

Lawrence R. Klein 1920-2013: Notes on the early years

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Abstract

Lawrence R. Klein can well be said to have created the field of macroeconometric modelling and his international influence started at an early stage. The article offers scattered observations on Klein's early years from undergraduate study to the University of Pennsylvania in 1958. Klein was in 1944 hired by the Cowles Commission in Chicago to construct the first macroeconomic models in the USA, drawing on the experiences of the interwar modeling work of Jan Tinbergen and the new path-breaking econometric methods developed by Trygve Haavelmo. The first Klein model was taken into use at the end of 1945 to shed light on the prospect for the US economy in the transition from war to peace. After three years in Chicago, Klein travelled for a year in Europe and initiated macroeconometric modeling work in Canada, the University of Michigan, and Oxford University. This was only the beginning of the lifelong influence Klein exerted on modelers around the globe. The article pays attention to Klein's relations with Paul Samuelson, Jacob Marschak, Trygve Haavelmo, Ragnar Frisch, and others.

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1. Introduction*

Lawrence R. Klein died on October 20, 2013. He was in 1980 awarded the Sveriges Riksbank Prize in Economic Sciences in Memory of Alfred Nobel 1980 “for the creation of econometric models and the application to the analysis of economic fluctuations and economic policies.”¹ With Jan Tinbergen’s pioneering model building effort in the late 1930s as a strong inspirational source Klein was the first practitioner of macroeconomic modelling and he can well be said to have created this field in economics. With the modelling came also forecasting and policy analysis. Klein remained a leading force and carried the torch to inspire or actually initiate macroeconomic modelling to a remarkable number of other countries. The article offers scattered observations and archive information on Klein’s early years and experiences from undergraduate study years until his home base became the University of Pennsylvania from 1958.

Lawrence Klein was born in Omaha, Nebraska in 1920. After undergraduate studies in California and PhD at M.I.T. followed intensive and exciting years at the Cowles Commission in Chicago. Klein’s early work at Cowles Commission in Chicago 1944-47 was followed by involvement in a series of modelling projects in various countries with a number of co-operating partners. The first project for the Canadian government in Ottawa opened up macroeconomic modelling in Canada. The followed a travelling year in Europe with Sonia Klein (formerly Adelson). Most of the time was spent at the Institute of Economics in Oslo in close contact with Ragnar Frisch and Trygve Haavelmo, and at Tinbergen’s Central Planning Bureau in The Hague. Klein returned to USA to work at the National Bureau of Economic Research in New York and then at the University of Michigan, Ann Arbor followed by an abrupt move to Oxford in 1954. From 1958 Klein’s home base was the University of Pennsylvania at which he created a center for macro modelling attracting visitors from many countries.

After 1958 Klein inspired and influenced modelers and model users all over the world. At his home base he was in charge of a series of Wharton School models. He was an initiator of the huge SSRC-Brookings model project and the multinational Project LINK, both started in the 1960s, and much more. Friends from the early years called him Lawrie (in varying spellings), in later years Larry may have been more common.

2. From MIT to Cowles Commission

Klein studied mathematics in Los Angeles City College 1938-40 and continued with that at UC Berkeley 1940-42 adding economics to his portfolio. It was an unusual combination at the time. Klein credited teachers he met at UC Berkeley, William Fellner and Norman S. Buchanan, for having convinced him of the usefulness of studying economic problems, and a third teacher, Struan T.

* The article draws on correspondence from the Trygve Haavelmo and Ragnar Frisch archives at the University of Oslo, and from the Jacob Marschak papers at Young Research Library at UCLA. I am most grateful for valuable assistance from Professor Emeritus Kanta Marwah who managed to have the Albert G. Hart 1976 letter (cited below) retrieved from the University of Pennsylvania archives. I thank Ronald G. Bodkin and James Heckman for comments.

¹ This was the first and almost the only time “econometric” has been used in a Nobel Laureate caption. Some doubts linger about the use of this term, however, as the press release of October 15, 1980 stated that the Prize was awarded “for the creation of economic models ...”, see http://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1980/press.html. Perhaps a last minute reformulation?

Robertson, for having interested him in the writings of Keynes. There is a glimpse of Klein in Constance Reid's biography of Jerzy Neyman. One of Neyman's students, Francis Dresch, noted Klein's presence at a seminar in economics given by Neyman in Griffith Evans' home (Reid 1982, p.168).

Klein was accepted for the recently opened Ph.D. program in economics at MIT from 1942 and got Paul Samuelson as supervisor.² Klein chose Keynesian theory as topic for a PhD thesis. Klein had studied statistical theory as well but realizing that it might be worthwhile to delve even more deeply into it, he and his class mate at MIT, Joseph Ullmann, arranged a series of seminars in mathematical statistics with external speakers. They drew up an impressive list of possible speakers, including Trygve Haavelmo.³ Ullmann wrote to Haavelmo at the beginning of March 1943, told him that some of the students at MIT had got hold of Haavelmo's dittoed thesis *On the Theory and Measurement of Economic Relations* (Haavelmo 1941) and were interested in Haavelmo's ideas about testing of hypotheses and estimation.⁴ Haavelmo, who at the time worked in New York as statistician and analyst for the war administration of the huge Norwegian merchant fleet, accepted the invitation right away, deciding the title of his lecture to be *Some Problems of Statistical Inference in Relation to Econometrics*. The first meeting between Klein and Trygve Haavelmo thus took place in March 1943 and in view of later events it may be deemed an important event in the development of Klein's econometric maturity and interests.

