>
> I suggest that the work of the Commission within the field of control analysis be concentrated primarily on attempts at explaining the fundamental relations involved and at coordinating the available facts. This would mean that a considerable part of the work would have to be theoretical and mathematical. Only a comparatively small part of the effort should be directed towards the collecting of new facts pertaining to control. (…)
>
> Statistical methods of determining the nature of structural economic relations would have to be developed. I emphasize that the relations to be studied must be structural relations in distinction to time-shape relations. This distinction is fundamental for all attempts at econometric analysis. A structural relation is a relation whose character is not changed even if the character of some other relation is changed – in this sense it is autonomous. … From the point of view of control it is obviously the structural relations one needs to know.

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> Unfortunately, in many cases the statistical data present themselves in such a form that only time-shape relations – as determined by the several actually existing structural relations – can be discovered. In each case where an attempt is made to fit theoretical relations to available data a thorough scrutiny must therefore be made of whether the relations are really of a structural (autonomous) sort. … This is precisely where a person with a great technical ability in mathematics and statistics but with no economic understanding will be apt to make serious blunders.
>
> In those cases where available statistical data will not give the structural (autonomous) relations one is looking for, certain forms of experiments or inquiries may be attempted. … Whatever the shortcomings of such methods, they will in many cases be the only way out when the available statistics of what actually has happened are insufficient to determine the structural relations. (…)
>
> I suggest that an effort be made to study quantitatively the relations that exist between employment, the general level of wages, the purchasing power of the public, credit in circulation, interest rates, savings versus investments, the utilizations of the productive capacity of the nations, and a few other variables that are relevant in this connection. The limitation of this problem to a form where it becomes manageable should not be made by selecting one of these factors, say employment, and describing it in all detail. For control in the economy it is more important to consider the simultaneous interplay between all the factors listed, even though this in the first approximation will have to be done very roughly and on a macrocosmic basis, that is, by omitting many details regarding each individual factor and describing each factor by a composite index number. It is, of course, impossible to say a priori how far such an analysis can be pushed, but I consider it definitely worthwhile to make the attempt.' (Frisch, 1937, pp. 2–5 & 7–9.)

The memo shows that Frisch was way ahead of most of his fellow econometricians in his thinking about modeling and the use of models for economic policy. Key conceptual ideas, such as the emphasis on structural relations put together in simultaneous equation systems to capture interdependence, are often attributed to Haavelmo but came from Frisch.[33] Noteworthy is Frisch's emphasis on the analysis of economic control and the need to avoid purely political issues and vague research agendas. The research agenda laid out by Frisch goes far beyond work covered in the early postwar period. Remarks in the memo on the time forms of dynamic responses can be linked to ideas in Sims (1980). Also the call for experimental data to achieve identification is breathtaking (and was echoed in Haavelmo). Frisch's influence on Cowles' thinking in the early years of CC was

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considerable but largely missing in the historical accounts. Frisch ended the memo with a remark on the need for training:

> 'At the present moment there is a crying need for persons trained to tackle the complex problems that arise when economic control measures are to be studied, from the viewpoint of the economy at large. To be qualified for this task a person must … have a thorough background in general economic theory, a good background in mathematics, and … a considerable amount of experience in the handling of statistical data coupled with a good portion of common sense. Work within the Cowles Commission would seem to offer a splendid opportunity for the necessary training in the analysis of economic control if the work of the Commission were extended along the lines here suggested and directed by a first-rate econometrician.' (Frisch, 1937, p. 9.)

Cowles and Frisch continued for close to a year after the conference to discuss by correspondence candidates for the research directorship. There was also a price issue. The salary offered might vary from, say, $5,000–$8,000 for different candidates. The names considered during or after the conference were in addition to Tinbergen, Marschak, and Yntema, also R.G.D. Allen, Oskar N. Anderson, Oskar Lange, Ernest H. Phelps Brown, and Costantino Bresciani-Turroni; other names may also have been mentioned. Tinbergen and Anderson were both in the first group of ES Fellows elected by the Council in 1933. Allen, Marschak, and Bresciani-Turroni were three of the four ES Fellows added in 1935. Yntema, Lange, and Phelps Brown were not Fellows. Bresciani-Turroni had just been elected to the Council.[34] It can be noted there seemed to be a premium on Europeans for the position as CC research director.

But of these candidates only Marschak and Yntema were present at the 1937 conference. Cowles may have been inclined to choose Marschak, whom he knew that Frisch thought very highly of, while seeking Frisch's advice on Yntema. No conclusion was, however, drawn.

Oskar Anderson's name as a candidate was due to Van Sickle of the Rockefeller Foundation who in the early autumn of 1937 had brought it up in a meeting with Cowles about financial support. Van Sickle had gone out of his way to praise the German-Russian mathematician and statistician as a good choice for the research director position. Anderson was fifty years old and the oldest one of the candidates under consideration. As Cowles previously had heard indirectly through Tintner that Anderson would probably be available at a moderate salary he was interested also from a pecuniary point of view. Frisch considered Anderson as an excellent statistician, but not enough of an economist, at least from what he knew of his economic work.[35]

In November 1937 Tinbergen visited Oslo and Frisch solicited his opinion about Oskar Lange. Tinbergen thought highly of Lange and his articles. Frisch pursued the issue and Tinbergen more specifically stated that the lucidity of

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exposition in Lange's papers impressed him but with regard to powerful originality his opinion was more reserved. Frisch was skeptic toward Lange at the outset but revised his opinion after reading *The Rate of Interest and the Optimum Propensity to Consume* (Lange, 1938). Frisch was impressed with the good presentation of ideas in monetary theory that had been discussed in recent years and was prepared to move Lange higher up on the list of candidates but still decidedly below Tinbergen and Marschak. Cowles passed on to Frisch that Griffith Evans held strongly the opinion that Lange was ideally fitted for teaching and that it would be a mistake for him to undertake research instead.[36]

Frisch held R.G.D. Allen in very high regard. In October he informed Cowles that Allen had got a one-year Rockefeller fellowship and advised Cowles to get in touch with Allen and persuade him to spend time with the Commission, adding 'the more I have thought the matter over the more I have come to the conclusion that Allen is the man to consider as candidate for directorship taken everything into account.'[37]

Frisch's overall record for later years shows perhaps that he did not always hit the mark in personal assessments but he held sound opinions in 1937–38 as discussed above. He tentatively concluded to Cowles that he placed Allen, Marschak, and Tinbergen above the others. He also threw in the name of Phelps Brown but the proposal was not pursued.

Cowles concluded after having heard the opinions of Frisch and others that Marschak would be a good choice as research director. Some months after the conference Cowles sounded Marschak out about his interest and financial requirements. A salary of $8,000 was mentioned but shortly afterward Cowles informed Marschak by telegram explaining that his plan to secure a grant from Rockefeller Foundation had been unsuccessful and the tentative offer was withdrawn.[38] Cowles further told Marschak that

> 'I have been working on a plan to secure a permanent endowment for our organization, and, if this is successful, I shall be better able to provide an ample research director's salary with guarantees for the future. Furthermore, the unfavorable business developments of recent months in the United States make it seem unwise at this time to undertake an ambitious program of expansion. I have therefore made up my mind to get along for the present with a research director at a substantially lower salary than the one mentioned.' (Cowles to Marschak, April 20, 1938.)

Cowles then asked Frisch to check whether Bresciani-Turroni, an antifascist economist who had sought refuge in Egypt and was teaching at the University of Giza, might be interested at a salary of $5,000 or $6,000. If that was the case, Cowles would invite him with all costs paid for the forthcoming research conference to discuss it. Bresciani-Turroni was interested but the timing was inconvenient.[39]

With the benefit of hindsight we can note that Cowles in Marschak had indeed found a good choice for the research director position. The experience of being

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turned down by the Rockefeller Foundation and having to withdraw a stated offer to Marschak was embarrassing for Cowles. The CC had frankly so far not been much of a success as a research institution. It had had, as Frisch noted in his 1937 memo, only one proper research project, namely Cowles' own rather descriptive study of stock prices which resulted in CC Monograph 3 published in 1938 with a 2nd edition in 1939. Cowles wanted a position for the Commission with more leverage for generating additional financial support but how? CC's position within the Econometric Society was perhaps an asset that could be exploited.

It is not clear what kind of plan Cowles had been working on. According to Christ (1952) part of the plan was for the Cowles Commission to move to a university.[40] Colorado Springs was a great place for the CC research conferences, convenient, cheap, and with great natural surroundings, but in academic respects the location was a backwater. The search for a less costly research director continued a little while before Cowles decided to carry on without one.

There is some obscurity surrounding Marschak's own personal deliberations about the time he got the offer that was withdrawn. Frisch conveyed to Cowles his surprise about Marschak's interest in the research director position as he had just heard from a reliable source that Marschak had definitely decided to stay on in Oxford.[41] At the same time the Cowles offer to Marschak was withdrawn, Marschak applied by letter dated April 2, 1938 to Kittredge for a Rockefeller fellowship to visit the U.S. It was Marschak's absolute last chance to get the fellowship because of the age limit of 40 years. He got a fellowship for nine months and arrived in New York in November 1938, while his family remained in Oxford, with the stated intention of spending time at Columbia (Harold Hotelling), Berkeley (Griffith Evans), and Chicago (Henry Schultz).[42] He motivated his interest in such a visit as follows:

> 'A large proportion of my time here has been occupied by administrative work and I feel it would help both my own research and that done under my supervision if I were given the opportunity of concentrating my attention, for some time on the fundamentals of my subject and on the present state of its methods. Closer contact with American leading colleagues would be of very great value as many of the questions I have in mind, in fact the whole problem of quantitative methods applied to economic data, though clear in principle is far from being settled in practical details.' (Marschak to Kittredge, April 2, 1937.)

The details of where, when, and why Marschak decided to remain in the United States are not quite clear. The end of the nine month fellowship coincided with the outbreak of WWII and perhaps that was why Marschak decided to remain in the U.S. He sent for his family to come from Oxford. Soon after his arrival in the U.S. Marschak took part in the Econometric Society meeting in Detroit in December 1938 where he met with inter alia Hotelling, Neyman, Lange, Wald, Irving Fisher, Joseph Schumpeter, Paul Samuelson, Wassily Leontief, his former

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colleague from Germany Emil Lederer,[43] Chicago economists Paul Douglas and H. Gregg Lewis, former CC research directors Charles Roos and Harold T. Davis, and attended the memorial session for Henry Schultz.

3. MOVING THE COWLES COMMISSION TO THE UNIVERSITY OF CHICAGO

The death of Cowles' father in January 1939 forced the issue. Alfred Cowles 3rd had to move to Chicago to take care of the family's wealth and business interests. Cowles arranged to meet with President Robert Maynard Hutchins of the University of Chicago on the afternoon of February 6, 1939 about two weeks after the funeral.[44] Hutchins was a Yale man; he had been a high-profiled Dean of the Law School before he went to Chicago. He was younger than Cowles; they had not met at Yale. The meeting with Hutchins was like a replay of the meeting with Irving Fisher at Yale in October 1931 discussed in the opening paragraph of Section 2. As in 1931 Cowles had prepared an offer, communicated it persuasively to Hutchins face to face and immediately afterward put it in writing:[45]

> 'This is to confirm the understanding reached in our conversation of this afternoon. I propose to move the Cowles Commission for Research in Economics to the University of Chicago next fall or sooner if convenient. The commission's income for 1939 will be $27,000, and presumably a like amount for 1940. Our staff numbers nine people. It is my understanding that the University will pay approximately half of the total salaries of the research director and two research associates in return for part-time teaching assignments by these men and that they will be appointed to posts in the University of Chicago faculty. I also understand that the University will provide the members of the Commission staff with office space free of charge. The research director's salary is to be high enough to insure our securing one of the ablest men in the world in this field. The Commission will bring with it to the University of Chicago the offices of the Econometric Society, an international society for the advancement of economic theory in its relation to statistics and mathematics which numbers in its membership representatives of 42 different foreign countries. The editorial offices of the Society's quarterly journal, Econometrica, will also be located at the University. This understanding is, of course, subject to the approval of your Board of Trustees.' (Cowles to Hutchins, February 6, 1939.)

Cowles offered donations to be spent at but not by the university. In addition he offered the University of Chicago the Econometric Society connection that the CC and Cowles himself represented. The Commission was a research facility devoted to further the interests of the Econometric Society for which Cowles was

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both Treasurer and Secretary and in charge of the business and editorial office of Econometrica.[46]

Cowles returned to Colorado Springs after the meeting with Hutchins and received just one week later a message from Vice-President Emery T. Filbey that the Board of Trustees of the University of Chicago had approved the understanding outlined in Cowles' letter.[47] Cowles would get from the University the office space the Commission needed. Implied was that the CC would bring to the university some talented people, particularly the research director. All leading universities benefited from benefactors but it was not commonplace for benefactors to run their own units within the university. The symbiotic relation with the Econometric Society had by the agreement been extended to a symbiotic relation also with the University but this was not unproblematic. Although the Cowles offer was generally applauded in 1939 frictions arose later around the relations between the CC and the University (see below at the end of Section 5).

There were a few things that needed to be worked through before the Cowles Commission could move to Chicago on the terms agreed. Issues needed to be decided concerning the extent to which Cowles' human assets, the staff of nine persons mentioned in the letter, could be integrated into the university staff, and–more important–who would become research director.

The research director issue had to be settled first. It may from the ensuing events seem likely that this had at least been touched upon between Hutchins and Cowles at their first meeting, as, e.g., the choice of Theodore Yntema for the research director position was quickly decided. Yntema had been professor of statistics at the University's School of Business for almost ten years. He was able and experienced but a far cry from 'one of the ablest men in the world' as stated in Cowles' letter. There was a subtlety to this choice revealed in a letter from Hutchins to Cowles on Hutchins' 'investigation of personnel' in connection with CC's move:

> 'I should say at once that we purposely limited our investigation. We were afraid to ask the Economics Department whether they would like "a big man" from abroad, because we were sure they would say they would. We felt that it was in your interest and in the interest of the University to start with Yntema if he were interested and if we could be satisfied that he was worth a trial. We therefore talked with Yntema, being careful not to make any commitments, and with a few impartial people not directly concerned with the Department of Economics.
>
> We find that Yntema would like nothing better than an experimental run as research director of the Cowles Commission. I explained to him that you and the University might wish at the end of a year or two to use the directorship to attract a great man from outside the University. He said that title meant nothing to him and that if we decided to appoint another man to the position later on he would be entirely satisfied.(…)

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> My present view, subject to correction, is that it would be wise to start with Yntema. I believe that he is seriously interested in the project and that he will develop into a first-class leader for it. At the same time, this is much more your show than mine, and if you feel differently I hope you will say so candidly. One of the attractions about the Yntema suggestion is that his appointment would not prevent the selection of a great man at a later date and that it would give us time to decide which great man we wanted.' (Hutchins to Cowles, February 27, 1939.)

We may understand this reasoning as motivated by a need to smooth the way for the CC into the university. The need for 'a big man from abroad' was fully recognized but to choose one as the first act in Chicago carried its own risk of somehow getting off track. Perhaps Hutchins' wide experience led him to advise an unconventional but safe route. Cowles wrote back two days later expressing his appreciation of Yntema's interest in the position, and the advantage of choosing 'a native of the United States and well-acquainted with the members of your economics department.' He added that 'the appointment would not be as irrevocable as would be the case were we to persuade some European to forego a promising career in his native land.'[48]

Cowles offered immediately the position as research director to Yntema who accepted as he had told Hutchins he would. Thus in distinction from what happened in 1937–38 Cowles refrained from searching for the best research director, although in 1939 he had the financial side very well covered.[49]

With regard to the two research associates to be jointly financed by Cowles and the University, Oskar Lange was in the picture from the start. Lange had been hired by the University of Chicago as assistant professor from July 1938. Cowles wanted Francis McIntyre who came with him from Colorado Springs for the other position.[50] Hutchins could tell Cowles shortly afterward that Lange had been offered a full professorship at the University of California, countered by a Chicago offer of associate professorship and a higher salary. The prospect of losing Lange was bad news for Cowles. He told Hutchins that Lange was 'one of the most learned mathematical economists in the United States' and that 'it would be worth your while to do everything possible to persuade him to stay in Chicago' (Cowles to Hutchins, March 1, 1939). Lange decided to stay.

Vice-President Filbey did the homework for Hutchins and found that the deal proposed by Cowles, assuming Yntema, Lange, and McIntyre would fill the positions as research director and research associates, implied for the university 'six more units of teaching than we previously had, together with a budget saving of $2,652. This looks like a good bargain.' (Filbey to Hutchins, March 3, 1939.) When Cowles visited Chicago in the middle of March, he had further talks with Filbey and used the goodwill he had earned in an effort to solve some remaining personnel problems. Harold T. Davis, part-time mathematician and consultant at Cowles Commission since 1932, was professor of mathematics at Northwestern

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since 1937.[51] Cowles wanted a position for Davis. Cowles also had two research fellows, Horst Mendershausen and Abraham Wald. Mendershausen decided without further ado to remain in Colorado Springs. Filbey offered Davis and Wald to the Department of Mathematics. Filbey described to the mathematics chair Wald as a 'displaced German Jew... a distinguished mathematician of international reputation. Wald is looking for a teaching position, or for anything he can find to do that can keep him alive. Cowles has him on a stipend of $1.000' (Filbey to Gilbert A. Bliss, March 24, 1939). It was Wald at bargain price but Chicago let it pass.[52] Shortly afterward Wald was offered a lectureship in statistics at Columbia. As Chicago showed no interest in Davis, he stayed on at Northwestern (where he incidentally was T.W. Anderson's teacher). Cowles continued to pay him $1000 a year as a CC research consultant.

Cowles considered in the spring of 1939 to move the upcoming Cowles Commission Research Conferences to be held in Chicago but it turned out that it would be quite a bit more expensive than in Colorado Springs. As a result both the Fifth in 1939 and the Sixth (and last) conference in 1940 were held in Colorado Springs.

The Cowles Commission moved in September 1939 from the 3rd floor of the Mining Exchange Building in downtown Colorado Springs to the University of Chicago's Hyde Park Campus. The Commission had been allotted (rent-free!) a suite of offices on the top (4th) floor of the Social Science Research Building at the edge of the University quadrangles overlooking the Midway. In addition to Lange, others with teaching positions at Chicago were given appointments as Cowles Commission (part-time) research associates: H. Gregg Lewis, Jacob Mosak (both at the Department of Economics), and Joel Dean (School of Business).

The new location and the understanding reached between Hutchins and Cowles opened new possibilities. The Commission's own financial resources sufficed only for basic activities and selected salaries. The size and ambition of the operation that the CC could muster depended upon the provision of outside financial sources, but ultimately on the quality of the research associates and the auxiliary staff. The question of whether teaching opportunities, or even tenured positions, could be acquired for new staff was a factor of considerable importance. In the Colorado Springs period the Commission had after all achieved rather limited results in terms of published research toward its ambition of supporting the econometric program, although it could be credited for other achievements.[53]

Cowles was attracted by the possibilities for external funding to be vastly improved by being associated with a big university, but perhaps without fully taking into account that this advantage also would imply constraints. Within the hierarchy of the University of Chicago, the Cowles Commission affairs were placed under the Dean of the Social Sciences Division, Robert Redfield, Professor of Anthropology. The Division comprised the Department of Economics chaired by Simeon Elbridge Leland, Professor of Government Finance. Redfield had been on leave in early 1939 when Cowles negotiated his deal. On his return Dean Redfield set out to clarify the relation between the Division and the Commission with

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regard to access to Rockefeller funds. In a memo to Filbey Redfield stated his view in no uncertain terms:

> 'I think it would unwise for the University to submit to the Foundation more than one request for funds covering the same kind of general program of work. It seems to me that the work of the Cowles Commission is in part research in fundamental economics, and in part more special and immediately practical investigation relating to the conduct of particular businesses. I think the former work of the Commission is identical with research done in this Division and that a request for new financing of this work should be made as a part of the request for new funds to be made by the Division. To this end the Social Science Research Committee of this Division will submit its memorandum under preparation to Mr. Yntema and will consult with him to the end that a single request for funds will cover the program of the Division including that work, done by members of the Commission, that is of the same fundamental and theoretical nature. I do not think the Commission should make independent appeals for funds to the Foundation.' (Redfield to Filbey, May 3, 1940.)

Redfield's advice was two-sided. The Cowles Commission could get grants from the Rockefeller Foundation only when applied for via the Social Science Research Committee of the University but the Social Sciences Division would pay due attention to the needs of the Commission in its application to the Foundation. The integration of a number of CC staff in the Department of Economics would be helpful in this regard. But the University's reins over the Commission would be tightened later.

In September 1942 Yntema submitted his resignation. Was he nudged to do so? He was eased out of the research director's chair as prearranged and perhaps with no wish to continue in it. He became, instead, research director of the University's Committee of Economic Development. Yntema was research director when United States entered into WWII and in the wake of that Yntema got involved in war work and was much absent from the Commission. His research directorship had not fulfilled all hopes and aspirations.

Yntema's resignation led immediately to an effort to find a successor, with minimal involvement by Cowles. Simeon Leland was operational in compiling names to be considered in close contact with Redfield and Dean W.H. Spencer of the School of Business. Names were proposed in an open discussion in the Department of Economics. For more proposals Leland contacted Ralph Young, University of Pennsylvania who canvassed personnel of the NBER for suggestions and also Jacob Viner who agreed to discuss the vacancy with others.[54] Leland wrote at the beginning of October 1942 a first report of the situation to Hutchins:

> 'The Department is interested in securing the services of an outstanding person as Director of the Cowles Commission. In strict confidence, it is my opinion that unless such an appointment is made, the

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> work of the Cowles Commission may not be maintained to the point where it will interest the Cowles family or the Foundation and we may shortly have a "white elephant" on our hands. I am not altogether sure that the arrangements in the last few years have been happy ones for the Cowles group. On the other hand, I am quite certain that it has not been satisfactory to the University as a whole. If the right sort of an appointment is made, I think the Cowles Commission may become a distinct asset to the Department and the University.
>
> When the subject of a new Director was discussed with the Department, two lines of development were suggested: (1) an outstanding economist, interested in mathematical economics and statistics, to supplement our group could be appointed; (2) it was also suggested that a person who was predominately a statistician or mathematician, with somewhat less interest in economics, might be equally advantageous to the University. It was pointed out that at the present time we do not have in the business-economics group an outstanding statistician. Lange's interests seem to fall more in the field of mathematical economics and theory than in statistics and mathematics. Yntema, who is proficient in the field, is apparently more interested in the study of "applied" economic problems. Such an appointment could bring the Institute of Statistics and the Cowles Commission into closer relationship and from the University's point of view, this might be advantageous. In any case, it was our thought that the new Director should be a good administrator interested in full-time research for the Cowles Commission' (Leland to Hutchins, October 1, 1942).