Klein had also started to read Haavelmo (1941) when Haavelmo visited. Klein and Haavelmo had a talk during the visit in which Haavelmo explained the main ideas in his article on the "statistical Implications of simultaneity" (Haavelmo 1943).⁵ Klein wrote to Haavelmo shortly after the seminar to ask his advice about estimating parameters in a simultaneous system of differential equations. Klein used his newly acquired insight later the same year in a critical review of an attempt by Mordecai Ezekiel to estimate an investment function (Klein 1943), drawing on the reasoning in Haavelmo (1943) and citing also the poorly distributed Haavelmo (1941).⁶

Towards the end of his MIT studies Klein took part in the Econometric Society meeting in Cleveland, September 13-15, 1944. It was the first Econometric Society meeting in the USA since before Pearl Harbor, as the annual meetings in 1941-43 had been cancelled because of the war. Klein's paper for the meeting was based on one of the chapters of his (almost completed) thesis titled "From the Treatise to the General Theory: A Study in Keynesian Economics". Jacob Marschak, who since the beginning of 1943 had been the research director of the Cowles Commission at the University of Chicago, chaired the session with Klein's paper. Marschak was in fact in the process of assembling a research staff for the Commission for an econometric research program, see Bjerkholt (2015). At the time of the meeting the entire team comprised Marschak himself, Tjalling C. Koopmans, and Leonid Hurwicz. Koopmans had joined Cowles Commission only a few weeks earlier, while Hurwicz had

² Ball (1981, 81) oddly remarks that "Samuelson at the time was only about two years older than Klein", as the age difference was then and always five years.

³ Klein wrote many years later a documentary account about the seminar series in statistics at MIT, Klein (1991), published together with Paul Samuelson reminiscences from attending the seminars, Samuelson (1991).

⁴ The thesis was a preliminary version of Haavelmo (1944).

⁵ Private communication from Lawrence Klein to the author 2006.

⁶ Another 1943 paper was an outgrowth of the Berkeley year with George M. Kuznets as co-author on the market for lemons, "A Statistical Analysis of the Domestic Demand for Lemons" (Giannini Foundation of Agricultural Economics, Berkeley, June, 1943).

been at the Commission one year ahead of Marschak; before that he had been Paul Samuelson's research assistant at MIT. Both Koopmans and Hurwicz attended the meeting. Klein reminisced many years later that Marschak had said to him during the meeting that "what this country needs...is a new Tinbergen model, a fresher approach to it" (Klein and Mariano 1987, p.412).

The Tinbergen model mentioned by Marschak was an econometric model for the U.S. economy completed by Tinbergen in 1938/39 as part of an assignment for the League of Nations in Geneva on the study of business cycles. For devoted European econometricians Tinbergen's modelling work was an impressive pioneering work, an empirical study, both macroeconomic and econometric, opening up a new direction in economics, carrying the hope of a breakthrough in the understanding of economic fluctuations. The League of Nations project had started around the time when J. M. Keynes published his *General Theory*. Keynes was no less concerned about economic fluctuations but was deeply critical about the value of the outcome of Tinbergen's work which he had reviewed in *Economic Journal*. It was a sharp criticism not only of the specifics of Tinbergen's study but to some extent on the whole econometric methodology it represented. For Marschak and Koopmans this was a challenge to be taken most seriously. They were devoted econometricians and close to Tinbergen. Marschak and Tinbergen were among a small group who had attended all of the first 6-7 meetings of Econometric Society in Europe. Koopmans had been Tinbergen's student and had taken over his assignment for the League of Nations. At the same time Marschak (and Oskar Lange) had recognized that Keynes' criticism hit at some weak points in Tinbergen's work. It became therefore of utmost importance for Marschak when he became research director of the Cowles Commission to demonstrate that macroeconometric modelling was a most promising new econometric approach and that Keynes' criticism could be overcome. In fact a "Tinbergen type model" was at the center of the research program Marschak outlined for the Cowles Commission.⁷

The last night during the Cleveland meeting the Cowles Commission trio invited Klein to have dinner with them. Marschak asked Klein directly whether, when he had finished his PhD degree, he would consider a position at the Cowles Commission to work with Koopmans and Hurwicz and possibly others in an effort to develop a new and better Tinbergen type model. Marschak also said that he had thought there was a good chance that Haavelmo would join the Commission as well. The division of labor would be that Klein should be in charge of estimating the model, while Hurwicz worked on the theory and Haavelmo and Koopmans took care of the statistical methods.⁸ The offer appealed immediately to Klein, although his prior knowledge about Tinbergen's work may have been limited. But it was something that ought to be very carefully considered and he did not commit himself on the spot.

Klein's finished up his PhD dissertation shortly afterwards, it is dated October 9, 1944. He wrote out over 200 pages during the ten first months of 1944, titling it *The Keynesian Revolution*. It can be

⁷ Marschak had visited Tinbergen's group in Geneva while the modeling project was going on and had also attended the conference convened at Cambridge in July 1938 to discuss Tinbergen's results. Trygve Haavelmo and Abraham Wald whom Klein would meet at Cowles Commission in 1945 had both worked with Tinbergen in Geneva for a period of time, see Bjerkholt (2015).