Leland's letter comprised the names that had so far been thrown in. Yntema had proposed Jacob Marschak and Paul Samuelson, while the open department discussion had resulted in the following list: 'Allen Wallis, Milton Friedman, Corwin D. Edwards, Robert R. Nathan, W.J. Carson, Durand (son of E. Dana Durand), George W. Terborgh, Arthur F. Burns, and Abraham Wald' (Leland to Hutchins, October 1, 1942). John U. Nef who had absent from the department meeting submitted Colin Clark, while Dean Spencer suggested Sidney W. Wilcox, Joel P. Dean, and Neil H. Jacoby.[55]

Leland's letter comprised a significant remark about Arthur Burns:

> 'When the name of Arthur Burns was suggested, it was pointed out that his relationship with the National Bureau of Economic Research and with Willits had been such as to indicate at least the possibility of considering him very seriously. At the present time, he is engaged in helping Wesley Mitchell finish some work for the Bureau and gossip informs us that Columbia is interested in seeing whether or not it should bid for Lange as the coming theorist or place the mantle on

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> Burns. It occurred to me that it might be advantageous for us seriously to consider scooping Columbia (for, I think, it is fairly certain that Lange will return at the end of his period of observation). Burns is a most competent economist, a good statistician, and, I have been told, has a lot of administrative ability. Willits might be induced to take even more interest in the Cowles Commission if Burns were here.'[56]

Some of the names conveyed by Leland belonged to the Chicago faculty. But Leland was 'not inclined to favor the appointment of anyone on the staff here to this position, with the possible exception of Neil Jacoby. He would do an admirable job, but to do it, he should relinquish his position as Secretary of the University, where he is now doing an admirable service. Part-time devoted to either enterprise would not be satisfactory' (Leland to Hutchins, October 1, 1942).

Leland's top five choice comprised Burns, Terborgh, Edwards, Marschak, and Samuelson (in that order).[57] On Samuelson, Leland added that he did not know about his administrative ability. But Leland nurtured doubts, he added that 'upon making further inquiry, I may desire to change the above ranking' (Leland to Hutchins, October 1, 1942).

This was only the beginning of a frenetic naming game in a process that had to be rushed through quickly. After two weeks Leland had compiled more names. In a note dated October 13, Leland passed on to Hutchins that Yntema had added no less than five names; Tjalling Koopmans, Gottfried Haberler, Eugen Altschul, Wassily Leontief, and John H. Smith, while Ralph Young submitted James F. Dewhurst, Solomon Fabricant, Albert G. Hart, Frederick C. Mills, and Donald S. Thompson. Two days later Leland reported that Frank Knight had added Simon Kuznets, Lloyd W. Mints and Charles O. Hardy and Leland himself George J. Stigler (who was then working for SRG at Columbia).

On the same day, Hutchins called a meeting to try to hammer out a decision. Leland, Dean Redfield, and Vice-President Filbey were present and perhaps no one else. All the 28 names compiled were put on a list for those present. Hutchins kept the lists when they left. Some names seem to have been struck off the list by agreement during the meeting, while others were given cryptic annotations.[58] The deliberations in Hutchins' office concluded with a shortlist of four names: Haberler, Marschak, Burns, and Terborgh and the conclusion that advice on the shortlist should be sought from Alvin H. Hansen and Oskar Lange. Leland wrote to them the same night. Both Hansen and Lange returned well-reasoned arguments.

Hansen ranked Haberler and Marschak as 'distinctly superior to the other two.' Burns was dismissed as having 'severe limitations from the standpoint of economic analysis,' although 'doubtless a competent statistician.' Terborgh was found to have 'severe limitations from the standpoint of broad economic training and analysis' and 'definitely' ranked lower than Burns. Haberler (Hansen's colleague at Harvard) and Marschak were found 'evenly balanced, though my own preference would be for Haberler.' Both were found very highly qualified

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on abilities they both had. 'Marschak is possibly the better mathematician and statistician, and Haberler is perhaps to be preferred with respect to equipment in theoretical analysis.' He concluded that 'either one would be an extremely good choice', adding that Burns would not be a bad choice either but definitely inferior to Haberler and Marschak. He downplayed the importance of Haberler's bad hearing but doubted that Haberler would consider leaving Cambridge. (Hansen to Leland, October 21, 1942.)

Lange stated that he had not known that Yntema had relinquished the directorship until he got Leland's letter. He ranked the shortlisted names as (1) Marschak, (2) Haberler, (3) Burns, and (4) Terborgh. As Lange's evaluation may well seem to have clinched the matter it is well worth quoting at some length:

> 'I put Marschak in the first place on the list because I think he would be in a way the ideal man to direct an institution of the type of the Cowles Commission. Marschak is one of the most outstanding economic theorists, mathematical economists and econometricians in the world and is also a highly competent mathematical as well a practical statistician. His contributions to the econometric study of demand have become by now historic, just like those of Henry Schultz. He is one of the outstanding mathematical economists, and as a general economic theorist his contributions have been of a wide variety: price theory, capital and interest, monetary theory and policy. Of all the candidates proposed he alone has full competence in the field of modern mathematical statistics (in this field he exceeds Yntema) and has also done original work in problems of migration. In addition he has had the experience of having been the director (and organizer) of the Institute of Statistics of the University of Oxford, which institute has developed under his direction into one of the leading institutes of economic research in the world (vide the Oxford Economic Papers). His personal character and tactfulness make it extremely easy to work with him.
>
> Haberler, too, is one of the leading economists of today though, in my opinion, he does not reach up to the level of Marschak on account of the greater specialization of his work and of the lack of command over modern mathematical and statistical tools. Haberler is one of the leading authorities on the theory of international trade and on monetary theory, but (except for an excellent study on index numbers) his interests were pretty much confined to this field. I think he would be an excellent acquisition to our department, but as a lecturer rather than a director of the Cowles Commission. For he is not a mathematical economist, nor an econometrician, nor has he any knowledge of mathematical statistics. I do not doubt that under his directorship the Cowles Commission would do an excellent job of research, but the nature of the research would necessarily have to be different from

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> that which was conceived when the Commission was founded. The research would not be econometric or based on applications of mathematics to economic problems but rather general economic research which does not require such specific tools of analysis as those which have become the tradition of the Cowles Commission. It would be research of the type like, say, that of the Brookings Institution, though certainly on a much higher level of competence. I think also that Haberler could be obtained with greater difficulty and at much more expensive terms than Marschak.... Marschak would be quite ready as far as I know, to leave the New School of Social Research.' (Lange to Leland, October 1[?], 1942.)[59]

Did Lange lay it on too thickly for Marschak? When stating that Haberler was 'not a mathematical economist, nor an econometrician, nor [with] any knowledge of mathematical statistics', while Marschak was characterized as 'one of the most outstanding... mathematical economists and econometricians in the world' and the only candidate with 'full competence in the field of modern mathematical statistics' he had virtually clinched it for Marschak.[60]

Lange also had clear and specific observations on Arthur Burns, characterizing him at the outset as an excellent practical statistician 'of the crude empirical variety (like Kuznets)':

> 'I would not classify him as a mathematical statistician (of the type of Hotelling, Bartky, or even Marschak), nor do I think he is much of an economic theorist. He has, though, an excellent mind (probably a better mind than Haberler) but is rather untrained in theoretical economic analysis, largely due to an "institutionalist" contempt for such analysis imparted to him by W.C. Mitchell. I am just reading a manuscript of his and Mitchell's on business cycles which is rather disappointing: a collection of thousands of data which are mechanically averaged etc. without even an attempt to distinguish between basic and secondary facts, all this done quite deliberately in the name of "unprejudiced empiricism."[61] It seems, however, that the responsibility for this characteristic of his research is Mitchell's rather than his own. I am sure that under his directorship the Cowles Commission would, too, do a very good job. But the nature of the job would again differ from that for which the Commission was originally designed. It would not be econometric analysis, but rather pure fact finding research, something like a smaller edition of the National Bureau. In case of a choice between Haberler and Burns I should prefer Haberler, who, although much less of a statistician than Burns, has much more analytic maturity as an economist that Burns has.' (Lange to Leland, October 1[?], 1942.)[62]

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Together with the letters from Hansen and Lange, Leland submitted seven more names to Hutchins on October 24, 1942.[63] But there is no indication that these were taken into consideration. Lange's unequivocal statement in favor of Marschak was supported by Leland, despite the latter's different ranking at an early stage. When the matter was discussed with Cowles he was also in favor of Marschak and the matter was thus virtually closed.[64] Cowles' support was of course not surprising as he had selected Marschak already in 1937/38 from a strong line-up of candidates.[65]

We may never know whether Cowles would have insisted on Marschak if the advice that had come down from Hutchins had pointed to another candidate. Cowles knew, unlike some of the other participants in the palaver, that no other short-listed candidate than Marschak was able and likely to fill the position as research director to serve the purposes for which–in Lange's words–'the Commission was originally designed'.

Marschak passed by the University of Chicago on November 6, 1942. A lunch was arranged at the Quadrangle Club for Marschak to meet with a larger group, followed by a meeting with Hutchins, Redfield, Leland, and Cowles. As Lange had hinted at, Marschak was prepared to leave the New School at short notice to take over his new responsibility as research director. Around the middle of November 1942 Marschak received confirmation that he was the research director of the Cowles Commission from January 1, 1943.

There is one missing name in the discussion above, namely Oskar Lange himself. Lange was mentioned in Leland's October 1 note to Hutchins as quoted above but did not figure on the list of 28 names. It is hard to think of any other explanation for this than that he must have made it clear at an early stage that he was not a candidate. One reason for him to stay out of the race might be that he wanted to pave the way for Marschak, whom he knew would make a better research director. Lange's candidacy would, however, have run counter to Leland's advice of avoiding Chicago faculty, although currently on leave from Chicago.[66]

Another afterthought on the process was that the kind of research program to be pursued by the CC from 1943 was kept largely outside the discussion (although figuring prominently in Lange's evaluation). The entire discussion was in terms of names, not heeding Frisch's 1937 advice which few (or none?) of those who threw in names had seen.

There is some evidence of Marschak's idea of a research program at the time he became research director. It was stated in a letter to Jacob Mosak, who had been Henry Schultz' assistant and became CC research associate in 1939 but on leave since 1941 to work at the Office for Price Analysis. Marschak wrote to get Mosak back to CC to take over a price control study comprising field studies by questionnaires among producers and distributors of consumer goods in the Chicago Area 1942–44. This was thus Marschak's very first action as research director, albeit before he was instated. In the letter Marschak vented his lack of enthusiasm for the price control project and then set out a number of other projects 'of a more "econometric" type' which ought to become central in the Commission's

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program. In the hope of enticing Mosak to join him in Chicago he suggested they should work together on a list of research priorities satisfying their own research interests at the same time as it would become (part of) a research program for the Commission. Mosak declined the invitation.[67]

Marschak listed seven research topics, of which nos. 1–5 were all spinoffs from a huge consumption demand study Marschak had been working on in New York with George Garvy. The topics comprised completion of a yet uncompleted study of the demand for meat and total food, an extension to include the demand for new houses, interrelations of demand for large budget items, and to apply the results to a study of the 'econometrics of taxation'.

Topic 6, 'Dynamics of Income-Distribution', dealt with the question of how individual incomes are affected when the aggregate income changes and was a pre-requisite for demand studies 'because, to use national income as an independent variable in the demand equation, we need assumptions as to past income-distributions. Provisionally, the simple assumption that the Lorenz curve is unchanged, seems good enough for a first approximation… The subject is, however, important in its own right.'[68]

Finally, topic 7, although brief, pointed toward the future: 'study of probabilities in economics: questions of uncertainty, risk, valuation of assets, etc., in light of statistical inference theory combined with equilibrium theory'. Topic 7 had the central keywords for much of Marschak's future research but he did not bother to elaborate to Mosak. Marschak might already have reached the conclusion that the 'study of probabilities in economics' as expounded by Haavelmo was a priority in the CC research program.

4. MARSCHAK IN CHICAGO. THE FIRST STEPS

The war made its mark also on the Cowles Commission. According to the CC Report for 1942 'half of the members of the research staff had been drawn into public service or other work essential to the war effort'… while 'the financial resources of the Commission and the energies of the remaining staff members have been devoted, insofar as possible, to a study of price controls and rationing in order to appraise these means of maintaining stability in our war economy.' The staff had dwindled. When Marschak arrived all the research associates had vanished, mainly into war work or teaching. Oskar Lange was on leave at Columbia until July 1943 and expected to return.

Leonid Hurwicz had arrived to the U.S. in August 1940. The first and only job offer he got was from Paul Samuelson, to work as his assistant at MIT. In Cambridge he got acquainted with Haavelmo and commented the almost complete draft of Haavelmo (1941). At the end of 1941 he moved to Chicago where Yntema took Hurwicz on as research associate to work with him from the beginning of 1942 on the price control project alluded to in the exchange with Mosak above. When Marschak arrived Hurwicz had left the project and got affiliated with the Institute of Meteorology. But he retained his status as research associate

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so he had in fact not vanished like the others. This was fortunate for Marschak who ranked Hurwicz very highly.[69]

The price control project had been initiated by Yntema in 1942, planned as a survey of price control and rationing methods to be conducted jointly with NBER and financed by the Rockefeller Foundation. Yntema had been in charge with Leo Hurwicz as second-in-command. Thus as Yntema and Hurwicz abandoned the large price control study, Marschak was forced to take over the responsibility for it with a marked lack of enthusiasm. In the end the Hungarian-born experimental psychologist George Katona was secured to take over the administrative direction of the price control study under Marschak's supervision.[70]

Marschak's primary tasks were to build a team and to map out a research program. He was not under pressure to do this very quickly. He had brought his own research project on consumption demand analysis with him from New York. There he had for some years worked with George Garvy as his half-time assistant on the project which seemed to grow to unwieldy proportions. He considered bringing Garvy to Chicago but that did not work out. The consumption project continued but gradually subsided without much, if anything, published. Marschak may have lost interest at some stage.

But Haavelmo was definitely on Marschak's mind after he took over the research director's office. Marschak knew like several others that Haavelmo had no other wish than to return to Norway as soon as the war was over. When Haavelmo's Rockefeller Fellowship expired at the end of 1941 Marschak had been instrumental in arranging for a position for Haavelmo at the New School for Social Research. Haavelmo had accepted the offer and started to plan his lectures and also some joint research with Marschak when he was requested by the Norwegian government to take an assignment in New York.[71] Thus, from February 1942 Haavelmo worked as a statistician and analyst for Nortraship, a forced war time merger of the large Norwegian commercial fleet.[72]

In New York Haavelmo kept in touch with Marschak and others in the econometric circuit, including Abraham Girshick. In the spring of 1942 Haavelmo conceived the idea for the simultaneous equations paper (Haavelmo, 1943a) and worked out the details while convalescing after surgery. Marschak visited him in the hospital and recalled many years later the memory of a brief talk in the hospital:

> 'It must have been brief because one is not supposed to prolong the visit to a patient. You were in a hospital in New York, recovering from an operation. We talked about "simultaneous equations." It was the first time that the stochastic view became clear to me. It has not ceased to plague me ever since, in various aspects and guises.' (Marschak to Haavelmo, December 8, 1958.)

Shortly afterward Haavelmo sent Marschak the original typescript for the simultaneous equations paper together with a note on an assertion by Abe Girshick that a differential equations set-up for demand functions would be a more

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general formulation. Marschak returned the manuscript at the end of June suggesting more elaboration in the direction of tentative guidance for practical applications.[73] Twice in 1942, in February and December, Haavelmo presented his ideas at the "Marschak seminar" in New York.

But although Haavelmo was not within reach for recruitment to the CC team in 1943, Marschak was highly preoccupied with Haavelmo's econometric ideas, and perhaps nurtured a faint hope of somehow getting Haavelmo to Chicago at a later stage. In the spring of 1943, Marschak studied Haavelmo (1943a) thoroughly, partly assisted by Leo Hurwicz. That led directly to an intensive exchange by letter with Haavelmo in May–July 1943 to ensure Marschak's full understanding of the content and implications of Haavelmo's pathbreaking paper.[74]

Marschak's early study of Haavelmo led to a new idea for his own research: econometric production studies, perhaps motivated by a need to check out the practicability of Haavelmo's ideas on a well-known problem. He hired William H. Andrews as research associate from the second half of the 1943 to work with him.[75] A new research program of the Commission gradually took shape in Marschak's mind.

While the exchange with Marschak about Haavelmo (1943a) took place and perhaps spurred on by it Haavelmo started to re-edit and extend Haavelmo (1941). He had received quite a bit of positive feedback on the essay which had not been distributed very widely. Hence, he was interested in the possibility of getting it published in some way. He made a limited effort, reshuffled the sections in a more logical order, deleted some paragraphs, added a chapter on problems of prediction, and stuck on a conclusion, see Bjerkholt (2007, p. 211). In early June 1943, he gave the only copy of the manuscript to Oskar Lange, the acting editor of Econometrica, to read. Lange returned the manuscript at the end of June with the remark that he had read it with utmost interest and 'the book should be published by all means.'[76] When Lange came back to Chicago in the middle of June he showed the manuscript to Marschak, who immediately took steps to have it published as a CC Monograph. The monograph series was, however, not for outside authors. Marschak got around this obstacle by having Haavelmo appointed research associate from July 1943.[77] Then it became apparent that the manuscript was a bit on the short side to make a monograph on its own. As we shall see Marschak found a way around that too.

At this time, in June 1943 Marschak made his first recruitment move (disregarding the lukewarm effort to get Mosak back). It was for Abraham Wald who had advanced from instructor to assistant professor at Columbia. At this time Wald had been involved in high priority war work. Hotelling had brought him into the Statistical Research Group (SRG). He worked there part-time, thus combined with teaching and other research. According to Wallis (1980a, p. 324) Wald worked 30 months for SRG, which fits with mid-1943 until the end of 1945.[78] Marschak knew Wald well, but hardly much about the nature of his war work. Marschak must have realized the chances were slim. He wrote to Wald and pretended to ask

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his advice, laying out to Wald that '… solid econometric work requires division of labor and cooperation between three or four types of people: (1) economic theorist, (2) mathematician, (3) an expert in finding and critically interpreting factual sources, (4) a computing manager.' He added that 'the work of mathematician would have to bear both on the statistical side of the problem and on economic theory' (Marschak to Wald, June 17, 1943).

Marschak's real errand was to find out whether there was a possibility of getting Abraham Wald to the CC. Marschak seemed almost hesitant in asking directly: 'It is hardly necessary to tell you that the best solution would be to have you as the occupier of the mathematician's position. However, it may be presumption on my part to call upon you without any knowledge of your own plans and possibilities; nor do I know at all whether Chicago will be able to offer the necessary incentives' (Marschak to Wald, June 17, 1943). Instead he asked for Wald's advice on any suitable younger candidates for such a position, reiterating the point that the possibilities for financing would depend on quality of the person secured. At the end he added some further remarks to whet Wald's interest.

Wald rose to the bait, responding by stating the good prospects he had at Columbia in his current situation but still expressed an interest in exploring the possibilities for cooperation with Marschak at CC. Marschak reiterated the need for a mathematician of Wald's stature: 'I shall maintain a very strong stand defending the view that it is necessary for the success of the work of the Cowles Commission to have a professional mathematician on its staff' (Marschak to Wald, July 2, 1943). He also went into action to explore the possibilities for a position for Wald at the Department of Mathematics at the University. The key person to address was Walter Bartky, an astronomer, who was in physical sciences, but more important, chairing a committee entrusted with the co-ordination of statistical teaching at the University.[79] At the same time Marschak briefed Cowles for taking the matter up with President Hutchins.

Marschak was interested in Wald as the best statistician he could possibly hope to get and with deep insight in economics as well, and the interest was reinforced by the role that Wald had played in the development of Haavelmo's ideas. Wald's crucial assistance in the completion of Haavelmo (1941) during the weeks spent in Maine was followed up by Wald in 1942 when he initiated work on a paper with Henry B. Mann on what would appear as Mann and Wald (1943), an important underpinning for the methods suggested by Haavelmo and thus for the work that would evolve at CC in the ensuing years.[80]

After Marschak had raised the issue of a position in Chicago, Wald made some inquiries and wrote back to Marschak with a tentative assessment of the Department of Mathematics at Chicago. Apart from the very gifted Abraham A. Albert, who was not interested in anything except algebra, the other members of the department were hardly first rate mathematicians.[81] But that did not close the matter as far as Wald was concerned. If Chicago would seriously consider upgrading mathematical statistics and Marschak could work something out with Bartky, Wald would be interested.

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A brief insight into what else was on Marschak's mind at this time is provided by a note he wrote on his own current work agenda. It comprised two projects, titled '280 Demand Elasticity' and '281 Production function ("Douglas")', each with three subprojects. One of the subprojects under 'Demand Elasticity' had the following entry:

> Joint maximum likelihood estimation of parameters of demand and supply. This was done by [Dickson H. Leavens] using formulas in which production cost was disregarded. Formulas which take cost into account have been developed by Andrews and will have to be used as soon as index of meat production cost is constructed. (Marschak, 'Work in hand', August 1943.)

An additional remark noted that the two projects were linked by method. The same method, i.e., maximum likelihood, if applied to the equations of the firm 'yields the "improved Douglas function".' Marschak figured that 280 ought to result in a book on demand analysis, while an appropriate form for publishing 281 might be an article, and that might be suitable for the maximum likelihood estimation part of 280 as well.[82]

Marschak had from Europe considerable experience relevant for research directorship. As director of the Oxford Institute of Statistics he had sought Frisch's advice on how to set up a laboratory for numerical work. As a research director his style of supervising his staff has been characterized as follows: 'Marschak did not impose direction; he provided vision and drive' (Arrow, 1991, p. 139). This was combined with a choosy attitude in the selection of staff to work with. Only the best was good enough.

Marschak and Tjalling Koopmans knew each other from Europe. They had both been at the Cambridge conference to discuss Tinbergen's work in July 1938. After two years as lecturer at Netherlands School of Economics in Rotterdam between 1936 and 1938 Koopmans had worked with Tinbergen at the Economic section of the League of Nations in Geneva from the end of 1938 and taken over Tinbergen's job in 1939. His assignment was to make a model of the UK economy. After the outbreak of WWII the entire Economic Section of the League of Nations moved from Geneva to Princeton. After a brief stint as research associate at Princeton, Koopmans began in 1941 to work for an insurance company and was then drawn into war work as statistician to the Combined Shipping Adjustment Board in Washington D.C. This matched with his earlier interests in shipping. He retained academic contacts, including occasional attendance at the Marschak seminar in New York, and published in *Econometrica*, *Annals of Mathematical Statistics*, and other journals.

In the summer of 1943 Tjalling Koopmans wrote to Marschak and set out his wish list for what to do when the war was over. There was only one item on the list and it could be called 'redoing Tinbergen':

> 'What I would like to do most of all is to continue with the elaboration and clarification of the ideas of theoretical-empirical business

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> cycle analysis started by Tinbergen. A few years ago I made a little outline of what I felt are the directions in which further work is most needed. I think Tinbergen's study for the U.S. should be deepened and extended with the help of the data that have since become available, covering where possible the whole period from 1919 to date. But before any great amount of collection of data and calculation of regressions is done, I feel very strongly that one or two years' work on the exploration and exposition of the method itself should be done. In Tinbergen's investigation there was, and could be, little equalization of the marginal cost of fitting equations in various parts of the system to the marginal utility of the equations obtained in the system, as a whole. … A good deal of work should be done on the working hypothesis that Tinbergen's equations are true. This work should show where we can cut down on equations, and where in the system the vital coefficients and lags are located. It should also elucidate the dynamical consequences of the various coefficients and lags found to be vital. It should finally explore the cumulative effects of the "random disturbances" as manifested in the "residuals" from regression equations, which have not found a place in Tinbergen's equations.' (Koopmans to Marschak, August 22, 1943.)

He added, as an afterthought, perhaps with Keynes' criticism in mind: 'The total utility of the research product depends on how accessible it is to the intelligent and interested mind. Intensive rather than extensive work is needed to show what it is all about. I don't think we ourselves, including Tinbergen, have a clear picture of the implications of the equation system as it stands'.

Koopmans strong concern with Tinbergen's model may be read as encompassing an element of hurt pride on behalf of the econometricians after Keynes' sharply worded criticism of Tinbergen. Marschak and Lange had attempted to give an appropriate scientific response to Keynes' somewhat arrogantly formulated dismissal and submitted it to the *Economic Journal*, only to have their paper rejected by Keynes.[83] Koopmans had given his comment in the *Journal of Political Economy* (Koopmans, 1941), while Haavelmo gave his in a festschrift to Schumpeter (Haavelmo, 1943b). But Koopmans may have felt stronger about this matter than Marschak, Lange, and Haavelmo. Tinbergen had been his mentor on the path from physics via statistics to econometrics and he had taken over Tinbergen's mantle at the League of Nations as the chief investigator of the explanation of business cycles. Marschak and Lange had been aware of weaknesses in Tinbergen's work that Keynes had not commented upon.[84] Then there was Frisch's criticism of Tinbergen which few had seen and even fewer understood.[85]

The 'outline' enclosed with Koopmans' letter had been formulated in 1940, i.e., while Koopmans still worked for the League of Nations but at Princeton.[86] It elaborated methodological issues, all considered as preparatory tasks for the research program Koopmans drew up for himself as what he wished 'to work

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on to the extent of opportunity and ability', but where? NBER had its mind all set 'but unfortunately in another direction'. Koopmans could not resist asking: 'Would Chicago be a possibility?'[87]

Marschak had experience with the Rockefeller Foundation both from his Oxford institute and from the New School. He responded to Koopmans by first explaining the ground rules. Rockefeller Foundation was interested in developing scientific economics and building institutions but reluctant to consider grants to individuals. On the other hand, Koopmans' ideas for his own research fitted reasonably well into the current plans which the Foundation knew from the past intercourse. (Marschak to Koopmans, September 9, 1943.)

Thus in the letter Marschak offered Koopmans employment as research associate on the following conditions: (A) $2,500 per annum would be paid to Koopmans as a permanent member of its staff; (B) as soon as possible the Cowles Commission would work out, with Koopmans' cooperation, the program and budget for a research project, preferably for two years, to be submitted … to the Rockefeller Foundation. Koopmans' remuneration would then also comprise a chunk of the Rockefeller grant. This was to be decided before the final budget was submitted. A new program could not start earlier than in July 1944 as that was the expiration date for current grants. But Marschak also noted 'differences of emphasis' between his own thinking about a research program and the ideas put forth by Koopmans. He set out his own view:

> 'Our work here has originated as studies of the behavior of consumers and producers, with a view on certain practical applications. Beginning with the demand analysis for single commodities, (modifying Schultz' approach in various ways), and with the revision of Douglas' "production function", it is inevitably becoming more "macrodynamic", in two respects: 1) we have become interested in large groups of commodities (food, rent, durables, savings) rather than in single commodities, 2) we propose to estimate the parameters involved in the system of the relevant equations – a point emphasized by [Haavelmo 1943a].' (Marschak to Koopmans, September 9, 1943.)

Marschak then elaborated on the 'differences of emphasis' in four points. The first point was the practical orientation of the CC research: 'we may continue to devote considerable attention to the parameters of particular equations because of their practical importance', e.g., applying Haavelmo's method to a system of equations describing the joint supply of milk and beef and the demand for each of these two commodities, and while Koopmans' note suggested that he would like to concentrate on the damping ratio of the macrosystem as a whole, 'we should hope for your assistance in these more "detailed" problems as well'. The second point was aggregation or index numbers 'which you do not regard as urgent now. But for our current work the development of this point is of great importance' (Marschak to Koopmans, September 9, 1943).

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Marschak's third point referred to what Koopmans' 1940 outline note said about 'logical foundations' and 'statistical dynamic equations' and continued: 'new developments have taken place since 1940; of these, [Koopmans 1942] was one; others are, for example [Haavelmo 1943a] and a new manuscript which Mann and Wald were urged to write for *Econometrica* (October, 1943), and which seems to push the matter still forward, though only for large samples. In the light of this, you may wish to revise the formulation of the program.' Marschak added that the same applied to what Koopmans had stated about testing of residuals. (Marschak to Koopmans, September 9, 1943.)[88]

Marschak's fourth point was related to Koopmans' obsession with Tinbergen's model: 'In constructing a medium-size dynamic system we may start with Tinbergen's large one, or with some rudimentary one. You seem to feel more committed to the former as we would be. The fact that Tinbergen has already invested much empirical work does not weigh very much, considering that further 9 years can and should now be added to his 13 years.' Marschak smoothed out the differences by a remark at the end that 'these are details that cannot preclude our co-operation'. (Marschak to Koopmans, September 9, 1943.)[89]

On Marschak's mind was also the still inextinguished hope of getting Wald to Chicago. Before Marschak actually sent his answer to Koopmans he provided Wald with a copy of the conditions he had offered Koopmans with the remark that Wald could consider himself 'as having an "option" on the opening in question. I do hope you will read the letter carefully, regarding the offer it contains as an offer directed to yourself.' Marschak made one modification in the offer, however, as he told Wald that the basic salary from the Commission would be $3,500. (Marschak to Koopmans, September 9, 1943.)

Marschak asked for a quick response from Wald, which came by cable immediately and unequivocally: 'SORRY BUT CANNOT CONSIDER OFFER WITHOUT PROVISIONS CONCERNING STATUS ON FACULTY' (Western Union telegram, September 11, 1943). Marschak knew Wald well enough to expect a reaction as dismissive as he got. He knew what counted for Wald but he could not offer it. He had tried to sweeten the pill by confirming that Cowles had studied Wald's record and was well prepared to talk with Hutchins about the whole matter of reorganizing the teaching of mathematical statistics at the University of Chicago and bring up Wald's name in that context. But Cowles was biding his time, waiting for the right moment to approach Hutchins.

Hence the ball passed to Koopmans who accepted Marschak's offer without hesitation. He came to Chicago for 2–3 days in the middle of December 1943 and gave a CC seminar on 'Dynamic Economic Systems.'[90] In meeting with Marschak Koopmans suggested the recruitment of Richard L. Anderson whom he knew from Princeton. Anderson was currently at North Carolina State College working under Gertrude Cox. Back in Washington Koopmans drafted in longhand a letter for Marschak to send to Anderson. The letter conveyed the intention of developing in the course of time, a center for statistical and econometric studies: 'With Koopmans and, possibly, A. Wald joining us, and Walter Bartky,

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Leonid Hurwicz, and Oscar Lange on the staff, the prospects for serious work are good. If you are interested too, please let me know.' The letter further announced that CC was engaged in a 'research program entitled *Studies in economic behavior and business fluctuations* of which the ultimate objective is to construct a system of equations that describes and explains business fluctuations in the United States in the inter-war period'.[91]

The program title mentioned in the letter must have been agreed, at least implicitly, between Marschak and Koopmans, but the program title was soon abandoned and cannot be found in print in Commission documents. The program title, as indeed also Koopmans' letter to Marschak in August 1943, clearly put a (Tinbergen type) model at the very center of the research program. This was still half a year before Koopmans joined Marschak in Chicago.

The letter to Anderson went on to say that before the main task could be undertaken certain preparatory studies were planned under three headings:

(1) Mathematical economic theory of dynamic equation systems.
(2) Statistical methods relating to time series.
(3) Numerical work, based on the best available data, with the object to find the pivotal equations, lags, and coefficients, which require most accurate statistical estimates.

(Marschak to R.L. Anderson, December 28, 1943.)

Koopmans had told Marschak that his earlier work under (2) had led him to believe that further results could be obtained under that heading if a determined effort was made. He had in mind certain procedures that generalized his previous methods. Koopmans was anxious, however, to spend the greater part of his time under the headings (1) and (3) and suggested that Anderson was invited to work, under Koopmans' direction, on the problems under (2). These were further outlined in a 2-page note, enclosed with the letter to Anderson.[92] Anderson did not respond until April, 1944. He had got leave of absence from N.C. State College to work at Princeton and ruled out the possibility of participating in the CC project, particularly as Koopmans had said it would require a complete break, but still expressed interest in it.[93] It might have made a difference if Wald had been in Chicago.

Koopmans and Marschak arranged to meet when Marschak visited Washington in April 1944. Koopmans later recalled that they had sat together on a bench in Lafayette Park and discussed Haavelmo (1944), which was not yet published (Koopmans to Marschak, May 21, 1944). Perhaps it was on this Lafayette Park bench, impacted by R.L. Anderson's negative response, that the program was reoriented. Koopmans may reasonably have drawn the conclusion that he would have to take the responsibility for point (2) in the letter to Anderson himself. That would turn the 'preparatory studies' of the statistical estimation problems into the major part of the program, rather than a macro model, unless more resources were forthcoming. The Lafayette Park bench chat may also have had a share in

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this as Koopmans learned from Marschak, who at the time was likely to have studied Haavelmo more thoroughly than anyone else, more about the deep insight of Haavelmo (1943a, 1944) and the challenges that followed from that.

While Haavelmo was out of reach to be recruited into Marschak's team, Wald had not been given up. After he made the first approach to Wald in June 1943, Marschak had got hold of the galley proof of Mann and Wald (1943) and started immediately an exchange with Wald to ascertain that he had understood everything properly and also raising several critical questions about the analysis, just as he had done with Haavelmo about the simultaneous equations article in the spring. He had also got hold of Haavelmo's manuscript for the revision of Haavelmo (1941) and conceived the idea of publishing Haavelmo's essay and the forthcoming Mann and Wald (1943) together as a Cowles Commission Monograph.

Marschak saw the chance to get a really path-breaking work as the first monograph published in his directorship. He wanted the two pieces of the monograph to appear as reasonably integrated and attractive to readers. He asked the three authors to make cross references, give examples, etc. that would be helpful to readers, and provide lists of other relevant articles by the authors. The authors had, however, little capacity to accommodate such changes and resisted Marschak's demands.

Marschak drafted an announcement notice for the monograph. He entitled it *The Theory of Economic Measurements* by Haavelmo, Mann, and Wald. Haavelmo's part was titled *The Stochastic Analysis of Economic Relations*. Haavelmo did not like the title for his part, and reacted to 'stochastic' which he found referred to the object of an analysis rather than to the method: 'I would like the title of my part to express the fact that my analysis deals not only with methods of statistical analysis in economics but also with the foundations for this, namely the probabilistic formulation of economic models'. He listed two alternative titles and left the final decision to Marschak:

(1) *The Probability Approach in the Theory and Measurement of Economic Relations*
(2) *The Probability Approach in Econometrics*[94]

Marschak decided on (2), although with some hesitation. But did he make the best choice? Haavelmo's reasoning as quoted above seems to point explicitly toward (1), probability approach in measurement and in theory. The short name would have been the same in both cases.[95] Haavelmo also found the name for the volume too pretentious and suggested adding 'Contributions to … ' in front.[96] Marschak concurred, although the additional two words could not be fitted on the spine.

The fact that Mann and Wald (1943) was about to be published in *Econometrica* made it easier to get a monograph out quickly but Marschak's nice plan collapsed, however, due to wartime regulations. The Cowles Commission did not have the paper quota required (Bjerkholt, 2007, p. 814). *Econometrica* had excess paper quota as the July issue each year had been canceled during the war.

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Hence, Haavelmo (1944) eventually appeared as the July issue of *Econometrica* 1944.

Hence, Marschak's correspondence with Wald about a position in Chicago was interspersed with long letters on econometric and statistical reasoning in Mann and Wald (1944), and other letters again on the preparation of the joint monograph. At one point during the exchange Wald asked about Koopmans. Marschak told him that Koopmans had indeed accepted but took care adding, 'this does not detract from the desirability of your co-operation in the future if and when satisfactory provisions are secured (Marschak to Wald, September 27, 1943).

In December 1943, Marschak could convey confidentially to Wald that new developments were finally under way at 'to create a position for the teaching of and research in mathematical statistics, with especial view to centralizing in competent hands the advice on the application of mathematical statistics in the various fields. … Naturally, we are mentioning your name, while trying to avoid the impression that "a job is being made for a man"' (Marschak to Wald, December 6, 1943). Marschak was, however, worried about Hotelling's reaction. The promotion of mathematical statistics within universities had been Hotelling's idea more than anyone else's.[97] But how would Hotelling react if Wald left for Chicago? Wald responded briefly, reiterating his interest, and dismissed the idea of any misgivings on Hotelling's part.

Then, at the very end of the year Marschak came back to Wald with more definite news:

> 'You probably will be invited one of these days to come to Chicago to get acquainted and to discuss the proposals which will be made to you. The Departments of Mathematics and of Economics have agreed that an associate professorship in mathematical statistics be created. If you accept such a position, your connection with the Department of Mathematics will be both in teaching and in research; while the connection with the Department of Economics will be entirely in research (unless you yourself desire to give occasionally or to participate in a course for economists).' (Marschak to Wald, December 27, 1943.)

Marschak stated that it was up to Wald to take the lead in development similar to that which had taken place at Columbia. He also intimated that chances for a future promotion to full professorship were good. Marschak's behind-the-scene effort seemed to have been successful. Cowles followed suit by writing to President Hutchins in the middle of January 1944:

> 'It is my understanding that the University of Chicago does not at present have in its faculty an outstanding scholar in mathematical statistics, a field in which great advances have been made in recent years, particularly because of the need in modern genetic and agricultural experiments. Still further work in mathematical statistics is

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> required in order to serve the needs of economics and the other social sciences. . . . The men associated with me in the particular branch of economics represented by the Cowles Commission for Research in Economics and the Econometric Society consider that it would be of tremendous advantage to them to have the opportunity for consultation with a man of professor Wald's particular qualifications. I, therefore, hope very much that you will find it possible to approve the plan which has been worked out for bringing him here.' (Cowles to Hutchins, January 14, 1944.)

One week later, Marschak sent Wald the course catalogue for the University with markers placed for the courses in statistics. He also conveyed on behalf of Bartky an invitation for Wald to give an informal talk to the Mathematics Club on February 1. Wald chose to speak on 'recent developments in the mathematical theory of statistical tests.' During the brief visit Wald met with university officials about details of the offer and also had a chance to talk with Marschak about the plans for the Cowles Commission. Wald departed and Marschak felt relieved although Wald had not definitely committed himself. On the train back to New York Wald decided to accept the offer, as he told Marschak afterward.

But then the blow came. One week later Wald turned down the generous offer from Chicago. He gave Marschak full details. Before Wald left for Chicago he had checked with Frederick C. Mills and got assurances that no promotion could be expected for the coming academic year. On his return he learnt immediately, also from Mills, that the University authorities had already made recommendations for promotion to associate professor from July 1, 1944. To Vice-President Foley, Wald laid it on thick: '. . . since my leaving Columbia would have disturbed the teaching program of statistics, and since my colleague Harold Hotelling, to whom I am very much indebted, expressed his desire for me to stay here, I committed myself to remain at Columbia provided that the board of Trustees approved my promotion' (Wald to Filbey, February 8, 1944, attached with Wald to Marschak, February 8, 1944). Hence, Marschak's attempt to recruit Wald to Chicago and CC had failed. But the effort Marschak went to is a significant observation about Marschak's concern with topnotch recruitment.

Wald continued to assure Marschak that he really wanted the prospect of close collaboration with him and with the Cowles Commission: 'The fact that I finally decided in favor of Columbia does not detract in the least from my great appreciation for the opportunities and prospects of interesting work I would have had at Chicago,' (Wald to Marschak, February 27, 1944). Marschak treated Wald's vacillation with great patience and understanding. He seemed to disregard the possibility that Wald was driving a hard bargain; Marschak's attitude seemed more to reflect that he understood that such a decision situation was hard for Wald to handle.[98]

Wald was apologetic about the whole thing and sorry about having misled and disappointed Marschak. Without being asked he offered his second best, advice on

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other statisticians that might be of interest: 'The group of well qualified mathematical statisticians is rather small. Among the younger men, Wolfowitz is undoubtedly one of the ablest and most promising. … Another young man is Dr. Mann. He is an excellent mathematician with good potentialities to become an outstanding statistician.' Wolfowitz and Mann were Wald's two favorite coauthors. None of them showed any interest in working with the Cowles Commission, although they remained in the picture for a while. Then he added at the end: 'I hope that in spite of my not coming to Chicago we can keep in touch with each other and that I shall have some opportunity in the future to collaborate with you in problems of mutual interest.' (Wald to Marschak, February 27, 1944.) Marschak took advantage of this offer by asking Wald for assessments again and again.

But despite this defeat, Marschak did not give up on getting Wald a tenured position in Chicago. He wrote back to Wald that he had made a very good impression in the Department of Economics as a mathematician and as a teacher. Nobody had taken offense in Wald's decline of the offer and it was a general opinion that Chicago might reopen the negotiation with Wald on a new basis (assuming that he was not tied to Columbia for more than a year). It had been said, though, that "to bargain with Columbia backward and forward would be awkward." On the other hand, if Wald dropped out of the picture Chicago was all set on starting negotiations with other statisticians (Marschak to Wald, March 2, 1944).

In his letter of February 8, 1944 Wald had expressed interest in giving a summer course in Chicago. However, there were too few students around for that. But the suggestion had given Marschak another idea:

> '… would you consider coming to Chicago for a couple of weeks to meet Koopmans, possibly Haavelmo, Anderson, Hurwicz, Lange and myself, and to discuss with us questions of time series and simultaneous stochastic equations. This would not be a formal conference, but a series of informal discussions of a small group at wide intervals (of say 2, 3 days) between the sessions, to give you and the others the opportunity to concentrate on each question quietly. We shall thus be able to review the present situation in the field somewhat systematically, and to get a good start for further work' (Marschak to Wald, March 2, 1944).

Marschak thus wanted an open, unstructured discussion by the whole team, in the style, say, of the CC Research Conferences, to 'get a good start for further work.' He offered to remunerate Wald as for a summer course.

Wald responded mainly on the prospects of further negotiations for a position. He had specific conditions he insisted upon, e.g., that it had to be a full professorship. He also fully acknowledged the bargaining situation: 'Would the University of Chicago be willing to reopen the negotiations even if an improvement of the conditions by Columbia which, in turn, may lead to the possibility of my staying here? … While I can fully understand the desire of the University of Chicago to avoid "bargaining with Columbia backward and forward", it does not seem to me

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quite fair that I should confront Columbia with accomplished facts.' Marschak merely passed Wald's conditions on to Bartky and to Simeon Leland, who was chair of the Department of Economics, and got back from Bartky that he doubted very much that the Department of Mathematics would make a recommendation 'along the lines proposed by Mr. Wald.'[99]

5. PUTTING A TEAM TOGETHER

Leo Hurwicz moved back and forth between the Institute of Meteorology and the Commission and worked on similar ideas in both places. He worked on several papers related to stochastic time series in close contact with Marschak. Three of the five papers he eventually contributed to Monograph 10 (see below) were initiated while working for the Institute of Meteorology. Then Hurwicz applied for and got a position at Iowa State College at Ames from the second half of 1944. This was a threatening loss to Marschak who had known Hurwicz since 1942 and rated his intellectual power extremely highly.