⁸ Koopmans had offered himself to Marschak as interested in doing the modeling work for an improved Tinbergen type model but had failed in finding a suitable statistician for the team. Instead Koopmans offered to do the statistical work himself, and thus Marschak was looking for recruitment of a model builder at the Cleveland meeting. For further details see Bjerkholt (2015).

noted that Klein's thesis text of 1944 did not contain any occurrences of the terms *macroeconomic(s)* or of *econometric(s)*, suggesting that these were not household words during his study years.

Marschak followed up the good impression he had got of Klein as a very suitable candidate for the work ahead. He wrote to Samuelson to get his assessment. Both Marschak's way of asking and Samuelson's response is of interest here. Marschak first explained to Samuelson what was going on in Chicago:

"...we are working here on 1) statistical method applicable to simultaneous stochastic equations involving time lags and 2) application 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 economic 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 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." (J. Marschak to P. A. Samuelson, October 25, 1944.)

Marschak asked straight out: "Would you regard him (as I am inclined to) as one or the best men of his age available for econometric work of the type described?" Samuelson was positive all along and characterized Klein as "...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." He commented Klein's works and added: "He is certainly the best student that I have had, and I have known very few better ones."⁹

Klein reviewed his options. He was definitely attracted by the task outlined by Marschak but considered the Fed as an interesting opportunity and also work in mathematical statistics of importance for the war effort.¹⁰ He decided on the Cowles Commission offer (although it was not particularly promising neither in pecuniary terms nor in tenure prospects. The CC annual report for 1944 stated that Klein started to work as research associate on November 21, 1944. To begin with he studied Tinbergen's methods as set out in the 1939 volume in close contact with Marschak and the others. The time series Tinbergen had used had been extended from 1919-32 to 1921-41. That was the data set he would work with. The next step was to specify relations in the model within an overall Keynesian framework. Klein set down two principles for the further work. Macro relations had to be rooted in micro theory and aggregation procedures had to be established for the pieces to fit together.¹¹

Later on research associates would often be offered teaching positions in the Department of Economics but that was not the case yet.¹² Klein had been offered three years employment on the condition one year was covered by a SSRC fellowship for which Klein subsequently had to apply. He applied for the fellowship at the beginning of 1945 and Marschak was asked by SSRC for a

⁹ P. Samuelson to J. Marschak, October 28, 1944,

¹⁰ Klein may have known about the Statistical Research Group organized at Columbia and similar units at a couple of other universities.

¹¹ Hence this was the origin of the search for "micro foundations" for macro relations.

¹² Klein did some outside teaching at Roosevelt College in Chicago.

confidential statement. Marschak responded with a characterization of Klein's suitability as a modeler:

"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.

[Klein] 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 for properly assigning each variable to one of the three groups: the 'economic' variables, whose interacting constitute the system in question; the identifiable 'external' variables which strongly affect the economic variables but are not significantly affected by them; and the non-identifiable external variables whose effects are visible only in aggregation, as 'random components' of the system. 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 instincts: it will save him much disappointment. Klein seems to be the type of man who will work overnight and over the week-end if the problem interests him." (Confidential statement to *Social Science Research Council*).

When Klein arrived the CC the research staff comprised Marschak and three research associates. At the end of 1945 and the beginning of 1946 three more research associates were added to the team: Theodore W. Anderson, Trygve Haavelmo and Herman Rubin. In addition there were research assistants, as well as other assistants doing data preparation, computational work and secretarial assistance.

The main emphasis in the research program drawn up by Marschak was the development of a macroeconomic model for the U.S. economy; redoing Tinbergen it could have been called. The model was thought of mainly as a device for providing a better understanding of economic fluctuations, as different from a policy oriented model, the concept of which had been considered by few apart from Frisch and Tinbergen. The research program also emphasized the need for developing the most appropriate statistical methods to be used in the model project. This emphasis on methods was much influenced by Haavelmo's work, particularly the problems relating to the estimation of parameters of simultaneous relations as set out in Haavelmo (1943). It was Marschak who realized the importance of Haavelmo's ideas and brought them into the research program.

Hence the research target became divided, the modelling project to which Klein was assigned, and the "statistical inference in economics" project lead by Koopmans. It was the latter project which became the best known one, resulting in the famous Cowles Commission Monograph no. 10 (Koopmans 1950). Klein's work resulted in Monograph no. 11 which also appeared in 1950 (Klein

1950).¹³ Klein prepared a set of three models: (1) “Model I, A Simple Three-Equation System”, (2) “Model II, A Reduced Form”, and (3) “Model III, A Large Structural Model” (Klein 1950, Chapter 3). Model III was the real model, the others served more pedagogical and methodological purposes. The “large, structural model” comprised 16 equations. The first version of model III was operational in 1945, about a year after Klein had arrived in Chicago.

When Klein died several of the newspaper obituaries made similar statements like: “Lawrence R. Klein ... predicted America’s economic boom after World War II” (NYT, October 21, 2013) and “Challenging widely accepted notions at the end of World War II that predicted a post-war return to economic depression, Klein used econometric models to show the opposite”. Anyone who tries to find a publication by Klein underpinning these assertions will, however, search in vain. So what happened?

As World War II was ending the conventional wisdom in Washington was that the postwar period would drive the American economy back into a long depression. The University of Chicago’s Committee for Economic Development was working on a report on postwar economic strategy. Albert G. Hart who was a staff member of the Committee for Economic Development later recounted:

“We were aware that Klein was working with the Cowles Commission ... building a pioneer econometric model of the US economy. It occurred to us that it would be illuminating to see how far this model would confirm the then existing professional consensus that the prospect was for a major postwar depression with ‘8 to 10 million unemployed’. We were getting indications that on the contrary there might be high activity and inflationary pressure ... but still were much impressed by the arguments of Myrdal and his school that a postwar depression might be avoidable only by heroic measures to stimulate activity. When he agreed to our request for a trial run, Klein indicated that he tended to agree with the school that saw a great tendency to depression; he warned us, moreover, that his model rested on experience under depressed conditions, and might be seen as biased toward predicting only variants of a depression. When he came back ... a few days later, he told us he was surprised. We were struck by the fact that arriving at uncongenial results, he neither suppressed them nor tried to explain them away. On the other hand, given the unripe state of the model and the fact that the ‘data’ consisted in good part of shaky preliminary estimates, he evidently felt no urge to push them to publication.” (Hart 1976; see also Bodkin et al. 1991, 50, note 28.).

Klein recalled the 1945 events in 2006:

“How did things work out after 1945? There was no large increase in unemployment. During that year, after our preliminary calculations with the first version of the Tinbergen-type model at the Cowles Commission, my position was completely changed to one of no immediate serious recession. In office after office in Washington, economic analysts, ... responded to the calculations from the Cowles Commission, ‘Just wait until mid-1946; there will be 6 million unemployed.’ A better response would have been about 2 million.” (Klein, 2006, 174-5).

¹³ Both books were in reality completed in 1947 but severely delayed in a printing queue for about two years before being published.

The general purpose of the modelling project was, in line with Tinbergen's project for the League of Nations, primarily to shed light on the cyclical nature of the economy, see section 4 below. Thus the models were not intended at the outset to be used for policy simulations, tracking exercises or to forecasts, these conceptual ideas were still in embryo, see Bodkin et al. (1991, 42). The 1945 calculations were done with an early version of Model III. A documentary account of these calculations was many years later written out by Hart and submitted to *Econometrica* in 1976 but the paper was rejected and thus never got published.

There is an interesting witness account on these events. Trygve Haavelmo had through 1945 worked in Washington and had been present at some of the presentations Klein had given there. From New Year 1946 Haavelmo was Klein's colleague at the Cowles Commission in Chicago and early in the year he conveyed a report to Ragnar Frisch in Oslo:

"We have just had a quite interesting test of the usefulness of knowing the system of structural equations: The experts in Washington predicted 6 months ago, practically unanimously that the United States now would have recession and something like 8 mill. unemployed. They used a simple and rather superficial *propensity-to-consume* function and guessed at the volume of investment. Mr. Lawrence Klein at Cowles Commission made at the same time a forecast based on a comprehensive system of dynamic structural equations and used refined statistical methods. His forecast was that we by now would have a strong inflationary pressure and almost no increase in unemployment. His forecast proved to be one of few, if not the only one, which hit the mark. One should perhaps not make too much out a singular success like that, but it offers some hope." (T. Haavelmo to R. Frisch, February 21, 1946, transl. by the author)

Klein's forecast was far from a properly undertaken *ex ante* forecast and that was among the reasons why Klein refrained from publishing the forecasts, as explained by Hart. In fact, properly undertaken forecasting was not done by Klein until around 1953. Klein's attempt at calculating the economic effects of the transition from war to peace with an early version model and shaky data showed convincingly enough that the pent-up consumer demand outweighed the sharp reduction in armaments and other war related demand with regard to employment effects. It was in any case a valuable experience in pointing out the tasks ahead.

Trygve Haavelmo conveyed to Frisch some further thought to the problem:

"If one knew the system of structural equations (including some properties of the stochastic shocks that enter), one must first and foremost be able to calculate all effects and side effects of changes in the parameters under government control. If however the ambition is to control new parameters which earlier were structurally determined, some of the structural equations must be broken up. The choice of structural equations to be eliminated is a political and social problem, but the economist could perhaps exert a certain influence on the choice by demonstrating the consequences of the various alternatives. It is also possible to take economic theory one more step and construct something like a 'social choice theory'. I would like to take a closer look at this latter problem if I can find time." (T. Haavelmo to R. Frisch, February 21, 1946, trans. by the author).

Frisch responded a few days later with enthusiasm over what Haavelmo had conveyed about Klein's work and invited Haavelmo to prepare a survey for *Econometrica* about the problems of managing the macro economy and the use of structural equations for that purpose. Frisch suggested a "mapping of the problems without pretending a final solution of all issues" and "a pointing out of the direction to pursue" (Frisch to Haavelmo, March 2, 1946).

Haavelmo mentioned the idea of a survey to Marschak who liked it but advised caution in handling the issue. Haavelmo explained to Frisch:

"The situation is the following: I mentioned that the experts in Washington hadn't done so well in prediction while the informal and unpublished results from the Cowles Commission were much better. It could thus seem as if the problem was to convince the Washington experts. The crux of the matter is however that these experts are a bit on the defensive relative to large third group, namely people who are adverse to formulae and numerical calculations. This latter group really got going when the expert calculations flopped and they do not distinguish between different groups of formula makers. They rather reason that the more intricate the worse the outcome. In reality the Washington experts, which after all is an influential comprising many very clever persons, are very interested in adopting more sophisticated methods. There is a great interest among them to know more about the Cowles Commission methods and there is already much cooperation. Under these circumstances Marschak reasons – and I have to agree with him – that it is of very great importance how a survey article is undertaken, especially when it is coming from the Cowles Commission. I thus cannot promise anything in the very short run." (Haavelmo to Frisch, March 27, 1946).