Marschak wrote to Hurwicz in June 1944 to tell him that research problems for the Commission for the next years, 'will be very much concerned with problems of statistical estimation in economics and especially with time series.' This would open up for participation of Hurwicz even from a remote location if he could work out an agreement with his superiors in Ames that he was allowed to do some duties for the Cowles Commission, mainly (1) read and criticize work prepared by staff members, (2) continue his own work and present it for discussion, and (3) attend regular conferences at CC.[100]

After half a year at Ames, Hurwicz applied for a Guggenheim Fellowship for 1945/46. He got the fellowship which allowed him to spend one year at the Commission with leave of absence from Ames. Marschak's recommendation of Hurwicz for the fellowship is noteworthy, as excerpted below:

> 'He is without any doubt one of the most creative and brilliant men of his generation in his field. His particular gift is that of quickly discovering the essentials of a problem, piercing through the cloud of verbiage, side-issues, lazy-minded vagueness and well-meant or malicious superstition which still surrounds most of economic discussion. His favorite words, to start a sentence in discussion are, "Essentially, the problem boils down to this," and what follows is, in most cases, essential indeed.' (Note by Marschak for the Guggenheim Foundation, undated but early 1945.)

Koopmans arrived on July 5, 1944. Soon after Koopmans' arrival he and Marschak must have decided to have as a target point a small conference in early 1945 to present and discuss papers related to part 2 of the research program. The papers could then be published as a CC Monograph. It would become the much-cited Monograph 10. The specific time for the conference was not decided.

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Coinciding with Koopmans' arrival Herman Rubin was hired as a research assistant to replace a predecessor who had been drafted into the U.S. military. Rubin teamed up with Koopmans to work on what eventually became the much cited (and very long) chapter 2 in Monograph 10.

As noted above, Koopmans had shifted from modeling to statistical methods. But his interest in modeling and policy was still there. Soon after his arrival he wrote a note to himself (with a copy for Marschak) titled 'Notes on a Long-Term Program,' where he stated that to construct and statistically test a complete equation system for the U.S. 1919–41 would take an estimated 4 to 5 years. He listed some specific issues of economic policy that ought to be considered and extensions of the modeling frame into international economics to allow the analysis of five problems in international economics. One of them was the following: 'Business cycle policy of a small country, which attempts to diminish the effect of fluctuations that it cannot influence, and of a big country which discounts the effect on other countries of the voluntary and involuntary fluctuations in its system' ('Notes on Long-Term Program', Koopmans August 10, 1944.)

Shortly after the Econometric Society meeting in Cleveland in September (see below) Koopmans sent Haavelmo his paper, later published as Koopmans (1945), and an incomplete draft of another paper meant for the January 1945 CC conference.[101] Koopmans was particularly concerned with, 'how far a statistical determination of demand equation for a commodity that represents only a small item on the consumers' budget is affected by the existence of simultaneous macroeconomic relations.' Haavelmo answered at some length:

> 'I would like to make a general remark as to the necessity of operating with "the total system" if one is only interested in estimating one or a few particular equations.
>
> Once one has set up a determinate system, including a sufficient specification of the distribution properties of the random elements involved, then this system can be considered as a composite hypothesis Ω which one accepts without test, the hypothesis to be tested, or the unknown thing to be estimated, being a particular set of values of the (unknown) parameters involved. What I have tried to point out in the article you refer to is that once one has accepted Ω as a basis one should not use such methods of estimation (or of testing hypotheses) which – in order to be correct – require that Ω is in some respects false. Because, nothing good, I believe, can come out of logical inconsistency. What one can not prove, however, is that, if a man tries to estimate a particular demand equation (say) by the single equation l.s. method, his result will necessarily be bad. In fact, it might be excellent, because the "true", but unknown, Ω for all the related economic variables might in fact be such that this particular method of fitting would be the correct one.

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> I think, however, that one could safely make the following statement: if the economist goes on fitting the various types of economic relations one by one by the single equation l.s. method, making in each case the assumptions necessary to justify his method, his will either, sooner or later, come to a point where his assumptions will have to be incompatible with assumptions previously made, or he will have to make assumptions that get more and more awkward, concerning the variables that are still "at liberty" in the total system.' (Haavelmo to Koopmans, November 7, 1944.)

Herman Rubin was the prodigy at Cowles. He studied mathematics at the University of Chicago after receiving his high school diploma from when he was 16 years old. As Rubin was hired at only 17 years of age, he was too young to be drafted (Bock, 2004, p. 409). According to the annual CC report for 1944 he was concerned with time series in a single variable, extending the proof of Mann and Wald (1943) to a simple 'unstable' system, and also proved that two methods of approximating the sampling distribution of the serial correlation coefficient were identical. Marschak and Andrews (1944), which must have been submitted soon after Rubin arrived, credited Rubin for several proofs, quite a feat for a 17 year old student! Rubin completed within the next few months both bachelor and master degrees in pure mathematics.

Rubin got, however, called up for U.S. Selective Service, Draft Board 12 in Chicago on December 15, 1944. After he passed the physical examination he could be inducted into military service at short notice. The Cowles Commission went into action. In the short run it was important that Rubin could continue to work up to and during the January conference, that is for 10 $\frac{1}{2}$ weeks after the physical. Marschak took action in advance: '… the contribution which Mr. Rubin can be expected to make during this time toward the solution of our problems, and thus to the welfare of the community, will be large' (Marschak to U.S. Selective Service, Draft Board 12, December 19, 1944). It worked; Rubin was not inducted until March 1945. But that was only the beginning, CC needed Rubin beyond the conference.

While Rubin was starting basic training at Fort Knox, Marschak wrote to W. Edwards Deming in the Bureau of the Budget about Rubin's exceptional qualities. Deming acted at the time on behalf of the Institute of Mathematical Statistics as an advisor to the Armed Forces on questions of personnel qualified to work in mathematical statistics. Deming passed the information on the right person in the War Department. Marschak then wrote to S.S. Wilks who was well connected and took the matter up with several Army research groups. But it was to no avail as the information came back from the War Department via Deming that it was 'extremely difficult to do anything about transferring [Rubin] to another job … it is one of those imperfections that have not yet been straightened out.'[102]

Rubin applied some months later for discharge in view of the illness of his parents whom he had to support. Marschak wrote to Rubin's commanding officer

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arguing that '[Rubin's] special work was concerned with the mathematical foundations of statistical research in application to non-experimental data … of great importance for the solutions of problems of business cycles and unemployment, and therefore also for the economic adjustment after the war.'[103] Rubin was discharged at the end of the year, resumed his duties as part-time research assistant at the beginning of 1946, i.e., when Haavelmo arrived, and became research associate in November, 1946.

Lawrence R. Klein might also be called a prodigy although not quite as young as Rubin.[104] Marschak met Klein for the first time during the Econometric Society meeting in Cleveland September 13–15, 1944, the first such meeting since before Pearl Harbor. Marschak had by then studied and appreciated Klein (1943) in Econometrica, the first paper drawing on Haavelmo's work. At Cleveland, Klein presented a paper titled 'From the Treatise to the General Theory: A Study in Keynesian Economics'.[105] Marschak chaired the session.[106] The Commission staff had a strong presence at the conference as Marschak spoke on 'Random Production Equations', Koopmans on 'Statistical Estimation of Simultaneous Economic Relations', and Hurwicz on 'Aspects of the Theory of Economic Fluctuations.'

At the end of the Cleveland meeting, Klein had dinner with the CC crowd and Marschak asked him to consider a job at the Cowles Commission as part of the team he was assembling around Koopmans and possibly Hurwicz. He also told Klein that he hoped Haavelmo would soon join the team. Marschak set out to Klein the division of the various tasks. Klein was to estimate the model; Hurwicz would work on the economic theory, and statistical method would be in the hands of Haavelmo and Koopmans.[107]

Klein had not really started to shop around for a position yet but promised to respond quickly. He wrote to Marschak in the middle of October, expressing his interest for what was going on at CC and adding: 'I do not like the idea of taking a temporary job whose continuance depends upon winning of a fellowship and which then is completed after one year.' He indicated that Paul Samuelson, his doctoral adviser, might be able to help him get something more permanent and also that his conscience kept telling him that he should take a war job in mathematical statistics. Marschak must have realized that he was about to risk losing a person of exceptional value and reacted accordingly. He wrote to Samuelson and explained what it was all about:

> '… we are working here on 1) statistical methods applicable to simultaneous stochastic equations involving time lags and 2) applications of these methods to economic models, both for single markets and for the economy as a whole.
>
> The particular gap which Klein would be expected to fill is primarily in the field of economics rather than in statistical theory. We need a first class man who would go over the economic literature on business cycles to get from it suggestions for various "dynamic models," to be formulated mathematically. In doing this he will have to keep an eye

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> on the available statistical sources to find ways of reformulating the hypotheses in such a way as to make them manageable without losing too much in accuracy. … we are at present somewhat top heavy on the statistical side and need an economist.' (Marschak to Samuelson, October 25, 1944.)

One might wonder here whether 'top heavy' implied a hopeful reference to Wald, albeit the letter was written at a time when Wald seemed lost to Marschak. Marschak explained to Samuelson that Klein had underestimated the duration of tenure as the offer was two years' salary and three years employment if Klein could also get a fellowship. He asked Samuelson for his opinion of Klein by asking the following question: 'Would you regard him (as I am inclined to do) as one of the best men of his age available for econometrics work of the type described?'

Samuelson was full of praise: '… very promising, able, young economist with an excellent training. To a surprising degree he has been able to go ahead on his own steam in these disorganizing years', and after commenting on Klein's papers, added, 'He is certainly the best student that I have had, and I have known very few better ones.' Marschak wrote soon after to Klein with the offer of three years employment (conditional on Klein getting a one-year SSRC fellowship, see below). The letter also stated Klein's assignment: 'We expect your particular contribution in the coming year to be in the field of the critical surveying and construction of economic "models"; though there is naturally a close connection between the economic and the statistical aspects, and your participation in discussion on all the aspects will be welcome.'[108]

Klein accepted Marschak's offer against a job in the Fed or staying in Cambridge, Mass. He joined the small group as research associate on November 21, 1944 and started to work on individual equations and the theory of aggregation, latter was deemed very important in constructing a macro model based on micro theory.

Klein had met Haavelmo earlier, when he came to M.I.T. to give a lecture at the Statistics Seminar in the spring of 1943 (Klein, 1991). In their first conversation Haavelmo explained to Klein the OLS bias in estimation of single equations from a system. Haavelmo (1943a) had just been published. After arriving in Chicago Klein realized that it did not seem likely after all that Haavelmo would join the team. But when in 1946 Haavelmo did come after all, it was for Klein like icing on the cake.

According to the 1944 annual CC report Klein's work within the group was 'to scrutinize hypotheses and arrange data to be submitted to the statistical tests and measurements'. This was the beginning of the Klein models of economic fluctuations, eventually published as Klein (1950). At this stage the small team was roughly divided into a 'statistical' and an 'economic' team. As the annual report put it: '… the teams interchange ideas constantly, to prevent each other from working in a vacuum. … The statistical problem is being shaped to meet the

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most urgent requirements of the economists; while the economists are kept from indulging in purism or in unmanageable and unverifiable detail'.

Klein applied for a Social Science Research Council (SSRC) postdoctoral research training fellowship.[109] This was an entirely external resource and, if successful, would relieve the Commission from finding other sources of financial support for Klein. Marschak was selected as the confidential evaluator of Klein's qualifications, including rating him in comparison with other Ph.D.'s not over 35 years of age with whose work the evaluator was familiar. Was he among the top 2–3 percent, the top 6–7 percent or lower? Marschak answered scrupulously: 'It is difficult to choose between the first two ratings. … But then, again, I would have to correct for an unfair bias favoring the younger people and due to the improvement of tools that has taken place in the meantime. (The inventor of the cart wheel was probably a greater genius than the inventor of the motor car). Since I do not know whether or not other reporters have the same statistical scruples, I have assigned Klein the highest ratings without being sure that he is actually the best.'

Marschak gave lucid and sharply observed characterizations of Klein, as in the following excerpts:

> 'Before I knew him better, a certain impetuosity of his style in writing and speaking made me expect from him an excess of self-confidence (not unusual in young people of his ability) if not partisanship. On nearer acquaintance this has not been confirmed: my collaborators and myself have found in him a person prepared to understand and appreciate the other point of view; equally agreeable in giving and in taking; and more interested in having the problem solved than in winning the argument or making a career.
>
> He has a good eye for the essential. His goal is a logically consistent explanation of observed facts. He will not try to escape into theoretical perfectionism (which tends to make economics logically complete and beautiful but unverifiable) or into empirical detail (substituting enumeration for explanation). The kind of study proposed by Klein requires a sound instinct … If this instinct fails, the hypotheses subjected to verification are either so incomplete as to lead to biased conclusions; or so pretentiously complete as to be unverifiable by the facts at our disposal. Klein seems to possess the necessary instinct: it will save him much disappointment.
>
> Klein seems to be the type of man who will work overnight and over the weekend if the problem interests him.' (SSRC report by J. Marschak on L.R. Klein, February 1945.)

The training fellowship was relatively poorly paid and it is kind of embarrassing to note in retrospect that a brilliant talent was treated so miserly. Marschak's remark on 'partisanship' hinted at Klein's political radicalism within a group that overall was left leaning.[110] The friction on this point was between

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Koopmans and Klein. It came to the fore in the autumn of 1946 when Klein drafted a note titled "Marxian Theory of Effective Demand" which was an attempt to discuss how to apply econometric methods to a Marxian model. Koopmans disliked the idea but reacted out of proportions when he wrote a strongly worded three-page note to rebut it:

> 'His paper is an attempt to sell the idea of econometric model building to adherents of Marxian economic doctrine. . . . I believe that such attempts, including the present one, are harmful to the understanding of the objective of econometric model building. (. . .) This leads to my strongest objection to the paper: Our approach is not to develop equations systems expressing the ideas of specific authors and quoting them right and left to demonstrate our faithfulness. The form of our equation system is determined by a balance between simplicity, approximation to reality, and kind of policy question that is to be answered. . . . I therefore recommend that the Cowles Commission abandon the attempt to present an equations system as derived from Marxian economics.' (Koopmans to Marschak, December 10, 1946.)

Klein later re-edited the paper and published it as part of an article which also dealt with Keynesian theory (Klein, 1947).

In 1944 the CC research program had been re-titled to become *Quantitative studies of economic behavior*. This was perhaps too general. After Klein's arrival it was clear that a strong effort would be made after all to construct a model for the study of business cycles. Hence in 1945 the research program title was recast again to comprise two parts:

1. *Studies of economic fluctuations*
2. *Adaptation of statistical tools to economic analysis*

The problems of fitting the Cowles Commission into the University of Chicago did not go away. Marschak's room for maneuver was at times unpleasantly constrained, as in the Klein case, and Marschak became much involved in sorting out the issues. The Commission's Advisory Council which in 1939 comprised exactly the same persons as in 1932 had for years played little or no role in the decision making at CC. It was extended after 1939 to comprise Jacob Viner as representing the University but that hardly made much difference. In the summer of 1943 the University of Chicago established an Advisory Committee of Cowles Commission for Research in Economics to coordinate the work of the Cowles Commission with other research of the University. The Committee comprised Simeon Leland (chair), Louis Wirth (vice-chair), Walter Bartky, Garfield V. Cox, T.W. Schultz, and J. Viner, and in addition also Alfred Cowles, J. Marschak and O. Lange and D.H. Leavens as secretary of the Committee.[111]

In October 1944 the integration issue flared up and initiated a process which lasted beyond the remaining years of the Marschak period. Acting Dean Ralph W. Tyler (in Redfield's absence) broke the issue open:

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> 'The Social Science Research Committee has been discussing the work of the Cowles Commission and its relation to the research of the Social Science Division. It is our understanding that the University is unwilling to incorporate the Cowles Commission as an integral part of the Division until the Commission is able to provide for its support for a considerable period of time. Since the Commission is drawing upon some of the funds of the Division for its research and since the Commission staff is interested in a closer relationship with the University. It seems clear that some general policies need to be followed by the Research Committee in developing research plans for the next few years and particularly in drawing up the proposal for the Rockefeller Foundation. Mr. Marschak has expressed the opinion that Mr. Cowles is anxious to provide more adequately for the Commission but that he needs some definite request of this sort. Mr. Marschak is afraid that Mr. Cowles is in poor health. and may drop off at any time[!] and the Commission would be unable to continue its activities.
>
> What policy do you think the Committee should follow in its overall plan for divisional research?' (Memo from Dean Ralph W. Tyler to President Robert Hutchins, October 3, 1944.)

The pressure was on Cowles to come up with financial commitments. At the quarterly meeting of the Advisory Committee at the end of November 1944, Marschak outlined 'a long-range investigation into the statistical foundations for economic policy' and proposed to apply to Rockefeller Foundation through the SSRC for a five-year grant of $10,000 a year for the project. Cowles, who was present, was challenged to indicate what the five-year plan could expect to get from the Commission. Cowles wrote shortly afterward to Hutchins and gave his commitments in the following statement:

> 'I do not wish to bind myself legally, but I can assure you that I do not doubt that we shall be successful in securing for the Cowles Commission contributions totaling $20,000 a year during the next five years. We propose to apply at least $10,000 a year of these receipts, and probably a major part of them, to defray expenses of the research project mentioned above.' (Cowles to Hutchins, December 12, 1944.)

Cowles added that the expenses had never been below $20,000 a year and saw no reason for it to change. He could not legally commit his wealthy relatives who constituted the Commission; his own contributions had in fact only been a very minor part of the annual donations. That was as far as it got this time around and hardly fully satisfactory for the University. But there were frictions also over other matters. Vice-President Filbey notified Joseph Brandt of the

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University of Chicago Press that the Cowles Commission published its monographs with the Principia Press without indicating any relationship with the University of Chicago. Filbey indicated that this ought to change. Brandt responded that he had suggested years ago that the University Press should take over but the issue had got 'water-logged' by the resistance of the Commission. He added further that some member of the Commission had proprietary interest in Principia Press, which did not make it any easier. Filbey advised to 'permit them to ride along' until a closer working relationship has been established.[112]

The University strengthened its grip on the Commission further by inviting the Secretary of University, Neil H. Jacoby, who was quite interested in quantitative economic research, to join the Advisory Committee. Jacoby succeeded Filbey as Vice-President in 1945, while Hutchins stepped down as President and was replaced by Ernest C. Colwell. Hutchins became instead Chancellor of the University.

Marschak wrote in November 1945 for the benefit of the new leadership of the University a 3-page *Memorandum on University Research in Econometrics*, stating that 'our University has a good chance to develop one of the world's leading centers of econometric research' and setting out the main ideas in the research program currently pursued. His main message stated at the end of the memo was that 'the present size of our activities does correspond fairly well to (or lie slightly below) the optimal size of a manageable research institution of this kind'… [but]… if the quantity and quality of work is to be maintained. Its financing must be put on a more permanent basis; in fact, it is hard to attract and keep really valuable and mature workers of rare qualification by offering them short contracts; short term financing means continuous interruption of research projects just started, and the wasting of money in training mere apprentices who leave the team shortly afterwards.'

Marschak followed this up as instructed by the Advisory Committee with a letter to convey his view to the new President. His main message was to advise against rigidity in the relations between the Cowles Commission and departments of the University. The key point was that the criteria for selection of personnel for a research institution were not identical with those applied by a department:

> 'One reason for this difference is the fact that the work of the Cowles Commission is based on the cooperation of economists, statisticians and mathematicians…. If every appointment to our research staff had to receive the placet of the Department of Economics, the department may find itself in a difficult position.
>
> Secondly, the qualities required for a departmental appointment are traditionally those of a good teacher, though the research qualifications are also given attention. The appointments to a research staff are based, on the other hand, in the first line on research qualifications. To be sure, by joining a research group on the campus the appointee is also given a chance to show his other qualities…. Thus the

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> department members have a chance to get acquainted with the research workers and to form judgment on their qualities as possible faculty members. To form such judgment requires time. To wait for the judgment of department members before making an appointment to a research position would utterly paralyze the administration of research.
>
> Thirdly, a research institution is, by necessity, specialized. . . .The departments, on the other hand, must be, on the whole, well-balanced and represent a variety of subjects within a traditionally defined field.. . . our Department of Economics would feel heavily out of balance if swamped by people of predominantly quantitative of mind. In a natural reaction to this, the department would try to "dilute" the research staff by general economists, and decrease the share of, say, mathematicians. This would lower the research performance and diminish the chances of new discoveries, based on cooperation across departmental lines.' (Marschak to President E.C. Colwell, December 18, 1945.)

In conclusion, Marschak argued that it would be preferable to give the departments the chance of granting or not granting research staff members the rights and duties of department members; and to give the administrators of research the greatest possible flexibility in pursuing the work for which they were responsible.

Thus Marschak had to fight off tendencies of rigidity in the relations with the departments but the real problem with regard to financing rested with Cowles. The Cowles Commission could not get outside funds and within the University there was increasing concern over earmarking funds for the Cowles Commission because of the association with one particular donor. This caused problems even with getting an adequate salary for Marschak and also for Koopmans' remuneration. The problems remained unresolved. At the end of 1946 a proposal was prepared to persuade Cowles to be instrumental in establishing an endowment sufficiently large to earn $50,000 annually. Colwell asked Hutchins to talk with Cowles about it. Just before Hutchins' meeting with Cowles, Marschak talked with Hutchins over the telephone and then sent him a brief note with the substance of his 'telephonic implorations of a moment ago, written down for your convenience':

> 'It is not clear whether Mr. Cowles is aware of the present scope of the responsibilities of the Cowles Commission. The Cowles Commission does not produce disjoint studies by single individuals on various subjects. There is a unique large task to which every staff member is devoted: to formulate the relevant propositions of economics with logical precision, and work out adequate methods of empirical testing. As a result of this concentration, there is a continuous lively exchange of ideas between very outstanding young

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> persons that form our staff. There is no other organization in the world which concentrates itself on this task. In the last year some results have begun to be published and have quickly earned recognition. If the financing of our program is not ensured, it will be impossible to continue the work.' (Marschak to Hutchins, January 7, 1947.)