Haavelmo's survey article never appeared but Haavelmo's observations in Washington and Marschak's briefing provides insight on how the idea of macroeconometrics and model spread from the research frontier to policy-makers. Klein's closest contact in Washington was Arthur Smithies who in 1943 joined the advisory staff of the Bureau of the Budget in Washington. Like a number of the Washington experts he had an academic background; Smithies had been professor of economics at University of Michigan. He took a great interest in the modelling work of Klein assisted by Hart and exchanged a number of letters with Klein, particularly on the specification and estimation of investment relations.

3. Marschak's staff meetings

Marschak instituted frequent staff meetings for discussion of ongoing work. Minutes were taken and the role as minute taker circulated. Marschak wrote many of the minutes himself. According to Hildreth (1986, p.7) "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." This rule was in fact something Marschak had taken with him from the early Econometric Society meetings in Europe. Naturally, quite a number of the staff meetings were related to Klein's modeling project.

Below is rendered the minutes from the staff meeting on October 11, 1946.¹⁴ The topic was the specification of money demand in the model Klein was piecing together. Before the meeting he had circulated a memo, *Theory of Demand for Money*. Of the 15 persons attending the staff meeting the following (identified by initials) took part in the discussion: Jacob Marschak (JM), Lawrence Klein (LK), Tjalling Koopmans (TK), Leonid Hurwicz (LH), Trygve Haavelmo (TH), Don Patinkin (DP), Kenneth May (KM) and as a prominent guest. John R. Hicks.¹⁵ The minutes were written by Sonia Adelson (SA), soon to be known as Sonia Klein.¹⁶

COWLES COMMISSION STAFF MEETING, Friday afternoon, October 11, 1946.

I. Should securities appear in the individual utility functions in terms of market value or nominal value?

LK: Market value.

J.R. HICKS and others: Nominal value.

TK: If we use market values and the market values of the bonds change, this must be included as capital gains in income.

LK: Agreed.

J.R. HICKS: Substitute nominal values of bonds multiplied by market prices.

LK: The identity holds regardless of the form in which it is written.

J.R. HICKS: Allow for both nominal value and liquidity value of securities.

JM: The arguments of both the utility function and the production function are parameters of the joint probability distribution of the future yields.

LH: LK assumes money itself has a direct utility distinct from the probabilities of income it creates; in his model there is no primary utility function.

JM: Suggested as Hicks had that bonds be included at nominal values multiplied by its price.

LK: Agreed, but stated that this was merely a matter of notation and that results would remain unchanged.

It was finally agreed that the utility function might be alternatively written with the market values of securities and cash balances deflated by anticipated general price level.

LH: This would assume no money illusion, but is probably more realistic than statement in money values.

KM: Anticipated general price levels should be included in the utility function as parameters.

LH: These parameters might change although the preference function remained constant.

It was concluded that the individual demand equations for goods, securities and cash balanced be altered by inclusion of $P_1 \dots P_T$.

II. The length of the horizon.

LK, KM: Suggest finite horizon.

LH, JM and others: Suggest infinite horizon, with discounted values in far future approaching zero because of uncertainty.

¹⁴ This particular staff meeting was chosen partly because of its general and non-technical nature and absence of formulae. As far as the author knows none of these staff meeting minutes have been published apart from another specimen at the end of Hildreth (1986).

¹⁵ Also present were Dickson H. Leavens, Jan Hartog, Sonia Adelson, Selma Schweitzer and Ursula Hicks.

¹⁶ According to Klein's Nobel autobiographical note he "met and married" Sonia Adelson at the Cowles Commission. Sonia Adelson was among only 15-20 graduate students in economics at the University of Chicago in the last years of the war, see Patinkin (1981, p.6). At the time of the staff meeting Sonia Adelson had been awarded for 1946/47 a graduate Sarah Frances Hutchinson Cowles Fellowship for women awarded by the University of Chicago for students who would be working on quantitative economics or mathematical statistics. Sonia Adelson. In the interim she seems to have worked for NBER as George Stigler in a paper on domestic servants in the United States 1900-1940 (NBER, Occ. Paper 24: April 1946) acknowledged her statistical work.

J.R. HICKS: In actual practice some parts of the plan extend further than other parts.

JM: Have infinite horizon in the utility and the production function; on solving for the present inputs we shall find them least affected by the most remote parameters of the production function; hence we can approximate the solution by ignoring the terms for $t > T$ (horizon).

DP: In practice we write anticipated values in terms of a few lagged values; thus the length of horizon does not enter into the final macro equations.

III. What variables should be included in the statistical macro equations?

LH: How shall the initial conditions enter the macro equations for money? Treat initial money and bonds separately.

LK: The structure of the model demands that money and bonds enter as a sum, because of the way in which they enter the constraints.

JM and J.R. HICKS: Questioned the splitting of M into M1 and M2.

LK: We consider only M2 to be the cash balances in the utility model; and M1 are purely pipeline or transaction holdings.

Everyone agreed that according to the theory Y (income) and P (price level) should be tested as variables in the demand for M2. It was also agreed that this may be helpful in explaining the current relationship between interest rate and cash balances, as compared with pre-war relationship.

LK: The interest rate might also be tested in the equation for demand for M1 since businessmen hold large amounts of cash in demand deposits.

As the discussion had not really exhausted the topic at the end of the session, the discussion continued on the following (Saturday) morning with the same persons present:

SATURDAY MORNING, OCTOBER 12, 1946. Discussion of Klein's memo continued.

JM: There is a jump between LK's theoretical and statistical macro-equations. It was agreed that the M1 equation is to include the rate of interest and possibly prices. The M2 equation is to include income, prices, and previous holdings of securities as well as cash balances.

JM and LH: Variables should be retained as long as the coefficients are non-zero, even though they have large standard errors. When variables are very large (e.g. inflation) small coefficients may prove important.

TK: We must always keep in mind the objective of the study in the decision as to whether variables should be rejected or retained.

TH: Obtain independently the demand for securities and then check the consistency with the demand for money.

J.R. HICKS: Include the rate of interest in time deposits in the demand equation for M2.

JM: We need a utility theory for M1, we cannot merely say it is pipeline stocks: velocity does change.

(Remark to the minutes by JM): I remember the following interesting statement by LH which seems to belong here.

LH: Because data on demand deposits vs. time deposits are available, we are tempted to identify them with pipeline stocks vs. other stocks. This is hardly justifiable. Fortunately, no such temptation exists in the case of commodity inventories.

TH: Introduce lagged M1.

JM: In the theoretical equation L reduces to government debt plus cash balances. But government debt has been neglected in the statistical equation.

J.R. HICKS: Drop lagged interest rate (from the M2 statistical equation) since it tends to be equal to the non-lagged one.

IV. Supply of money.

LK: Outlined the reasoning behind the last two paragraphs of the memo.

Discussion centered around the form of the adjustment equation and a suitable definition of the supply for money.

TK: Objected to the adjustment equation on the grounds that there are no inventories in the money market.

LK: Disagreed: excess reserves are analogous to inventories.

TH and JM: Substitute for the adjustment equation a general theory of the supply of money based on profit maximizing behavior of bankers and autonomous government action.

J.R. HICKS: Money market is not analogous to the commodity market; there can be an equilibrium position with excess reserves.

The discussion had led from loose initial remarks towards deeper and more intricate points.¹⁷ The minutes caught only a fraction of the words actually spoken. The demand for money was not a new topic but this discussion may well have been the first within a macroeconomic setting.¹⁸

4. Redoing Tinbergen

As noted above Klein did not at the outset have the same close connection to Tinbergen's work and his model on the U.S. economy as Marschak, Haavelmo and Koopmans. Klein met Tinbergen eventually when he visited Chicago and gave two CC seminar lectures at the beginning of June 1946.¹⁹ But there is no doubt that Klein's modelling work at CC was preceded by an in-depth study of Tinbergen's work. CC Monograph 11 opened with the statement, "This book is written in the spirit of Tinbergen's investigations and is intended as an improvement and extension of his results" (Klein 1950, Chapter I), but then notified the reader that in two important respects very important progress had taken place since Tinbergen's project: (1) first, that economic theory was formulated in a more precise and transparent way as a result of the development which followed in the wake of Keynes' *General Theory*, and (2) the second was the Haavelmo view, namely that "we should relate the probability structure to the economic structure" (Klein in Klein and Mariano 1987, p.414). These were decisive points. But neither these points nor other weaknesses found in Tinbergen's work did in any way reduce the high esteem of him and his work at the Cowles Commission.

Tinbergen's mandate for the *League of Nations*, was to understand, i.e. to explain, the economic fluctuations in retrospect. This definition of the task was taken over by the Cowles Commission and made operational in Klein's model, as reflected in the title of Klein (1950), as different from Klein's later models and from postwar macroeconomic modeling in general which typically focused on prediction and policy analysis. Klein made this observation himself in Klein (1950, p.1), perhaps as an afterthought as he finished up the book.

Klein worked systematically and thoroughly and impressively quickly. Important theoretical and empirical results on agents' behavior, aggregation etc. were published in *Econometrica* en route, see

¹⁷ Patinkin (1981, p.247n) remarked in retrospect, "It is noteworthy that the work on the empirical nature of the demand function for money that was done at Chicago during the 1940s was carried out under the inspiration not of 'the Chicago oral tradition', but of the Keynesian model builders at the Cowles Commission', citing Klein (1947a, 1950).

¹⁸ Some other staff meetings discussing Klein's work in 1946 were Jan. 11, 1946 (consumption equations); Jan. 18, 1946 (Klein's use of von Neumann's test for randomness); Feb. 1, 1946 (investment equations); Feb. 22, 1946 (inventories and consumption function); March 29, 1946 (liquidity of firms and investment); June 14, 1946 (maximum likelihood computation for a three equation system); Dec. 6, 1946 (discussion of Klein's letter to Smithies).

¹⁹ The topics did not touch upon modeling however, the lecture titles were (1) *Discontinuous Functions in Economics* and (2) *Theory of Speculative Demand for Raw Materials*.

Klein (1946a, 1946b). Already at the end of January 1947 Klein presented major results of his modelling at an Econometric Society meeting, published as Klein (1947b). The manuscript of Monograph 11 was completed a few months later. A close scrutiny of Klein (1950) gives an impression of the work involved.²⁰

The target pursued by Klein was thus to show that the model could explain fluctuations by means of better and more sophisticated methods than applied by Tinbergen but with time series constrained to 1921-41. This did not succeed very well. The preface in Klein (1950) modestly describe the work done as “tentative and preliminary steps in econometrics model building” and the project as a whole almost as unsuccessful: “It is with all modesty that the research results are now presented in published form, and the main hope is that the work contained here may be of some help to other researchers in the field. If I were to begin today...I would proceed somewhat differently, benefiting from the knowledge of the weaknesses of the models in this volume. I would expect others to do the same.” (Klein 1950, p.vii). This was an unduly modest evaluation of what the Nobel committee 30 years later chose to speak of as “the creation of econometric models.” The gain to be reaped was not in the explanation of past fluctuations but in the experience gathered in the process.