Marschak followed up by discussing the problems with Cowles one month later. The Cowles commitment was raised to $25,000 annually and Cowles agreed in principle to the necessary changes of the Commission such that it might be able to receive possible donations from the outside. Some problems remained until the preparation of the move to Yale much later. But these problems drained Marschak's energy and one can understand his need for a sabbatical in 1948 (as visiting professor at the National University of Mexico and at the University of Buffalo) and to be relieved of the research director responsibility.

6. THE JANUARY 1945 CC CONFERENCE

Toward the end of 1944, preparations started for a conference to be held in January 1945. The documentary record is thin about this conference which has been called 'the most influential conference on statistical inference in economics ever held' (Malinvaud, 1983, p. 57). No list of papers prepared or agenda has been found. But the ultimate result of the conference was a milestone in the history of econometrics; the proceedings from the small conference became–after a rather long time lag–the famous CC Monograph 10 *Statistical Inference in Dynamic Economic Models*, published in 1950 from manuscripts completed through 1946 and into 1947, some of which, but not all, were presented as drafts at the conference.[113]

The exact timing of the conference has its own explanation. In the exchange between Marschak and Wald above, Marschak had at the beginning of 1944 suggested to Wald that a small informal conference could be arranged. Wald responded positively but Marschak did not follow up. But, with the forthcoming a conference in early 1945, there was every reason to cash in on Wald's willingness to take part. Marschak was in fact so keen to get Wald to participate in the conference that he let Wald set the time for it. He sent Wald a list of topics to be discussed and inquired when he could be present. Wald who was teaching and engaged in war work in addition to his research agenda had a tight schedule.[114] He offered Marschak two alternatives, either the week between Christmas and New Year or the week beginning Monday January 29, 1945 (Wald to Marschak, November 13, 1944). Marschak decided on the latter and set the time from January 28 to February 1, 1945. (Wald later expressed surprise that it started already on the Sunday which he perhaps had planned for something else.)

But Wald apologized for being unable to think of a paper to prepare for the conference that fitted well with the listed topics. He explained by giving Marschak a rundown of what he was absorbed with at the moment:

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> 'During the last few months I have been working on a general theory of statistical inference the foundations of which were given in [Wald 1939b], and in the last chapter of the Notre Dame pamphlet of statistical inference. Was able to make considerable progress recently, and my latest results show a close relationship of the theory of statistical inference to the theory of games by von Neumann. I discussed this matter with von Neumann and he got very much interested in it. Because of the mathematical nature of my results, they will be published in the *Annals of Mathematics* (von Neumann is one of the editors) in a paper entitled "Statistical decision functions which minimize maximum risk."[115] At the same time I extended von Neumann's minimax theorem, which is of fundamental importance in the theory of games as well as in the theory of statistical inference, to infinite domains (von Neumann considers only finite domains). Also this will appear in the *Annals of Mathematics* in a paper entitled "Generalization of a theorem by von Neumann concerning two person games".' (Wald to Marschak, December 12, 1944.)[116]

If he were allowed to speak on something else than time series he would be happy to speak on *Statistical inference and von Neumann's theory of games*. But Marschak did not want that. Wald came without a paper as did some of the other experts invited for the discussion of papers at the meeting.[117]

Invitations were handled informally as it was essentially an internal conference for a specific purpose. The CC staff comprised in addition to Marschak, the research associates, Koopmans, Klein and Hurwicz (in from Ames), and the research assistants Rubin and Roy B. Leipnik, who had just been hired. To some extent the conference may also have meant to serve as a recruitment effort for staff needed to complete the project. Thus R.L. Anderson was invited, as was the statistician William G. Madow, and Marschak asked Wald to convey an invitation to H. B. Mann. None of these three were recruited but all contributed papers. The final three participants were Harold Hotelling, Gerhard Tintner, and Haavelmo.[118]

Haavelmo might have been an obvious choice for Marschak to invite for the conference but perhaps with little hope that he would be able to come (details are missing). There was no change in Haavelmo's work situation so he was still not thought of as a possible team member to help complete the intended monograph. Haavelmo chose to present a paper on Frisch's confluence analysis. The paper followed Koopmans (1937) in a stochastic interpretation of Frisch's model and showed that it could have less reasonable implications due to Frisch's lack of a probabilistic framework.[119] It was a wise move of Marschak to invite Haavelmo as the visit to Chicago and the conference may have had some influence on his decision at the end of the year to accept an offer to join the team before returning to Oslo.

At the conference Wald was an active participant. When Marschak and Koopmans a year and a half later communicated about a second conference in

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September, 1946 (see below), Marschak reminded Koopmans that at the 1945 conference Wald 'was as productive as all other guests together' (Marschak to Koopmans, July 28, 1946). Before they parted company, Marschak made sure to get a commitment from Wald to communicate in writing his contribution to the conference. He wrote to Wald in March, 1945 to remind him of his promise. Marschak continued to treat Wald with the highest reverence: 'You can have all the space you want and you can put your contribution in whatever form you feel most desirable' (Marschak to Wald, March 21, 1945).

Marschak then reminded Wald that the notes taken at the conference showed that Wald's main contributions were:

1. On the identification problem:

   (a) applying complete systems of invariants;

   (b) studying the general (nonlinear) case by using Gramian determinants.

2. On "Incomplete Systems" of stochastic equations (the so-called "Black Magic").

   (Marschak to Wald, March 21, 1945.)

Wald's remarks resulted in two short papers for Monograph 10. Marschak's point one resulted in the paper *Note on the Identification of Economic Relations*. Wald could not remember, however, what he had said on Gramian determinants.[120] Marschak's point two is more intriguing. Wald's submitted paper was titled *Remarks on the Estimation of Unknown Parameters in Incomplete Systems of Equations*. But what was the so-called "Black Magic" supposed to mean? There was nothing about it in Wald's submitted paper but the expression must have been used in the discussion at the conference. Was there a thread from this discussion back to Keynes' use of the "Black Magic" epithet in his high-handed critique of Tinbergen's work?[121]

From Marschak's letter it seemed as if the "Black Magic" issue had caused discussion and perhaps disagreement at the meeting with Koopmans and Rubin on one side and Wald on the other. It had been agreed that letters should be exchanged followed by a meeting between Wald and Koopmans in the spring (Rubin was back in the Army). But this did not happen. But Marschak again in a most reverential formulation asked Wald not to be held up: 'This further development of the "Black Magic" ideas must not prevent you, however, from writing down and communicating to us your contribution in the form in which it was made on the spot or in the form which it has acquired to date' (Marschak to Wald, March 21, 1945). Wald submitted both his pieces for the Monograph in April 1945.

From the context it seems clear that "Black Magic" had to do with the estimation of incomplete systems, i.e., of relations of a system where some of the relations were unknown, the topic of Hotelling's paper. The lack of information

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about the rest of the system led to a very different approach than that of estimating a limited part of a known complete system. In his contribution, Wald had sketched a method for approaching the problem that drew on ideas he had applied in the paper at the CC Research Conference in 1939 that Haavelmo had written to Frisch about (see above) and later published in an extended version (Wald, 1940). Koopmans (1945), written after the January conference had an alternative approach, adapted from an approximate shortcut method he launched for estimating single equations in a complete system for which a full investigation would take 'years of research'. Koopmans' pre-LIML shortcut method assumed *a priori* known values of key parameters in other equations. He also gave a very brief summary of Wald's approach (Koopmans, 1945, p. 466).[122] The reference to "Black Magic" would surprisingly reappear some months later in a letter from Abe Girshick (see below).[123]

In December 1944, Marschak had taken Wald's case up again with Bartky and Leland to see if something could be worked out after all and in the faint hope of eventually getting Wald associated with Commission. Key points for Wald were that he be allowed to spend one quarter, preferably in the summer, away from the university if he wanted to and that outside earnings were not completely ruled out.

As the University of Chicago had finally realized that by getting Wald there was a lot to be gained, especially as there was no other high caliber candidate within sight, Leland and Bartky agreed on the conditions to be offered in a conference with President Hutchins in the spring of 1945. The fourth quarter vacation was guaranteed and the salary level raised to above what Wald would have earned including side incomes on the earlier offer. The offer was $8,000 and unlimited tenure. A revised offer was sent by Bartky who laid it out to Wald: '... our proposal is that you should come to the University of Chicago as professor of Mathematical Statistics, serving in the Departments of Mathematics and Economics (Bartky to Wald, April 13, 1945). The duties were four courses a year in the Department of Mathematics and the rest of the time 'free for research work of your own choosing.' The offer was as good as it could be at that time. Wald greatly appreciated the offer and promised a decision by the beginning of May. Marschak must have been overjoyed at the prospect of finally getting Wald into Cowles Commission work after all.

But then it fell apart again. On May 8, 1945 Wald wrote to Bartky about the 'truly splendid offer made to me', that he nevertheless was not in a position to accept because

> 'Columbia University has offered me a full professorship with conditions essentially the same as yours. Because of the long association I have had with Columbia University and because my colleague professor H. Hotelling, to whom I am indebted, expressed his desire for my staying here, I feel that I cannot leave them.' (Wald to Bartky, May 8, 1945.)

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One is again left wondering whether this was skillful bargaining or merely difficulties in personal decision making. Wald wrote to Marschak that he hoped he would 'understand that it would have been difficult for me to make any other decision in spite of my great interest in work of the Cowles Commission'. One may wonder what Marschak thought of this response after struggling to find a solution over almost two years. It was not much of a comfort that Wald assured Marschak that he 'would very much like to keep in touch with you and to collaborate on statistical problems of importance for the work of the Cowles Commission' (Wald to Marschak, May 8, 1945).

The game was over but Marschak found a way of using Wald's insight: 'It is clear that we cannot have you here. But may I at least try and take a ransom from you, however belatedly? You would do me a great favor if you could give – quite confidentially – your opinion about the suitability of a few mathematical statisticians, whose names follow; and also add, with your opinion, any further names you care to suggest.' This was not so much an inquiry on behalf of the University as reflecting Marschak's interest in finding someone who was suitable to work with the Cowles Commission. There was a vacant "senior" position, i.e., the position that Wald had turned down. The University had also decided to open a "junior" position. Both of them were meant as joint appointments in the Departments of Mathematics and Economics. Marschak explained the difference: 'the "senior" man teaches future mathematical statisticians, the "junior" man teaches future users of mathematical statistics' (Marschak to Wald, June 22, 1945).

The statisticians whose suitability Marschak wanted to know were Tjalling Koopmans, William G. Madow, Henry B. Mann, Scheffé, and Wolfowitz. As an afterthought he also threw in Milton Friedman and Allen Wallis. Wald carefully gave a brief characterization of each person's qualifications. Wolfowitz and Madow both got top marks with Wolfowitz ranked highest. Wald found also Mann, Koopmans, and Scheffé qualified for the senior position with various qualifying details. Koopmans was praised by Wald ('a first rate man with primary interest in mathematical economics'). Mathematical statistics was his secondary field of interest ('he is very strong in the mathematical theory of statistical inference, but perhaps not quite as strong as some of the other candidates'). Friedman and Wallis were considered by Wald primarily as economists and not qualified for a senior position in statistics.[124] As to other possible candidates Wald was almost lost for an answer. He could only think of 'a few well established men, such as Wilks, Neyman, etc.' (Wald to Marschak, July 5, 1945.)

One young statistician who had not turned up in this search was Theodore W. Anderson. Marschak first heard about him in the middle of October 1945, when Wald wrote that Anderson had dropped by and told 'about his plans for working on some problems in multivariate statistics with particular reference to time series analysis.' Anderson hoped to get a fellowship in about six months; hence he needed something to do for half year. Wald characterized Anderson as 'a very able young man with considerable capacity for original research … [hence]

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it would be mutually beneficial if he could spend some time with the Cowles Commission' (Wald to Marschak, October 13, 1945).

Marschak acted immediately on the information. He got in touch with Anderson and agreed to meet him at Rye, New York on Sunday October 28, 1945. Marschak was there for a meeting of concerned scientists about atomic energy, October 27–28, 1945. Anderson showed up and Marschak convinced him that to appoint him to work for only half a year was unsatisfactory as it would take Anderson some time to get acquainted with the results achieved so far and with the general drift of the work at the Cowles Commission. Anderson yielded and they agreed that he should start to work in Chicago from November 20, 1945, without fixing a definite end date.

Wald's letter of October 13, 1945 had, however, a more important and confidential message, namely that the University of North Carolina wanted to create a major Institute of Statistics. For the theoretical group of the new institute they wanted Harold Hotelling, Wolfowitz, and Wald plus another mathematical statistician whose name Wald did not feel at liberty to disclose.[125] Wald further furnished Marschak with the information that Wolfowitz had already received a formal offer for an associate professorship which he had accepted, while Hotelling, waiting for a formal offer to arrive, felt greatly tempted to accept (as he eventually did). Wald's own attitude was more reserved. He foresaw the possibility that Hotelling would accept the offer while he himself declined, adding: 'should this happen, it would create an entirely new situation for me here and I feel somewhat isolated. Has the University of Chicago taken any steps so far to fill the two positions under consideration?' He added a final remark that he would appreciate to know what Marschak could do about Anderson.[126]

The news was unexpected, to say the least. Marschak got revived hope that his efforts would bear fruit after all. He wrote back after a few days and assured Wald that no steps had been taken regarding other candidates but moves in that direction might happen soon. Marschak advised Wald in case he really wanted to renew the contact with Chicago, to write a letter explaining the situation and express agreement to the offer that was made in the spring of 1945. He assured Wald that the Department of Economics would strongly support him unless the letter was so delayed that a new candidate was within reach. Just to make sure that Wald realized that he was no longer in a bargaining situation he reiterated: 'The letter must be written so as not to suggest any new negotiations but merely to express your acceptance of the old offer if the offer still stands' (Marschak to Wald, October 20, 1945).

Marschak asked Wald to call him during the meeting at Rye, New York. Obviously, Marschak wanted to exert the utmost influence he could muster to make Wald commit himself by sending the letter to Chicago. Wald did not call, however, until after Marschak had left. Wald wrote a few days later and reported that the president of the University of North Carolina had come to New York and met with Hotelling. The offers could be expected within two weeks. 'Should such development take place here that I would like to renew the contact with Chicago,

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I shall simply write a letter… as you suggested' (Wald to Marschak, November 3, 1945). Was Wald's interest in Chicago already slipping? Wald also asked about Marschak's contact with Anderson. Was it due to a bad conscience that he had not thought about Anderson when he assessed a number of statisticians for Marschak, particularly in view of his high assessment of Anderson for the task at hand at the Cowles Commission?

The blow dragged out but it came. Wald wrote close to Christmas that Hotelling had accepted the offer from North Carolina. Columbia had then decided to offer Wald very favorable conditions. It turned out that Columbia had also decided to upgrade mathematical statistics and was looking for two statisticians of professional rank (in addition to Wald) and even some junior appointments in addition. Wald wrapped it up as a careful prediction: 'Under these circumstances I shall probably decide for Columbia (Wald to Marschak, December 23, 1945). At the very end, knowing that Anderson had by then been at work for a month in Chicago, Wald asked whether Marschak was satisfied with him, adding, 'I trust that he is doing good work. Where is Rubin now?'

Marschak passed Wald's letter to Bartky, summing up the situation:

> 'I am afraid [Wald's letter] not only removes the last hope of getting him but, in addition, conjures up the prospect of a further tug-of-war between Chicago and Columbia!… Wald's inquiry about the well-being of T.W. Anderson and Rubin (who is back with us) sounds outright sinister!' (Marschak to Bartky, undated).

It was a heavy blow for Marschak. Throughout the history of earlier disappointments Marschak had reasonably interpreted Wald's vacillation as due to Hotelling's influence, not necessarily through anything Hotelling had said or done but through his sheer presence, and ultimately due to Wald's gratitude toward him. Now Hotelling was out of the picture but to no avail. Marschak felt the brunt. He knew the implications and set it out to Wald in almost solemn words. It was not just about Chicago and the Cowles Commission:

> 'Naturally, I read [your letter of Dec. 23] with regret that the last hope of having you here has collapsed. This affects also the prospects of the Cowles Commission and of econometrics in general. Good statistical foundation is needed to make economics a science; scientific economics is needed to get good economic policy; and the potential leverage of economic policy for the welfare of men may be enormous – at present probably larger than that of, say, physics which seems to contribute as much or more evil than good. Hence my lively regret in case you remain divorced from those statistical problems that bear upon economics: a great loss to all.' (Marschak to Wald, January 4, 1946.)[127]

Wald's capacity for quick assessments could, however, still be drawn upon. Shortly after this extinguishment of the last hope Marschak asked Wald for a

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statement of the relative merits of Friedman and Koopmans. Wald knew them both; Friedman had reputedly given him some lessons in economics:

> 'You asked me to give my opinion as to the relative merits of Friedman and Koopmans. Here are a few comments that may be helpful to you.
>
> 1. General research ability and originality. I rate both men highly in this respect, but it is difficult to make a comparison between them due to their different training and background. I would say that Koopmans seems to be capable of a deeper and more penetrating analysis, while Friedman is probably quicker and superior in originality. For example, Friedman's idea of analysis of variance based on ranks shows considerable originality.
>
> 2. Mathematical training and ability. In my opinion Koopmans has had much better training and has considerably higher abilities in mathematics. This opinion is mine based on personal acquaintance with them, as well as on their published work.
>
> 3. Teaching of elementary courses in statistics. Both are well qualified and would do a very good job.
>
> 4. Teaching of advanced courses and research in the mathematical theory of statistics. Friedman's mathematical background is in my opinion not quite adequate for teaching very advanced courses, while Koopmans would be qualified to do so. Although Friedman has high research abilities and originality, I feel that Koopmans can be expected to accomplish more in making contributions to the theory of statistics due to his superiority in mathematics.
>
> 5. Experience and ability in applying statistical methods to empirical problems. Both rate high in this respect. While Koopmans' work is restricted to economic applications, Friedman has gained experience, through his war work, in other fields as well, especially in acceptance inspection of manufactured products.
>
> 6. Training and ability in the field of economics. You are more competent to judge the candidates in this field and there is no need for me to express an opinion.
>
> In conclusion I would say this: both are highly able men, but along somewhat different lines due to their difference in their background, training and interests. If advanced mathematical statistics and econometrics are emphasized, I would regard Koopmans as the more suitable one.' (Wald to Marschak, January 27, 1946.)[128]

Toward the end of 1946 Marschak had a meeting with Wald in New York and talked about statisticians, mostly young ones. Marschak wrote a note to himself

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with Wald's brief and concise readings of the potentials of the young talents, e.g., Savage ('a gifted young man, with interests in many fields, but Wald is not at all certain about his caliber and promise as a mathematician'), Girshick ('the best choice for the post in mathematical statistics with emphasis on application and teaching rather than on mathematical trail blazing and generalization'), Madow ('possibly a better mathematician than Girshick, but much less cooperative and attentive in applications'), and Tukey ('his knowledge of mathematical statistics is yet not perfect, but his gifts hold out a great promise'). Top mark was, however, given to Herbert Robbins, associate professor at Chapel Hill, as 'the most outstanding and promising man of the younger generation.' Others commented upon or mentioned on this occasion were Henry B. Mann, Pao-Lu Hsu (reckoned today as the founder of probability and statistics in China), William Feller and Joseph L. Doob and, finally even Harald Cramér. Wald put Cramér in the same class with Neyman and Wilks, i.e., men with established reputation and position who may or may not continue to do creative works. "Dollar for dollar" the appointment of younger or less established persons like Robbins and Girshick was probably more efficient (Notes by Jacob Marschak after conversation with Abraham Wald in New York, undated).

7. HAAVELMO JOINS THE TEAM

As noted above Haavelmo went into service for the Norwegian government-in-exile in 1942. After having worked as a statistician and analyst for Nortraship in New York for two years, he was asked to take another position with an office in the Washington, D.C. After shuttling back and forth between Washington and New York from April to June 1944, Haavelmo moved permanently to Washington and worked in the Norwegian Embassy directly for the Norwegian government-in-exile in London. The work was more strenuous with less opportunity to keep in touch with universities. But Haavelmo found out that Washington was an advantageous observation point for keeping in touch with American economists, including some old acquaintances from the econometric network, who had got into important positions. He was thinking much about the challenges to be tackled in postwar Norway.

The chance to take part in the Cowles Commission seminar in January 1945 was a welcome break and may have softened Haavelmo up a little bit with regard to the idea of having a stint of academic work before returning to Norway, if possible. In the spring of 1945 he had submitted his note on the balanced budget multiplier to *Econometrica*. After liberation he wrote to Marschak and conveyed greetings from Frisch. He added that he had to keep on at the Embassy for a few months more: 'After that I don't know, except that I am set on getting back into University work. If you should happen to have any suggestions in that connection I would certainly be most interested in hearing about them' (Haavelmo to Marschak, July 28, 1945). Marschak went into action immediately and wrote soon after to Haavelmo that he could expect to be visited by Vice-President Neil

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Jacoby about vacancies at the Business School (Marschak to Haavelmo, August 2, 1945).[129] It didn't come to anything but Marschak is also likely to have alerted T.W. Schultz about the possibility of capturing Haavelmo before he disappeared to Norway.[130]

Theodore Schultz' background was agricultural economics. He had left Iowa State College for Chicago in 1943 after being embroiled in the bitter margarine incident.[131] In Chicago he got his own subunit called Agricultural Economics Research. In the autumn of 1945 he was preparing to take over as chairman of the Department of Economics after Simeon Leland from 1946. At the beginning of October 1945 Schultz dropped by Haavelmo's office in Washington. They got on to good talking terms right away and Schultz brought up his errand which was to invite Haavelmo to come up to Chicago for some days to talk informally about a major research project in the field of demand, focusing on agricultural products. Haavelmo cleared the dates with the Embassy and spent October 22–24 in Chicago.[132] During those days Haavelmo was invited to join the project which came to be titled *The Structure and Trend of Demand for Agricultural Products.* Haavelmo expressed interest but had to clear it with his superiors in London. He reported back that they were not particularly happy about the idea but he would get his way unless 'some unexpected and powerful obstacle develops', adding, 'I do not have to add that I am looking forward to this possibility of getting back into research work' (Haavelmo to Schultz, October 30, 1945).