At the beginning of 1947 Ragnar Frisch visited New York on an assignment for the UN and found time for a brief visit to Chicago, where he gave a seminar lecture at CC on *Some Basic Formulae in Demand Analysis*. During Frisch’s visit Haavelmo suggested that Klein might get a fellowship and visit Oslo when his time at CC expired. Frisch who had been quite impressed by what Haavelmo had conveyed to him about Klein’s work, was delighted at the prospect of having Klein as visitor and researcher at the Institute. Klein was interested in getting a better understanding of both Frisch’s and Tinbergen’s activities and thus intended to visit both Norway and the Netherlands. Lawrie and Sonia wanted to combine the visits to the two countries with a tour of other countries as well. Klein’s impression of Frisch during his few days in Chicago was that had limited enthusiasm about the ideas pursued at the time at CC. To Klein Frisch who at the time was deeply involved in the economic postwar reconstruction, seemed more inclined to the way of thinking at the *National Bureau of Economic Research* (NBER). With a recommendation from Frisch Klein got a second SSRC fellowship for his European visit.

During the year or so they had together at CC Klein and Haavelmo got on very close terms. They both shared great admiration and respect for Marschak. Also Ted Anderson got on very well with Klein and Haavelmo. With Koopmans the relation was slightly more distant. Politically, Klein was radical, interested in Marxian theory and in exploring alternatives to capitalism, not least through social and economic planning as a remedy against economic depressions. At CC Klein had expressed an interest in estimating econometric relations based on a Marxian representation of how the economy worked. Koopmans had reacted with a sharp rebuke vetoing (with Marschak’s consent) publication under CC auspices of anything that smelled of Marxism.²¹

²⁰ Klein (1950) is downloadable from <http://cowles.econ.yale.edu/P/cm/cfmmain.htm>

²¹ Koopmans may have feared that CC in some way could become tainted in the political hysteria and political persecution under development and perhaps affected by the fact that Koopmans had held socialist convictions in his youth. But it seemed like a gross overreaction by Koopmans. Also the sharp attack on NBER around the same time was somewhat of an overreaction, perhaps driven by a perceived need for CC to acquire a stronger position vis-à-vis NBER in the battle for future funding. Klein published instead some his ideas in Klein (1947c).

Another person who was close to Klein and Haavelmo in this period was Don Patinkin. He was research associate 1946-47 and completed his doctoral thesis in 1947. In a reminiscence note Patinkin acknowledged Klein for stimulating discussions of his Keynesian revolution monograph manuscript, which had started Patinkin thinking about the problems dealt with in his thesis, and on “providing empirical clothing for Keynes’ theoretical model” (Patinkin 1981, p.14). He likewise remembered the long hours spent with Haavelmo, pipe clenched between teeth, discoursing on a variety of subjects, including the meaning of a derivative in economics, involuntary employment, and Slutsky’s analysis of the generation of cycles by random shocks.²²

5. Klein in Europe 1947-48

In March 1947 Haavelmo left Chicago and returned to Oslo. A professor position meant for him was included in the budget proposal, but after arriving in Oslo he was asked to serve one year under the Minister of Finance in preparing the National Budget 1948.²³ Klein got the SSRC fellowship. Haavelmo sent a letter to the Klein couple about the scarcity of consumer goods in Norway and a brief situation report:

“Thank you for the letter with the good news that we shall see you here soon. This fact is now known all over town here and there is a general conspiracy to exploit you when you arrive so you will probably be kept rather busy when you get here. But I think you will find it quite interesting too. We have now started on the work of setting up the national budget for next year (1948). I am supposed to be in charge of this work and shall probably be in the midst of it when you arrive. I hope it will be of interest for you to look into it too.” (T. Haavelmo to L.R. Klein, May 25, 1947.)

Klein left Chicago in the middle of June 1947.²⁴ Before he started on the fellowship he spent three months in Ottawa as adviser to the Director General of the Economic Research Branch, Department of Reconstruction and Supply of Canada. During this stay Klein initiated macroeconomic modelling in Canada, see Brown (1959).

At Klein’s departure the model project was in jeopardy. In the short run Andrew Marshall who was a graduate student in the Department of Economics worked closely with the Commission for half a year in a study of the Klein model as the subject of his M.A. thesis in economics. Then Carl Christ, whose background was physics and who had been involved at junior level in the Manhattan project, entered the stage. He worked at the Cowles Commission as a SSRC Fellow and inter alia reviewed and revised the model and exposed it to prediction tests (Christ 1949a, 1949b, 1951).²⁵

²² The discussions among them certainly inspired Patinkin (1949) and Haavelmo (1950), see Patinkin (1981, p.155n).

²³ The *National Budget* introduced in Norway in 1946 was not a fiscal budget. By yet another term introduced by Frisch it denoted a white paper presenting a budget in national accounting terms for the whole economy. It was the planning document at macro level.

²⁴ Before Klein left Chicago he prepared and submitted the manuscript for *The Keynesian Revolution*, based on the dissertation at MIT. It appeared in November, 1947.