Schultz followed up immediately with an application to the Social Science Research Committee for a two-year, one-person project designed for Trygve Haavelmo, to be supervised by a committee representing the Agricultural Economics Research and the Cowles Commission, comprising Gale Johnson (chair), T.W. Schultz, and J. Marschak. The application noted that '[t]he person of Trygve Haavelmo is particularly suited for this work. In addition to his well-known contributions in the field of statistical methodology, he has done concrete work on demand for agricultural products both in Norway and Denmark.'[133]

At the end of October Marschak and Klein came to Washington to take part in meetings. Klein told Haavelmo what he was doing in Chicago and how the whole project was shaping up. He was not privy to whatever took place of negotiations between Marschak and Haavelmo.

Schultz followed up with an invitation to Haavelmo for a second visit to Chicago over the period November 26–29, 1945 to take part in a conference about the project. It was a joint project of the Cowles Commission and the Agricultural Economics Research Group of the University of Chicago. At the conference were also Abe Girshick of USDA's Bureau of Agricultural Economics, Richard Stone, Margaret Reid, and others. Haavelmo signed a contract with Schultz during the meeting but was quite definite about not committing himself for more than one year. In the project plan Haavelmo's study was supposed to supplement the Cowles Commission's 'attempt to describe American economics as a macroeconomic system' and the Schultz led study of 'the allocation of resources within agriculture and between agriculture and the rest of the economy.'[134]

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It may seem a little surprising that Haavelmo responded in this way, given the impression he had given to most of his friends in 1941–42 that he had no higher wish than to return to Norway as soon as the conditions allowed. But there is no mystery here. Haavelmo had by the time he was approached by Schultz worked full-time for almost four years for the Norwegian government. While he in New York had been able finish his landmark works, Washington was strenuous and offered few attractions. But more important, he had half a year after liberation not been contacted from Norway about any position and he had no position to return to. Of course, Frisch and his own friends said they wanted him home but no openings had been indicated, only loose talk. This may seem odd but was due to the special political situation, with "old politicians" elected before the war, the government apparatus returned from London and Stockholm, and the civilian and paramilitary resistance vying for power and positions. The first postwar election in Norway took place in November 1945, just as Haavelmo signed up with Schultz. An operative and ambitious Labour Party regime came to power, with a program not least calling for high-level economic expertise. Shortly afterward Haavelmo received offers for highly placed positions in government at home. He generally responded by expressing interest in the offers but made it clear that he was unavailable in the short run, until 1947. Underlying his insistence on committing himself only for one year in Schultz' agricultural project was his deep sense of duty and self-imposed obligation to do his share in the postwar reconstruction work, surely paired with a sense of guilt for having been away from his artillery unit in the battle line in 1940 and the ensuing sacrifices during the war. Several of his co-workers from the Institute, including Frisch, had been incarcerated.

Haavelmo arrived in Chicago a few days into 1946 but had started to work on his assignment as soon as he had signed on to the project. Haavelmo must have been happy to find that Abe Girshick was at the project conference. He knew Girshick from some years ago and got on well with him. They spent a bit of time together in Washington at the end of the year. On December 21, 1945 Girshick and Haavelmo presented some of the new ideas on demand analysis to a group of economists at USDA. They agreed to get together again before Haavelmo left for Chicago. Thus, Haavelmo had already teamed up with Girshick and the general idea of writing a joint paper on the demand for food may have been initiated before Haavelmo arrived in Chicago.

Haavelmo noted on various occasions the interest and prominence given to Haavelmo (1943a). To him Haavelmo (1943a) was to him merely a somewhat popular exposition of ideas inherent in Haavelmo (1941, 1944). Perhaps he underestimated the appeal the brief and more easily accessible article on the estimation of simultaneous equations had to readers who hesitated in embarking on Haavelmo (1944). Haavelmo knew the CC team apart from T.W. Anderson who had been in Chicago only for one month or so when Haavelmo arrived. Anderson spent this time reading Haavelmo (1944) which he had never come across. He also studied Wald's paper on incomplete systems, Phillips (1986, p. 257).

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Herman Rubin returned from military service at the beginning of 1946; Haavelmo had met him at the 1945 CC conference.

On December 24, 1945 Girshick wrote a letter to Marschak that had an impact on the Cowles group, resulting in a rapid development of the limited information maximum likelihood method (LIML).[135] Girshick wrote: 'I have managed to do a little research on the problem of estimation and am convinced that for many systems there exists non-maximum likelihood estimates which are consistent and for which exact fiducial limits are available.' Girshick demonstrated his claim by using a stylized just identified two equation model to set out simple formulae for consistent estimates of the parameters.[136] Girshick further showed in an elegant way how Fisher's fiducial limits could be calculated, which for this simple case could easily be translated to Neyman–Pearson confidence intervals. Girshick's paper set off frantic activity in the Cowles group for methods of consistent estimation of single equations in a simultaneous system.

Girshick ended his letter by the following remark: 'I shall send you a more complete statement of these results together with some "black magic" estimates for incomplete systems which I am now working on.' (Girshick to Marschak, December 24, 1945). Here "black magic" re-occurred. The Cowles group must have recognized the expression from the discussion one year earlier (except Ted Anderson who had not been there). But for historians the context was lost. Epstein (1989)'s account of the work in the Cowles group noted that, 'Girshick closed his letter on a somewhat mysterious note by mentioning that he was experimenting with a "black magic" method of structural estimation that could also be used for incomplete equation systems' (Epstein, 1989, p. 103). Epstein went on to speculate what Girshick might have meant and verified that "black magic" was never mentioned in his later publications. He rendered slightly incorrectly what Girshick had said; Girshick had not mentioned 'structural estimation'. As we have seen above from the trail of the Marschak–Wald exchange above, "black magic" referred to estimation of incomplete systems, the topic of one of Wald's papers. (One may wonder from whom Girshick had heard about the "black magic" discussion at the 1945 meeting. Perhaps from Marschak but possibly this may have been a topic for conversation between Girshick and Haavelmo during their days together in Washington in December 1945.)

The immediate reaction to Girshick's letter was a two-page note by Koopmans and Rubin, dated January 10, 1946, which explained how Girshick's results had been achieved and that Girshick's estimates were maximum likelihood estimates obtained in a much simpler way (except for disregarding the independence of the residual terms).[137] The note was presented by Rubin and discussed at a staff meeting on January 18, 1946. The staff meeting minutes stated that Rubin had pointed out that a "black magic" method gave results which were 'asymptotically … formally identical'. Identical with what? Presumably with full information maximum likelihood. It was also stated that Rubin would give further results on "black magic" later. But that was the last mention of "black magic" in the staff meeting minutes.

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Then Anderson and Rubin completed a substantial 10-page note titled "A generalization of Girshick's method", in which Anderson drew on his earlier (dissertation) work on the Wishart distribution to generalize from estimation in just identified models to overidentified ones. It was the search for *Limited Information Maximum Likelihood* (LIML) but the name came much later. The Anderson–Rubin note was discussed at the staff meeting on February 8 with discussion continued on February 15. At both staff meetings the entire core team was present and no one else: Anderson, Haavelmo, Hurwicz, Klein, Koopmans, Leipnik, Marschak, and Rubin with minutes circulated also to Girshick and Wald. The minutes of the February 15 staff meeting, which had the first occurrence in writing of the term "reduced form method", had attached a one-page 'balance sheet comparing pros and cons of the reduced form methods and the maximum likelihood method', as set out by Rubin on the blackboard in room 404.[138]

In 1946 the Cowles Commission had more staff meetings and seminars than any earlier year. There were normally two staff meetings a week, each one based on one or two papers, whiles seminars were more ad hoc, often with outside speakers. As Marschak had stated at the beginning of the year, 'staff meetings are an essential part of our work'. Staff meetings were attended by staff plus occasional faculty members and outside visitors, often with less than ten persons in attendance. According to Hildreth (1986, p. 7), whose observations were from later years, 'chaos in these sessions was avoided only by judicious use of the "Cowles Commission Rule", namely that during a speaker's initial presentation he could only be interrupted by "clarifying" questions'.[139] This rule did not originate at the Cowles Commission, however, Marschak had brought it with him from the early Econometric Society meetings in Europe.[140]

The Econometric Society meeting in Cleveland January 24–27, 1946, exactly one and a half year after the Cleveland meeting mentioned in Section 5. Klein presented his model, Marschak spoke on the economist's problem of statistical inference, Hurwicz on structural estimation, and Koopmans (with Leipnik) on computation methods. *Econometrica*, still in a submission drought, gave each paper about two and half page in the report from the meeting. The results of the search for simpler estimation of equations in simultaneous systems, set in motion by the Girshick letter, were presented at conferences almost as they were developed. In April, 1946 Anderson presented a paper (with Girshick and Rubin as joint authors) at an ASA/IMS regional meeting in Washington, followed three months later by presentation of an Anderson and Rubin paper at the IMS meeting in August 1946 at Cornell, Ithaca. In Ithaca Koopmans presented Monograph 10 results. Finally, CC arranged its own conference in September (see below).

The development of LIML was a key achievement of the Cowles Commission in 1946. The train had been moving at full steam toward a resolution of the issue since Girshick's letter and Anderson accompanied by Rubin took a deeper look at the idea thrown in by Girschick. It is uncontested that the deep insight and driving force toward full resolution of the issue, and at an amazing speed was due to Anderson. The key factor was Anderson's prior knowledge.[141] Anderson had

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replaced the reduced form regression used by Girshick with the "the reduced rank regression". Anderson is known to have quipped: 'LIML? I had the reduced rank regression in my back pocket!'[142] The LIML method was after the Washington briefly known as the "Anderson–Girshick–Rubin" method but as Girschick felt he was not contributing further and Anderson and Rubin assented, his name was dropped. In Ithaca it was Anderson and Rubin. The theory and mathematics of the LIML method was finally published as Anderson and Rubin (1949). There were also other presentations and accompanying papers. Anderson's involvement with LIML can be thus be traced to have started at New Year 1946 and more or less completed by August the same year.[143] Girshick and Haavelmo wrote the first applied paper using LIML (also without the acronym) in 1946 with full documentation of the computations; it was published in *Econometrica* 1947 and gave rise to the Girshick–Haavelmo method as yet another interim label for LIML.

Koopmans and Marschak planned a CC conference in September 1946 and referred to it as the CC-2 conference with the January 1945 conference being CC-1. The main purpose was to present results to be published in Monograph 10 and problems related to those dealt with there. The two most important guests at that conference were intended to be Jerzy Neyman and Prasanta Chandra Mahalanobis. The conference was therefore nicknamed, if only between Koopmans and Marschak, the "Neyman–Mahalanobis meeting." In July 1946 Marschak drew up a list of issues to be discussed with Neyman and Mahalanobis, comprising the following: (1) a general outline of the problem; (2) the identification problem; (3) the maximum likelihood method; (4) the reduced form method; (5) possibly the "error of observations" problem; (6) possibly the "multiple hypotheses" problem; and (7) possibly the continuization problem. He stated to Koopmans that the object of the Neyman–Mahalanobis meeting was both to "propagandize", and to stimulate their work on the unsolved problems (Marschak to Koopmans, July 3, 1946). In the end neither Neyman nor Mahalanobis was able to attend. Koopmans proposed that Wilks be invited as he and Anderson had been working together on problems close to those involved in the LIML paper but Wilks could not attend either.

Marschak suggested other names to be invited, including Milton Friedman, Allen Wallis, and David Blackwell (none of them attended), and Gerhard Tintner and advised Koopmans to look for other persons at Ithaca suitable to be invited for the above stated reason. Such events were also an opportunity for recruiting and CC was in need of replacements. Koopmans discussed recruitment with Wald and Hotelling and got in writing strong recommendations for Kenneth Arrow. They had both impressed on Koopmans the brilliancy and excellent promise of Arrow. Yet Marschak had expressed some reserve about Arrow, so Koopmans met with him and found that he seemed drawn most strongly toward problems of tying in statistical assumptions with the theory of economic behavior. Arrow himself stated, when joining CC was suggested, that he had not expected to find a job which would be so much in line with his thesis work.[144] He was invited and gave a paper at a staff meeting on the eve of the CC-2 on 'Stability of Equilibrium'. He also wrote minutes from one of the sessions (in better prose than the usual

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standard). Arrow joined the Cowles Commission as research associate from April 1947).

Olav Reiersøl, one of Frisch's former students, and Harold Gregg Lewis also attended the conference. Paul Halmos, Holbrook Working, and Pierre Bessaignet from France came for a session or two. John von Neumann was so much in demand that he was hard to invite for anything. Hence he declined the invitation but gave Koopmans a window of opportunity by arranging to meet with him in Chicago to discuss computational problems a couple of weeks before the conference.[145]

Key presentations were by Marschak on 'Multi-equational hypotheses and the role of experiment and decision', Anderson and Hurwicz on 'Errors versus shocks', Anderson and Rubin on 'Reduced form method for estimating a single equation', Anderson, Hurwicz, and Rubin on 'Small sample confidence regions', and various topics for discussion on the last day. Minutes were taken for every session. Haavelmo did not attend the CC-2, as he had left for his doctoral defense in Oslo and was away for about six weeks.

Haavelmo's late arrival to the CC team gave him no chance to influence the main thrust resulting in Monograph 10. But he was lucky to arrive literally at the conception of the LIML and he grasped the opportunity to try his hand at it. Toward the end of 1946 he most likely longed to get to Norway for a number of reasons. He had more or less wrapped up the five papers he was working on at CC at the end of 1946 when he wrote two brief notes:

> *Remarks on prediction and economic policy*, CC Staff Papers Dec.1, 1946.
> *A note on structural equations and economic policies*, CC Staff Papers, Dec. 5, 1946.

Both were discussed at a staff meeting on December 6, 1945. The notes reflected Haavelmo's interest in applying what he had acquired in theory and methods for use in economic policy in Norway. The first note focused on the confidence region of policy prediction. The second note elaborated on the meaning of autonomy in the given context and other conceptual and methodological issues.

Haavelmo came (too) late to the team relative to the main effort of developing econometric methods inspired by Haavelmo (1943a, 1944). Wald never joined although nevertheless contributed in a limited way. There is hardly any doubt that Haavelmo and Wald were Marschak's top priorities as team members. He seems to have ranked both of them way above Koopmans for what they potentially would have been worth as team members, and perhaps for personal reasons as well.

An afterthought is that the issues Haavelmo introduced in Haavelmo (1943a) came to play a relatively larger role in the research program actually pursued in the team effort to complete Monograph 10 than the wider range of issues in Haavelmo (1944). A counterfactual worth considering is what difference it would have made if Haavelmo had left his war work after one year and accompanied Marschak to the Cowles Commission in 1943.

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8. THE SCATTERING OF THE TEAM

When Wald told Marschak in October 1945 about the young statistician, Theodore W. Anderson, he somehow had overlooked in his search for CC recruitment, he described him as 'a statistician with considerable capacity for original research'. In the same letter Wald mentioned that the University for North Carolina was out to get Hotelling, Wald himself, and Wolfowitz for its new Institute of Statistics. In the end only Hotelling left Columbia for North Carolina and Columbia responded by a decision to upgrade statistics. In 1946 a Department of Mathematical Statistics was established at the faculty of Political Science at Columbia. After Hotelling's departure Abraham Wald was in charge. He wanted it to be a department with three founding members: Wald himself, his teaching assistant, colleague and co-author Jacob Wolfowitz and Theodore W. Anderson. Anderson who carefully had avoided any commitment for the length of his stay at CC, took off for Columbia on the last day of the September conference.

Haavelmo completed at the very end of his stay the four papers he had initiated as part of the assignment (all got published in 1947). The couple of months he worked into 1947 were compensation for his absence in the autumn of 1946. On March 4, 1947 the entire CC group attended a farewell luncheon for Haavelmo at the Quadrangle Club.

In the middle of February 1947, Frisch paid a brief visit to Chicago. He had a demanding UN assignment, working out of an office in the Empire State Building. He gave a CC seminar on February 14, 1947 on *Some basic formulae in demand analysis*. Jan Tinbergen had given two seminars half a year earlier and also Kalecki had made an appearance. These outside visits by prominent European econometricians were rare events in the immediate postwar period and much appreciated. The seminars with Tinbergen and Frisch generated enthusiastic discussions.[146]

One reason for Frisch's visit to Chicago may well have been to make sure that Haavelmo returned to Oslo. He could tell Haavelmo that he had a firm promise from the Minister of Finance that a professorship meant for Haavelmo would be included in the forthcoming government budget, i.e., from July 1, 1947. Haavelmo wanted a number of personal conditions guaranteed, including arrangements for Beulah Midgett to come to Oslo.[147] Klein observed that Haavelmo was extremely private with his personal arrangements but got the impression that Haavelmo vis-à-vis Frisch was 'driving a hard bargain, with all "t's crossed and I's dotted" so that he got exactly what he wanted with the government of Norway, the University, and appropriate treatment for Beulah' (personal information from L.R. Klein, 2005).

It was during Frisch's visit that Haavelmo raised the possibility of Klein visiting Oslo for a year. Frisch was very positive, especially at the prospect of Klein getting an SSRC fellowship, for which Frisch served as one of the referees.

Frisch is likely to have spoken out about the importance of getting Haavelmo back to Norway, surely to the annoyance of Haavelmo. At the same time

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FIGURE 3. The top picture is of Trygve Haavelmo and Jacob Marschak walking toward the Social Sciences Building, University of Chicago. The bottom picture shows the entire Cowles group near the Quadrangle Club, University of Chicago. From left to right: Ed Boorstein, Dickson Leavens, Lawrence Klein, Gershon Cooper, Tjalling Koopmans, Herman Rubin, George Perazich, Jacob Marschak, Jacques Hartog, Trygve Haavelmo, Sam Schurr, Selma Schweitzer, Gertrude Nissenbaum. Sitting, from left: Don Patinkin, Sonia Klein, Estelle Maas. Both pictures were taken on March 4, 1947 by Don and Dvorah Patinkin and made available by the courtesy of Sonia Klein.

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Haavelmo had surely decided to return before Frisch arrived. Some of his Chicago colleagues, not least Schultz, may have believed that there was a chance he would stay on after all. One can see the situation and the feelings reflected in Schultz' report to Louis Wirth:

> 'This is to recommend the termination of the appointment of Trygve Haavelmo as Research Associate effective February 28. Mr. Haavelmo has accepted a professorship at the University of Oslo, which they have urged upon him for some time and which the recent visit of Professor Frisch finally persuaded him to accept. I am sure, in view of Mr. Haavelmo's values and beliefs with regard to his contribution in Norway and this country, and the high position they have offered him, that it is to his interest and the profession to accept, as he has done, thus terminating his appointment with us.' (Schultz to Wirth, March 4, 1947.)

Actually, Haavelmo did not become a professor until March 1948. In the intervening year he was the key coordinator of the "national budget" in Norway, a very demanding position. No one knows for sure though how Haavelmo would have reacted to a proper offer for a position at Chicago. Schultz was perhaps taken by surprise that Haavelmo did terminate his engagement after all.

Haavelmo left the U.S. by ship from Brooklyn on March 7, 1947. In New York he dropped by a Norwegian economist friend and former colleague as assistant for Frisch in the late 1930s. They had worked on Frisch's national accounting system together. The friend worked at the United Nations and expressed astonishment that Haavelmo, with such good prospects in the U.S., would leave for postwar austerity in Oslo. Haavelmo answered briefly that he liked trout fishing. The friend pointed out the excellent prospects for trout fishing in the U.S., only to be told: 'Yes, but that is not Norwegian trout.'[148]

Shortly after Haavelmo left the CC-3 conference took place, with two papers by Koopmans, three by Rubin, and also by Mann, Wald, and Leonard Savage. Wald spoke on 'Efficiency of Maximum Likelihood Estimates.' Klein sent Haavelmo a brief report:

> 'There has not been much excitement around CC since you left. … There was a conference here at the end of March. Wald was very good, but most of the other papers seemed like old stuff. Arrow has arrived here and is doing good work. He is very bright and should contribute new ideas.' (Klein to Haavelmo, May 5, 1947.)

Klein summed up the situation. The excitement had evaporated from the statistical inference project while Wald kept on top of the problems undisturbed. Klein had a short stint in Canada before he followed Haavelmo to Oslo in October 1947 to spend most of a year at Frisch's Institute, observing at close range the postwar reconstruction of the economy.[149] Klein had been closer to Haavelmo during

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his period at Cowles Commission than any of the others. He too had wondered why Haavelmo had decided to return to Norway and made the following observation: 'One night in the Oslo countryside, we had been drinking a lot, and Trygve took me out to see a "picture-postcard" view in the moonlight with deep snow all round. He said, "Look at this scene and you can understand why I wanted to return".'[150] These anecdotal remarks surely had a grain of truth but at a deeper level Haavelmo must have felt an obligation to contribute to the postwar reconstruction in his home country after having, against his will, spent the war years in comfortable circumstances in the U.S.

After Oslo Klein spent time with Tinbergen at the Central Planning Bureau before he returned to the U.S. to work at NBER. T.W. Anderson who left Cowles Commission for a position at Columbia, followed suit by spending 1947/48 as a fellowship year in Sweden, allowing occasional visits to Oslo. Haavelmo was appointed professor in economics and statistics in the middle of the spring term 1948. Practically his first act as professor was to initiate a university invitation to Abraham Wald who received it just a few weeks before the fatal trip to India.[151]

Hurwicz went back to Ames in the second half of 1946. In 1949 he joined the University of Illinois as a research professor, only to be embroiled in the Bowen incident[152], and went back for another stint as research associate at the Cowles Commission. Rubin left for Princeton in the middle of 1947 and continued on to Illinois Tech and Stanford.

In December 1948 there was again an Econometric Society meeting in Cleveland for the third time since 1944. Koopmans spoke in a joint session with AEA on Current Research in Business Cycles with a paper titled "The econometric approach to business fluctuations." The only other paper in the session was by Robert A. Gordon. Marschak and Hurwicz had moved to new topics (incomplete information and linear programming, respectively), while Klein was present without a paper. But something happened at that session, as mentioned by Klein in a letter to Marschak a few days later:

> 'I still have hopes of showing Haberler, Schumpeter and some of the others, who tried to write off our efforts at Cleveland, a few things when the time is ripe, but meanwhile I am just going to do some quiet research here. I have some hopes concerning the Canadian model and the ability to make short run forecast provided all the work is kept up to date.' (Klein to Marschak, January 3, 1949.)

What had happened? One of four discussants in Koopmans' session was Gottfried Haberler who threw scathing remarks in Koopmans' direction: 'The econometric approach of the Cowles Commission seems to be petering out rapidly or not to be getting anywhere beyond extensive methodological discussions.' Schumpeter who was President of American Economic Association in 1948 was present in the session and seconded his compatriot's remarks.[153] But Haberler had a point, the econometric program of the Cowles Commission was petering out.[154]

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9. AFTERMATH

In 1954, Haavelmo received a letter from Lloyd G. Reynolds, Chairman of the Department of Economics at Yale that must have surprised him. Reynolds wanted Haavelmo's 'impression of Professor Koopmans scientific ability and reputation … at your earliest convenience' (Reynolds to Haavelmo, September 22, 1954).

Haavelmo answered right away that he 'would not hesitate to name Professor Koopmans one of the most outstanding econometricians of our time. … As a director of the Cowles Commission for Research in Economics he has a large share in the scientific achievements for which that institution has gained world-wide recognition.' He recounted his contact with Koopmans since they first met in Oslo in 1935 ('impressing all of us with his advanced ideas in the field of statistical theory') and concluded:

> During the time I spent at the Cowles Commission I don't remember any seminar where Professor Koopmans was not present as an active participant. I was always impressed by his obvious pedagogical faculties in the way of organizing discussions. I would like to add another personal impression which is perhaps not so obvious to the non-mathematical readers of Professor Koopmans' publications, viz. his genuine interest in economics rather than pure mathematics. I know that it has been his great ambition to direct the Cowles Commission constantly towards theoretical and applied economics as the main field of research, with mathematics and statistics as auxiliary tools (Haavelmo to Reynolds, September 28, 1954).

The letter had just been sent when Haavelmo received a telegram from Reynolds on September 30, 1954 with a similar request related to Marschak. In Haavelmo's response sent the next day one may sense the slight chill he must have felt at the thought that Koopmans and Marschak perhaps competed for the same position. He stated that he 'would find it difficult indeed to rank one of them first and the other second' and wrote the following succinct assessment of Marschak:

> Among economists Marschak's publications range over a wider field of economic and econometric problems and because of the longer period of time in which Marschak already has held the status of a world-renowned scholar in the field of economics. What I think is the most impressive part of Professor Marschak's scientific performance is the capacity he has shown of remaining almost constantly at the frontier of scientific development. Thus, for example, in the early 1930's when the technique of measuring demand functions was in its first rudimentary stage, Professor Marschak advanced ideas that point towards the more modern methodology of our time. He was the driving force in the work … upon a general revision of econometric methodology. I remember that there were not many people at that time that saw the need for – and the possibilities of – such a revision

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> as clearly as did Professor Marschak. Another example: At a time when most of us were only vaguely aware of the economic implications of atomic energy, Professor Marschak was already engaged in explicit research upon this subject."[155] (Haavelmo to Reynolds, October 1, 1954.)

Reynolds wrote two weeks later to state that both men had been appointed as part of the plan to relocate the Cowles Commission to Yale as of July 1, 1955, 'hoping that we may be able to carry forward the tradition which Irving Fisher did so much to develop during his lifetime.'[156]

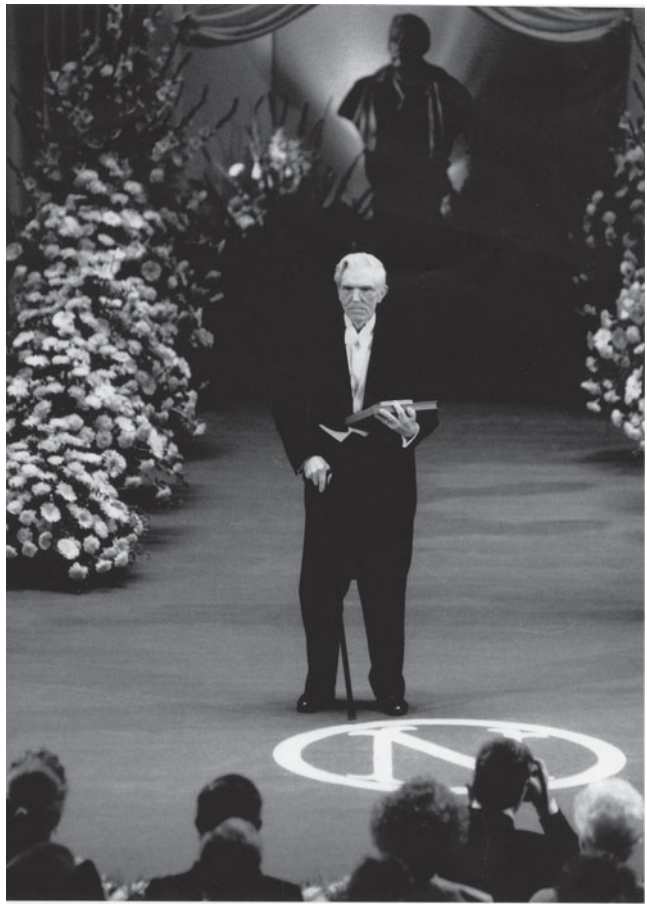


FIGURE 4. Trygve Haavelmo received in 1989 the Sveriges Riksbank Prize in Economic Sciences in Memory of Alfred Nobel *for his clarification of the probability theory foundations of econometrics and his analyses of simultaneous economic structures*. It was the first (and after 24 more years still the only) time the caption used in the press release included the term *econometrics*.

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At the end of 1955, T. W. Schultz, still department chair in Chicago wrote to invite Haavelmo to visit for one year. Haavelmo had received such invitations from Koopmans or Schultz practically each year since he left. But this time Schultz added: 'May I also ask the harder question. Is there a chance that you would give up Norway and consider taking a permanent position in the United States? (T. W. Schultz to Haavelmo, December 13, 1955).

Haavelmo responded that he finally had definite plans for taking a year off ('unfortunately the opportunity comes later than I had hoped'); he had decided to apply for leave to spend 1957/58 in the U.S., adding 'I hesitate in saying a definite "no" to the "harder" question that you have asked, because one cannot know the future. But so far, I have not changed my old attitude' (Haavelmo to Schultz, January 2, 1956).

NOTES

1. Frisch to Cowles, June 14, 1939.
2. Marschak to Haavelmo, December 8, 1958.
3. After his visit Haavelmo applied in the autumn of 1937 for a vacant position at the League of Nations' Economic Section which comprised Tinbergen's unit, but was turned down.
4. Immediately after the Business Cycle Conference Marschak conveyed his impression to Frisch: 'The conference proved to be a great triumph for the econometric cause, I think. Even the most skeptical or non-mathematical members found that Tinbergen's work deserves the greatest attention and that the method is worth trying again and again.' Marschak to Frisch, July 22, 1938.
5. Marschak reported to Frisch about Haavelmo's visit and seminars that 'we all learnt much from him.' Marschak to Frisch, June 16, 1938.
6. A little earlier during the conference Marschak had reported to Frisch that Haavelmo was 'a great asset for anyone who stays here.' Marschak to Frisch, July 14, 1939.
7. Hotelling also brought Wald into the Statistical Research Group which was a unit under the Office of Scientific Research and Development 1942–45, located at Columbia University but not part of it, see below and Wallis (1980a). Notice the remark in Haavelmo's letter to Frisch, cited above, that Wald would, take over Hotelling's position at Columbia. That was an advance rumor which turned out to be true but cannot have had any verification at the time. One may wonder from whom Haavelmo picked this up.
8. Frisch had indeed visited Cowles already in 1932 before the CC was incorporated to lay the groundwork for *Econometrica*.
9. Haavelmo to Kittredge, R.F., April 15, 1939; Rockefeller Foundation application dated June 10, 1939.
10. Haavelmo to Frisch, November 15 & November 17, 1939. The fellowship was later extended for another year.
11. In view of the emphasis on Neyman–Pearson test procedure in Haavelmo (1941), it is more surprising that Haavelmo did not acknowledge any debt to Jerzy Neyman. Haavelmo visited Neyman's Statistical Lab for two months immediately after Colorado Springs and reported to Frisch that he had attended Neyman's lectures, had discussions with him, and got a number of issues related to statistical testing sorted out. At the very end of the visit Haavelmo gave a seminar in the Statistical Lab. On Haavelmo's earlier contact with Neyman, see Bjerkholt (2005, p. 510). Both the missing acknowledgement and the anecdotal remark on Neyman in Haavelmo's Nobel lecture in 1989 are puzzling. Perhaps the acknowledgement Haavelmo gave Neyman in the Nobel lecture in 1989 was motivated by a desire to make up for the omission.

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12. Haavelmo to Frisch, March 27, 1940. The letter is informative also on the discussions with Wald and on other research ideas pursued by Haavelmo at that time. Haavelmo (1940) acknowledged valuable comments both from Marschak and Wald.

13. Haavelmo's interaction with Wald and Marschak was observed by others as Tjalling Koopmans noted in a memorial article after Marschak's death in 1977 that in 1940–42 '[t]he contacts between Marschak, Haavelmo, and Wald were particularly intensive.' (Koopmans, 1978, p. ix.)

14. Marschak's last westward move was due to incidents of prejudice in prudish New Haven, according to one of Marschak's acquaintances from the Chicago years (private communication).

15. Diary of Henry Schultz, unpublished. On the day before the meeting, a special session was held with Ragnar Frisch giving Colloquium-lectures on the algebra of linear transformations and quadratic forms 10-12:30, 14:30-17:30, 19:30-22, i.e., 8 full hours, probably hard to digest for several of the 30 participants.

16. Frisch spoke with T.B. Kittredge in the Paris office of the Foundation in early April while he was giving lectures at the Institut Henri Poincaré and later wrote to John van Sickle at the New York office but to no avail. (Frisch to Marschak, May 3, 1933.) Marschak had told Frisch that he would apply for a lectureship at Oxford, meant for a refugee economist. On his way home from Paris Frisch passed through London and talked to Lionel Robbins, who was on the committee and assured him that Marschak was already under consideration. Marschak was to some extent known in England as he had spent some months there on a Rockefeller fellowship in 1927.

17. Hagemann (1997, p. 239) asserts that Marschak fled via Vienna, Spain and the Netherlands (an unlikely escape route!).

18. Marschak (1934). The manuscript was submitted in German and was translated in Colorado Springs before it was published, a practice soon to be abolished. The language of Marschak's 15–20 letters to Frisch February–November 1933 changed from German via French to English.

19. Hagemann (1997) is a highly commendable source on Marschak's life and career before 1933 while weaker on the post-1933 period. Cherrier (2010) covers the later period extensively but in a somewhat superficial fashion and with an annoying inclination to be assertive on details that are notoriously incorrect, e.g., Marschak was never minister in a *Ukrainian* republic (p. 443), he went to the U.S. on a Rockefeller fellowship rather than 'emigrate' and it took place in 1938, not 1939 (p. 444), Koopmans succeeded Marschak as research director in 1948, not 1947 (p. 444), Marschak did not switch from Jakob to Jacob after 1933 but between 1941 and 1942 (p. 444), etc. ad nauseam.

20. Frisch had on one occasion nurtured doubts about Marschak's mathematical proficiency but that turned out to be unfounded. Frisch promoted strongly Marschak's nomination for ES Fellowship at the first regular election of new Fellows by Fellows in 1935. Louçã (2007, pp. 31 & 47) oddly asserts that there was no Fellowship election in 1935, in a volume marred by very many inaccuracies.

21. Frisch to Wald, June 5, 1937. The Annecy meeting was prepared by Tinbergen and the venue was chosen for its proximity to Geneva where Tinbergen worked assisted by a group of young econometricians. Tinbergen had told Frisch that Wald would work with him in the autumn and thus would come to the Annecy meeting. Wald's assignment in Tinbergen's group was to work on international comparisons of costs of living but he did in fact help out with putting the whole system of equations together (Wald to Cowles, January, 1938).

22. Frisch to Cowles, November 22, 1937. Cowles had introduced CC fellowships a couple of years earlier, e.g., for Gerhard Tintner. It was a lowly paid researcher position ($1000 a year), suitable, e.g., for European scholars with modest requirements and for refugees.

23. Frisch to Wald, January 20, 1938; Wald to Frisch, February 3, 1938. Frisch informed Wald that the Norwegian New York Line would offer him a free railway ticket to Oslo.

24. Wald to Frisch, June 27, 1938. Wald's friend, Karl Schlesinger, with whom he had attended the Annecy conference of the Econometric Society in the middle of September 1937 had shot himself on the day of the Anschluss March 12, 1938.

25. Extensive details are given in Christ (1952, pp. 7–9). Christ (1952) is the closest one can get to an official history of the Cowles Commission, giving a rich picture of the Commission's activity but with less detail for the early years.

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26. The Econometric Society had, since it was founded, a strong wish to establish a journal but no viable plan whatsoever. Alfred Cowles 3rd and his close relatives were wealthy. Cowles' grandfather had been business manager and part owner of the Chicago Tribune. Cowles was a Yale graduate like his father and uncle, with whom Fisher was acquainted.

27. The name was changed before the first monograph appeared, Cowles to Frisch, June 28, 1934. Frisch had planned to write the first monograph but it was cancelled due to some controversy. Frisch had indeed in 1931 proposed a series of "Monographs of the Econometric Society" (Memorandum on the research and publishing activity of the Econometric Society, November 12, 1931). Frisch's third and last visit to Colorado Springs took place in 1937 and resulted in another memorandum, as discussed below.

28. Davis later published two textbooks, one as a CC Monograph. Both may be considered as having sprung from the original initiative, Davis (1940, 1941).

29. Roos was also the first to commercialize econometrics, as soon after his departure from CC, he started a consultancy agency he called *The Econometric Institute*.

30. Tinbergen to Frisch, January 18, 1937; Frisch to Cowles, January 25, 1937.

31. There were few other sources for financial support than the Rockefeller Foundation which supported or had supported NBER, the Brookings institution, the University of Chicago, and a host of institutions in other countries from China to Switzerland, including the London School of Economics, Marschak's Institute of Statistics in Oxford, and Frisch's Institute of Economics in Oslo. Thus Cowles set his hope to the Rockefeller Foundation and prepared an application.

32. A tiny notebook retrieved from Frisch's archive with densely written notes jotted down by Frisch appears to be Schumpeter's opinion about a number of persons whether candidates or not: Haberler, Marschak, Tinbergen, Allen, Louis Bean, Ezekiel, Waugh, Wald, Hotelling, Woytinsky, Elmer Working, Lundberg, Arthur Burns, Phelps Brown, and Kalecki.

33. Cf. Haavelmo's acknowledgement note that '[t] he reader will recognize many of Frisch's ideas in the following, and indirectly his influence can be traced in the formulation of problems and the methods of analysis adopted' (Haavelmo, 1941, p. v).

34. He was thus the highest ranking Italian within the Society. Bresciani-Turroni, who was an antifascist in exile, residing and teaching in Egypt, had been elected to the Council on Del Vecchio's recommendation when the latter's period expired.

35. Cowles to Frisch, October 15, 1937; Frisch to Cowles, November 1, 1937. In an obituary Wold (1961, p. 655) gave high marks for qualifications and personality and praised not least Anderson's 'causal analysis on the basis of non-experimental data.'

36. But Evans had his own agenda, he wanted to recruit Lange to Berkeley. Frisch to Cowles, November 22, 1937; Frisch to Cowles, April 19, 1938; Cowles to Frisch, May 6, 1938.

37. Frisch to Cowles, October 22, 1937.

38. Cowles to Marschak, April 20, 1938; Cowles to Frisch, May 6, 1938.

39. Cowles to Frisch, May 6, 1937; Frisch to Cowles, June 21, 1938; Bresciani-Turroni to Frisch, May 1 & June 10, 1938; Frisch to Bresciani-Turroni, May 12 & 21, 1938.

40. According to Christ (1952) but with no evidence provided, discussions about a move were conducted with UCLA, Yale, and Northwestern in 1937–38. It is not clear what kind of relationship was sought between the Commission and either one of these universities.

41. Frisch did not recall the source in the letter to Cowles but his files reveal that it was Horst Mendershausen, who visited Marschak in Oxford at the beginning of the year and reported to Frisch: 'Marschak told me that he has not the intention to go to America. You will enjoy this.' Mendershausen to Frisch, March 11, 1938.

42. Marschak to Kittredge, April 2, 1938; Kittredge to Marschak, May 16 & August 21, 1938.

43. Emil Lederer had been on Marschak's dissertation committee in 1922 and they had worked together in Berlin in the mid-1920s, see Hagemann (1997).

44. A former student at the University of Chicago characterized Hutchins at that time as 'the country's youngest, handsomest, wittiest, and most arrogant college president' (Gardner, 1973, p. 1).

45. It seems likely that Cowles' offer was premeditated rather than a spontaneous, postfuneral whim. Cowles' father was 74 years old and his death perhaps not entirely unexpected. The wide-ranging content of the offer suggests that there had been earlier contact between CC and University

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of Chicago about a move. The role played in this process by the death of Henry Schultz in November 1938 as expounded by Christ (1952) is unclear, possibly an ex post rationalization.

46. Recent research papers on topics related to the Cowles Commission in the 1940s tend to propagate a picture of the Cowles Commission quite estranged from the Econometric Society, e.g., Till Düppe and E. Roy Weintraub: Siting the New Economic Science: The Cowles Commission's Activity Analysis Conference of June 1949 (CHOPE Working Paper No. 2013-03) and Catherine Herfeld: Axiomatic Choice Theory Traveling between Mathematical Formalism, Normative Choice Rules and Psychological Measurement, 1944–1956 (CHOPE Working Paper No. 2013-11).

47. Emery T. Filbey had a career hard to imagine at any other university. He had through a long career been inter alia Director of the Institute of Meat Packing, Professor of Commerce and Business and Vice-President. After retirement in 1944 he was in succession Acting Dean of the Division of Social Sciences, the Federated Theological Seminary, and the Oriental Institute. Throughout he was on the board of Chicago's Y.M.C.A.

48. Cowles to Hutchins, March 1, 1939.

49. The choice of Yntema took place barely one year after Marschak was offered the position. Did Cowles think of him at this time as a better candidate than the one he had chosen? Hardly, but the move to Chicago seems to have implied that the University took charge of the selection of research director, leaving to Cowles only the formal appointment. This became more evident next time around.

50. F. McIntyre had been employed by Cowles Commission since 1937 but left soon after the move to Chicago.

51. According to Christ (1952, pp. 7–8), it was Davis who in 1931 had advised Cowles to get in touch with the Econometric Society and 'offer to finance the publication of a journal for them and set up a research organization under their auspices with the resources and freedom for econometric research and publication.'

52. Wald was lost for Chicago in any case as Hotelling had a better offer.

53. Monographs 1 and 2, both by Charles Roos, did not represent CC research. Monograph 1 comprised essays from before 1932, while Monograph 2 had been written by Roos while employed by the National Recovery Administration (NRA). According to Christ (1952) Roos' devastating criticism contributed toward eroding congressional support for the legislative revival of the NRA after the Supreme Court declared the National Industrial Recovery Act unconstitutional.

54. Leland informed Director J.H. Willits of the Rockefeller Foundation's Division of the Social Sciences, 'it being the Department's belief that the situation should be clearly explained to him.' (Leland to Hutchins, October 1, 1942.)

55. With the proposals were attached a 'data sheet' for each person. The same was done for the additional proposals below. The unnamed son of E. Dana Durand was David Durand.

56. Leland to Hutchins, Oct. 1, 1942. Oskar Lange was at Columbia on leave from the University of Chicago and Cowles Commission 1941–43. What did Leland really mean to suggest about the Willits-Burns relationship?

57. George Terborgh and Corwin Edwards were somewhat odd proposals as neither of them had shown any interest in the Econometric Society. Terborgh had been at the Fed and co-authored with Lauchlin Currie and Beardsley Ruml; Edwards was an institutionalist, had been at NBER (and was much later honored by the Veblen-Commons award). Terborgh and Edwards both wrote books causing controversy over economic policy proposals.

58. From the notes left from the meeting it seems that Yntema brought up yet another proposal, namely Harold Gregg Lewis who had been research associate since 1939 and was regarded as very promising.

59. The letter was obviously misdated as Lange acknowledged Leland's letter of October 15, just as Hansen did. So more likely the correct date was October 21. The letter had in fact been transcribed from the handwritten original for which Lange apologized adding, that 'as I do not know the girl in the Department here who takes down the dictations I preferred not to dictate the letter'[!].

60. The additional remarks that Haberler could only with great difficulty be persuaded to leave Harvard and on quite expensive terms while Marschak was ready to leave the New York any day, seem out of place in a comparison of qualifications but revealed perhaps how much Lange wanted Marschak to get the position.

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61. The MS by Burns and Mitchell is likely to have been an early draft of Burns and Mitchell (1946), the centerpiece of the 'measurement without theory' Koopmans-Vining fracas discussed in Hendry and Morgan (1995, pp. 69–71 and 491–522), who did not mention that Rutledge Vining had indeed embraced Haavelmo's probability ideas as well as Marschak's theorizing in Vining (1945).

62. Lange withheld judgment on Terborgh whom he had never met, although his publications had impressed Lange as 'extremely competent fact-finding research.'

63. Ralph Young had added Harold Berger, Raymond T. Bowman, Morris A. Copeland and Clark Warburton and three more names, presumably from the department of economics, were John M. Cassels, Oskar Morgenstern, and Roswell H. Whitman. Leland to Hutchins, October 24, 1942. Cassels had on Schumpeter's recommendation visited Frisch in Oslo in 1935 (coinciding with T.C. Koopmans' visit). Bowman, Warburton, Morgenstern, and Whitman were members of the Econometric Society but only Whitman seems to have been an active member.

64. Filbey who may have leant in another direction told Professor of Astronomy, Walter Bartky, that 'Lange's appraisal of these men is so much more penetrating than many of the others we received that the final decision just had to rest very largely on his judgment. I will admit his judgment was confirmed by Leland, Cowles and others who had taken considerable pains to investigate the candidates personally.' (Filbey to Bartky, December 9, 1942.)

65. Lange said in his letter to Leland that he had stated already in 1938 to Professor of Economics, Chester W. Wright, that Marschak was the ideal man for the position as a research director when the transfer of the Commission from Colorado Springs to the University of Chicago was under discussion. It is unclear when this took place, whether at the end of 1938 in the wake of Henry Schultz' demise or earlier, and why it was told to Chester Wright.

66. It goes without saying that in 1942 all potential candidates not resident in the U.S. were left out of consideration.

67. Marschak to Mosak, November 20, 1942. Mosak published his 1941 PhD dissertation as CC Monograph 7 (Mosak, 1944) but never returned to the Commission as research associate. He resigned formally in 1945.

68. Marschak to Mosak, November 20, 1942. Marschak wanted hypothetical differential equations with random coefficients to be set up to explain the causation of known income distributions. He had also noted that data in existing income studies 'could be used with much more rational purpose, if thought out more carefully.' A brief note about these ideas appeared as Marschak (1943).

69. Hurwicz had worked on a project titled 'Statistical Methods to Test Business-Cycle Theories' under the direction of Lange. This was another thread from the Tinbergen model. Note that the title of the project title was strikingly similar to that of Haavelmo (1943b). He continued to work on time series problems.

70. Katona completed the price control study with supervision from Marschak as Monograph 9 (Katona, 1945), and then embarked on a new career combining economics and psychology which would make him quite well known. Katona does not figure further in this story, neither does the psychometrician L.L. Thurstone, but in the spring of 1944 while Katona was working on the price control study near the end of his contract, efforts were undertaken to get Katona a position of some kind at the University. But not everyone agreed. As Filbey reported to Redfield: 'Professor Thurstone stopped me at the Quadrangle Club to protest Katona's on the Quadrangles. As I recall his reaction, it was a vigorous one.' (Filbey to Redfield, May 26, 1944.) The often commented strained relationship of the Cowles group vs. Friedman and other Chicago economists hardly stooped to such a level of personal animosity. Katona left Chicago for USDA/BEA and got his own Survey Research Center (U. of Michigan, Ann Arbor).

71. Marschak had by then already arranged for a joint seminar by Haavelmo and Marschak on 'Current Econometric Literature' to be listed in the university catalogue (Marschak to Haavelmo, November 19, 1941).

72. Nortraship, which had office on Broad Street, Manhattan, was run by Norwegian ship-owners under supervision by the Norwegian government-in-exile.

73. Marschak to Haavelmo, June 23, 1942. T. Haavelmo: A note in connection with our discussion on random differential and difference equations, typewritten, 4 pp., June 6, 1942.

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74. See Bjerkholt (2007, pp. 812–813, 2010).

75. Andrews had been a fellow at the University of Chicago 1942/43 and worked only until March 1944 when he became an ensign in the U.S. Naval Reserve. His work with Marschak resulted in Marschak and Andrews (1944).

76. Lange to Haavelmo, June 29, 1943.

77. See Bjerkholt (2007, p. 843). This caused confusion in posterity for historians as Haavelmo appears in the CC reports 1943–45 as research associate with no indication that he lived and worked far away from Chicago throughout those years. See, e.g., Malinvaud (1983, pp. 51, 56).

78. Some of the other statisticians mentioned in the following were also involved with SRG, e.g., Jacob Wolfowitz, Allen Wallis, Milton Friedman, Abraham Girshick. They were probably all recruited through Hotelling who worked 39 months (part time) for SRG.

79. Bartky had taken part in the sixth CC Research Conference in 1940 also attended by Wald (and Haavelmo).

80. Henry B. Mann had given a lecture at the Marschak seminar in New York on the forthcoming paper in April, 1943. The seminar had continued after Marschak's departure. Haavelmo attended the lecture and conveyed to Marschak his impression from the lecture and further talks with Wald and Mann, that the paper was practically finished and that they 'had cleared up everything, practically speaking, at least for large samples, and that the maximum likelihood method seemed to be a good method in all cases.' Haavelmo to Marschak, May 18, 1943.

81. Wald noted that two great names had just retired; the number theorist Leonard Dickson and Gilbert Ames Bliss, known for work on the calculus of variations (Wald to Marschak, July 19, 1943).

82. The production study (281) was published as a long *Econometrica* article (Marschak and Andrews, 1944), while nothing appeared on estimation part of 280.

83. The rejected paper appeared eventually in Hendry and Morgan (1995, pp. 390–398); Bjerkholt (2007, note 47).

84. See Bjerkholt (2007, p. 792).

85. Frisch penetrating 'autonomy' note arrived too late for the Cambridge conference convened to discuss Tinbergen's League of Nations study. Tinbergen suggested to publish Frisch's note annexed to his study but his advice went unheeded. Haavelmo (1941) elaborated at length upon 'autonomy', citing the Frisch note, see Boumans (2010).

86. The outline note itself has not been found.

87. Koopmans also wrote that he had been working on 'a somewhat wild, but administratively simple, plan to prevent a postwar inflation of the 1919–20 type' and asked for comments on an attached draft. Marschak dealt with it thoroughly in a separate letter, summarized Koopmans' ideas in five points which he commented upon in detail, ending in the somewhat dismissive conclusion: 'As a result, some readers, dissatisfied with the other points, reject the whole scheme' (Marschak to Koopmans, September 14, 1943). The draft is likely to have been the paper by Koopmans which appeared in AER (33(4), pp. 882–888).

88. Note Marschak's formulation that Mann and Wald had been 'urged' to write Mann and Wald (1943). Urged by whom?

89. Epstein (1989, p. 61) asserts that Marschak after becoming research director 'planned to devote all the resources of the Commission to develop the work of Tinbergen in the light of the works of Haavelmo and of Mann and Wald' with reference to a letter by Marschak to Joseph H. Willits of the Rockefeller Foundation, June 17, 1946. But there is no support for this assertion in the letter. Instead Marschak's four points may indicate familiarity with Frisch's 1937 Memorandum discussed in Section 2.

90. Epstein (1989, p. 101) suggests that Koopmans at this seminar presented 'in crude form' the essentials of Koopmans, Rubin, and Leipnik (1950) but that seems to have no justification. Koopmans' seminar was more obviously related to topic (1) in the letter to R.L. Anderson as cited below.

91. Marschak to R.L. Anderson, December 28, 1943. Again it seemed like re-doing (and perhaps rehabilitating) Tinbergen's study!

92. The note titled *Research Program in Statistical Methods Relating to Time Series* distinguished 'A (Asymptotic) large sample theory, B (Exact) small sample theory, and C Medium size sample

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theory (based on chosen asymptotic approximation)' and may be regarded as the first sketch of the statistical methods program actually pursued at CC.

93. R.L. Anderson to Marschak, April 18, 1944. At Princeton Anderson worked for the Office for Scientific Research and Development (OSRD) which just at this time initiated the infamous use of conscientious objectors as guinea pigs for everything from extreme temperatures to poison gas, but perhaps not in Princeton.

94. Haavelmo to Marschak, September 24, 1943. Haavelmo mentioned as a third alternative to retain the original Haavelmo (1941) title.

95. It seems to have passed unnoticed that Haavelmo in 1941 indeed anticipated the change in the Cowles motto ten years later from 'Science is Measurement' to 'Theory and Measurement'; cf. the title of Christ (1952) *Economic Theory and Measurement*. But Haavelmo may well have borrowed his title from Henry Schultz' *Theory and Measurement of Demand* (1938).

96. With the revised titles it was listed as forthcoming in the CC Report for 1943.

97. See Hotelling (1940).

98. Wald's involvement with SRG and the value of his work there may have made it difficult for Wald to extricate himself for a move to Chicago. Part of Wald's work at SRG was on estimating the vulnerability of various parts of an airplane from data showing the number of hits of planes returning from combat. The military was inclined to provide protection for the parts showing most hits. Wald, assuming that hits in combat were uniformly distributed, recommended the opposite, the reinforcement of areas with the least frequency of bullet holes. The downed aircrafts, not in the data set, were the ones that had received the fatal hits in areas free of bullet holes in the surviving planes, see Wallis (1980b, pp. 334–335). Wald's reasoning is asserted to have mirrored that of Sherlock Holmes famously solving a case by a critical piece of evidence, a guard dog that did not bark, see http://www.dangreller.com/the-dog-that-didnt-bark-2/.

99. Wald to Marschak, March 5 and April 9, 1944; Bartky to Marschak, March 16 and April 20, 1944.

100. Marschak to Hurwicz, June 26, 1944.

101. Koopmans to Haavelmo, October 27, 1944. The paper for the 1945 conference was titled "The bias in single-equation methods for estimating behavior equations relating to a small sector of the economy." It was not included in Monograph 10 but was issued much later in *Scientific Papers of Tjalling C. Koopmans* (New York: Springer-Verlag, 1970).

102. Marschak to Deming, March 26, 1945; Deming to Dr. White, War Department, March 29, 1945; Marschak to Wilks, April 4, 1945; Wilks to Marschak, April 9, 1945; Deming to Marschak, April 10, 1945.

103. Marschak to Commanding Officer of the 468th Quartermaster Deport Company at Camp Shelby, Miss., September 15, 1945.

104. Epstein (1987, p. 62) denotes Klein 'the most junior at 24' of the group, overlooking that Rubin was only 17.

105. Klein's presentation was straight out of his Ph.D.-thesis completed the same year and resulting in his 1947 book *The Keynesian Revolution* (New York: Macmillan).

106. Klein recounted many years later that during the Cleveland meeting Marschak had said to him: 'what this country needs… is a new Tinbergen model, a fresher approach to it' (Klein and Mariano, 1987, p. 412).

107. Personal information from Lawrence Klein 2005.

108. Klein to Marschak, October 16, 1944; Marschak to Samuelson, October 25, 1944; Samuelson to Marschak, October 28, 1944; Marschak to Klein, November 6, 1944.

109. The acronym SSRC was in use both for the Social Science Research Council, founded as an independent, nonprofit international organization in 1923, and by the University of Chicago's Social Science Research Committee.

110. At the beginning of 1947 Marschak again wrote an evaluation of Klein for an SSRC fellowship for 1947/48. He referred to the ratings and general remarks he had given two years earlier and found confirmed by and large on nearer acquaintance: 'I may merely add … that there is somewhat more evidence of partisanship that I had observed at that time. It is for this very reason that I think a trip

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to Europe… will be very beneficial to Klein, who will learn first-hand from facts.' (SSRC report by J. Marschak on L.R. Klein, February 1945.) Klein got the fellowship and chose to spend most of the time with Frisch in Oslo and also visited Tinbergen in the Netherlands. When he applied he had also considered visiting Soviet Russia.

111. The information is from the CC Research Report for 1943. In the 1944 Research Report the Advisory Committee was stated as comprising only the six names not associated with the Commission but that may have been a mistake. Quarterly meetings of the Advisory Committee were introduced in 1944. The old Advisory Council was removed like cutting the umbilical without notifying the Econometric Society which had nominated the members. The Econometric Society thus was deprived of the opportunity to revitalize its close relation with the Commission after the war was over. The interpretation can hardly be other than that Cowles was willing to sever the links with the Econometric Society to further Commission's position within the University of Chicago.

112. Filbey to Brandt, February 10, 1945; Brandt to Filbey, April 30, 1945; Filbey to Brandt, May 1, 1945.

113. The title of CC Monograph 10 was decided in 1945 (after—or perhaps during(?)—the conference), and indeed also the number, cf. CC report 1945. Seven CC Monographs had been published 1939–45, but then followed a five year gap between monographs no. 9 and 10! Printing queue seems to be a bit too tepid explanation of this striking fact. Was Koopmans' straitjacket notation system part of the explanation, or perhaps the change of publisher?

114. Wald was a prolific and productive researcher with articles in 10 out of 12 issues of *Annals of Mathematical Statistics* 1942–44 on top of other heavy commitments.

115. Wald's work on decision theory had begun before his involvement with SRG, as Wallis (1980b, p. 334) recounts: "When Savage first joined SRG, I introduced him to Wald at lunch one day. Wald discussed some of his ideas on decision theory and Savage, who was a former research assistant of von Neumann's remarked that he knew a rather obscure paper that would interest Wald, namely von Neumann's 1928 paper on games. Wald laughed and said that some of his ideas were based on that paper." But it may seem likely that some of Wald's experiences in SRG were helpful as well.

116. The two papers were indeed published back-to-back in *Annals of Mathematics* 1945 and both were prominently mentioned in the preface to the second (1947) edition of von Neumann and Morgenstern: *Theory of Games and Economic Behavior* (Princeton: Princeton University Press).

117. In Chicago Wald spoke, however, to the Mathematical Club on January 30, 1945 on the title he had suggested to Marschak for a paper.

118. Madow, not mentioned in Malinvaud (1983), chose to publish his paper separately. Tintner, unmentioned in Epstein (1989, p. 63, note 43), was active at the conference but did not submit anything. Hotelling had also been invited as a discussant and submitted a very short paper on matrix inversion methods.

119. 'If Frisch's statistical model … is accepted, I think it will be very difficult to construct any realistic economic theory that would imply such multicollinearity in the strict sense' (Haavelmo, 1950, p. 262). As Haavelmo on a number of occasions during his first years in USA had taught or discussed confluence analysis his chosen topic is likely, knowing Haavelmo's way of developing an idea, to be something he had had in his mind for some time, perhaps since he was teaching confluence analysis at Harvard and Michigan in 1941 and chose to write it out for the occasion.

120. The use of Gramian determinants in Haavelmo (1944) is one of the points on which Haavelmo received advice from Wald (Bjerkholt, 2007, p. 801).

121. Keynes' remark was in his comment to Tinbergen's response, Keynes (1940), and, indeed, is a curious expression of praise of Tinbergen: 'There is no one, therefore, so far as human qualities go, whom it would be safer to trust with black magic' (Keynes, 1940, p. 156).

122. Koopmans (1945) is likely to have been the first announcement (in footnote 16) of the forthcoming Monograph No. 10. In his discussion of incomplete systems, Koopmans cited one of Wald's two contributions but unfortunately the wrong one.

123. It can be noted here that Haavelmo's first internationally published paper was also about incomplete systems (without the term), but in a different analytical setting, see Haavelmo (1938).

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124. On Friedman's statistics, see Stigler (forthcoming), who concludes his paper as follows: 'Up to 1945 Milton was both a statistician and an economist, and his work was fairly evenly divided between the two fields. … After 1946 he was really simply an economist, albeit one with an acute statistical understanding underlying much of his work. Had he taken a different route in 1946, he could well have become one of that century's major statisticians, although that was apparently never an option he seriously considered.'

125. The undisclosed mathematical statistician can be identified as Maurice S. Bartlett, who was invited by Hotelling to Chapel Hill in 1946 with an offer to stay, see Olkin (1989, 156). I am grateful for most valuable help from John Aldrich and Stephen Stigler for this identification.

126. Information and quotes from Wald to Marschak, October 13, 1945.

127. The rhetoric used by Marschak here is oddly reminiscent of that of the Frisch article marking his return to the editorial chair: '[econometrics] should only be put into the hands of really first-rate men' (*Econometrica* 14(1), 4) which had just appeared.

128. One may wonder why Marschak wanted this assessment. Both Koopmans and Friedman were at the time in the picture for tenured positions at the Department of Economics. For Marschak it was vital that Koopmans got tenure. Koopmans was appointed in 1946, Friedman in 1947.

129. The Business School had two vacancies, after Joel Dean and John H. Smith, while they both were CC research associates. Both had attended the sixth CC Research Conference with Haavelmo, and now worked for OPA and BLS, respectively.

130. Again it was the unavailability of funds which prevented Marschak from giving Haavelmo a direct offer.

131. See Beneke (1998).

132. On the first day of the meeting October 22, 1945 Haavelmo gave a regular CC seminar at 8 p.m. on the balanced budget multiplier; the paper already in print in the October issue of *Econometrica* 1945.

133. T.W. Schultz to Louis Wirth, Secretary of SSRC, October 25, 1945. In a further note of October 31, 1945 it was made clear that the Cowles Commission was not able to underwrite this item and Haavelmo's salary of $4000 would come out of Schultz' project. Thus Haavelmo is likely to have been the only CC research associate who never got any remuneration from the Cowles Commission.

134. In the CC Report for 1945 the announcement of the project spoke of Haavelmo as 'one of the first writers to express doubts as to the applicability of traditional regression methods in studying economic relations.' It was a characterization that fitted as well for Ragnar Frisch, cf. Bjerkholt (2005, 495).

135. The Girshick letter is discussed at some length in the ET interview with T.W. Anderson (Phillips, 1986), and in Epstein (1989).

136. Epstein (1989, p. 103) points out that Girshick's stylized model was identical to Tinbergen's 1930 model but Girshick stated that the model was taken from Koopmans' just published expository paper Koopmans (1945, p. 452).

137. At this early stage Koopmans seemed to have nurtured reservations about the new method. He argued that if it were applied properly it would at best not be cost saving in terms of computations.

138. The synoptic table had columns marked "Reduced form method" and "Maximum likelihood method", respectively and row entries for I. Linear systems; II. Nonlinear systems; III. Incomplete systems. Thus, the top row, "linear systems, all equations just identified" was marked as "equivalent" in both columns, while the row, "linear systems, some equations overidentified", had entries "loss of information" and "presumed 100% efficient", in the two columns, respectively. The bottom row was "incomplete systems", marked with "available" in the reduced form column and "not applicable" in the maximum likelihood column.

139. Hildreth (1986, p. 7) noted that the rule later became known at UCLA as the 'Marschak Rule'.

140. As Marschak had stated at the commemoration of the tenth anniversary of the Econometric Society in 1940: 'The fruitfulness of frank and energetic discussions found expression in the customary rule of the meetings of the European branch: everyone may interrupt the reading of the paper to ask the author to repeat or clarify a definition – a rule which has probably prevented many a discussion

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at cross purposes, so common in economics' (*Econometrica* 9, p. 179). Marschak also spoke of the European meetings of the ES's 'ideal of long thinking and short speaking', which was perhaps a fitting description for the CC staff meetings as well.

141. Anderson's dissertation on the noncentral Wishart distribution had involved him with multivariate statistical problems with a matrix of means of rank lower than the dimensionality of the number of vectors. This was closely related to what was needed for the single equation maximum likelihood estimation he was confronted with at Cowles Commission, Phillips (1986, pp. 259–260).

142. But even here there was a touch of Wald. He had pinpointed Anderson as the right person to hire. And perhaps contributed a clue to the "black magic"?

143. It is a bit inexplicable that the Anderson and Rubin paper was not published until three years later. Anderson and Rubin (1949) did not use 'LIML', neither as acronym nor written out, nor did they use "reduced rank regression" although the paper is regarded as the origin of these concepts in econometrics.

144. At the time Arrow had another thesis project in mind than the one which resulted in CC Monograph 12.

145. Koopmans had met von Neumann already in 1941, perhaps at an IMS meeting, see Koopmans (1942, p. 24).

146. Klein got the impression that Frisch was more sympathetic with the NBER approach and even hinted at Haavelmo's stubborn attitude. Personal information from L.R. Klein.

147. Beulah Midgett was Haavelmo's female companion. They had met soon after Haavelmo arrived in the U.S. in 1939. She followed Haavelmo to Oslo and survived him with a couple of years.

148. Personal communication from Stein Rossen.

149. Klein sent the following field report: 'The Norwegian National Budget for 1948 has just come out, and Trygve certainly did a nice job on it. I don't think the average person realized how much his life is affected by Trygve's decisions' (Klein to Marschak, February 13, 1948). Klein wrote on his observations during his visit, 'Planned economy in Norway' (AER 38, pp. 795–814).

150. Personal communication from Lawrence Klein.

151. 'I recently received an invitation from the Dean of your Faculty… I presume that you submitted my name to him… I would certainly very much like to spend a few months with you and your colleagues there' (Wald to Haavelmo, October 31, 1950).

152. Howard Bowen was appointed dean of the College of Commerce, University of Illinois in 1947. His tenure was a good one with improvements in many different programs but in 1952 Bowen had to resign from the University of Illinois because senior professionals did not like his curriculum, particularly not the Keynesianism that came with it. See Solberg and Tomilson (1997), Darity et al. (2012, p. 38). Also Haavelmo was offered a position at the University of Illinois by Howard Bowen but was already in Oslo when it reached him.

153. Haberler's remarks were not in the *Econometrica* report from the meeting, only in AER 39(3), 84–88. Haberler also hinted at the sharply worded review Edwin B. Wilson had given of Haavelmo (1944).

154. Koopmans who is likely to have retained his antiMarxian stance from two years earlier may have been surprised to find that the opening session on December 29, 1948 of the American Economic Association meeting he attended was a grand centenary commemoration of the *Communist Manifesto*, under the auspices of President Joseph Schumpeter and Vice-President Simeon Leland.

155. The research resulted in *Economic Aspects of Atomic Power*, An Exploratory Study under the direction of Sam H. Schurr and Jacob Marschak. Princeton: Princeton University Press, 1950 (289 pp), with the content described as an analysis of the potential applicability of atomic power in selected industries and its economic effects in both industrialized and underdeveloped areas.

156. But speaking from inside the Cowles Commission/Foundation Reynolds chose his words about the relocation differently: 'this very important center for the study of economics is returning home, since Mr. Cowles himself is a Yale alumnus.' (Cowles Foundation web site 'The Move to Yale.')

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