²⁵ Christ (1951) was when presented met with sharp (but different) criticism both from Milton Friedman and from Klein.

The Klein couple arrived by ship to Gothenburg on November 10, 1947 and continued by train to Oslo. In Oslo Klein became part of Frisch's research group. Frisch involved him in discussions about a wide range of topics. Frisch exploited both Klein and Haavelmo as *Econometrica* referees; they were close at hand and extremely efficient, rarely using as much as a week on a referee report. Klein gave some seminar lectures, including three lectures on *Econometric Tools for Planning* (Klein 1948a). He wrote an essay on *The Case for Planning* (Klein 1948b). Klein took part in a Nordic meeting of economists and conveyed by letter to Marschak his impressions of Scandinavian economists. Ted Anderson who spent a fellowship year Stockholm showed up in Oslo several times for happy reunions with Trygve and Lawrie.

The *National Budget 1948* was published as scheduled in January 1948. Two-three weeks later Klein wrote to Marschak:

“Trygve and I have been working on problems of welfare economics and theories of planning. If we can ever collect all of our results into a well-organized paper we may send it to you. 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 realizes how much his life is affected by Trygve's decisions.” (L.R. Klein to J. Marschak, February 13, 1948.)

The joint paper was never written, however. Klein took a deep interest in Haavelmo's work on practical planning and wrote a very concise and informative paper on Norwegian planning (Klein 1948c). It seems likely that he drew considerably on the contact he had with Haavelmo during the preparation of the national budget. The paper praises the Norwegian planning in general but makes also a number of critical observations:

“A danger which besets all planned economies may be called the problem of ‘the number of degrees of freedom’. There is always the possibility that central planners will try to control too many things at once. Given the technological possibilities of the economy and given the markets that are to be left free, there are only a fixed number of variables at the disposal of the authorities. In the National Budget for 1947, a rather complete national accounting system was utilized to bring about mutual consistency among all the plans, but the definitional relations contained in the national accounting systems are not enough by themselves. In addition, such things as the production functions, consumer demand for unrationed goods, tax laws, the supply of labor, etc., must all be systematically taken care of as side conditions.” (Klein 1948c, pp.811-2).

This can be read as a clear message that planning ought to be supported by econometric models. Klein added that when this aspect of the planning had not been given sufficient consideration in advance it was due to the pressing situation and need for urgent decisions. The remark suggests that steps in that direction had been discussed between Haavelmo and Klein but put aside. Klein praised in the article Haavelmo's 1948 national budget for a more systematic treatment than its predecessor and for having a concluding section stating the planning problem in line with modern welfare economic viewpoints.

Klein noted the lack of emphasis on making the planning policy sufficiently well known for the public and he criticized sharply the use of instruments, as e.g.:

“...taxes have become oppressive. Although taxes are an indirect control, they are criticized more by the general public than many forms of direct, quantity-regulating controls. The government is only fooling itself and not the people if it thinks that indirect controls are not felt.”

“...the government has not aggressively carried out ... [the program] ... for recruiting women into the labor force ... a vigorous approach ... would help to overcome ... the shortage of labor power. The inducement of women out of the home into paid work rests on the supply of social amenities such as cheap laundries, mending services, nurseries, etc., by the government, but this has not been done.” (Klein 1948c, p.812.)

Klein may also have influenced Frisch in his modeling views. Frisch was at the time working on an instrument-target model for his UN assignment. Shortly after Klein left Norway Frisch coined the term *decision model* for the type of model he wanted to pursue and it became something that occupied him for much of his remaining life. Frisch involved Klein in long discussions related to his UN assignment on the situation in the world economy and Frisch’s attempt at using trade matrices as input-output tables to study trade problems under scarcity in convertible currency.

In the late spring of 1948 Lawrie and Sonia took off on a European tour with a longer stay with Tinbergen at his Central Planning Bureau of the Netherlands and shorter visits to Denmark, France, Switzerland and England. He reported his impressions by letter to Haavelmo, comparing his impressions of postwar planning and economic thinking in general in Norway with those of the Netherlands and Denmark. In Sweden he got opportunity to speak at length with four economists who happened to comprise both the chairman (Herman Wold) and the secretary (Ragnar Bentzel) of the committee who in 1980 awarded Klein the Nobel Prize. The other two were the distinguished Stockholm School economists Erik Lindahl and Erik Lundberg. After a brief visit to the United States the Klein couple returned to Norway in September 1948 for another couple of months in Oslo. A minor event during this period was recounted by Klein in retrospect as follows:

“When we returned for a couple of more months in Oslo, Paul Samuelson arrived, just after the successful launching of his elementary textbook. He wanted, very much, to see Frisch, but that could not be arranged. We showed him around Oslo, and his parting words to me were, ‘If I came primarily to see Ragnar Frisch, that part of the trip was a total loss’.” (L.R. Klein in email to author, 2000)

6. Moving on: NBER, University of Michigan, and Oxford

After Oslo the Klein couple returned to New York towards the end of 1948, as Klein had become research associate at the National Bureau of Economic Research (NBER). He stayed with NBER until 1951 but from 1949 he was also research associate at the Survey Research Center, University of Michigan (Ann Arbor). So far Klein had mainly worked with time series data. In Ann Arbor he dug into survey data in a comprehensive study of consumption behavior, not least oriented towards the importance of liquid assets for the consumption demand. From 1950 his affiliation at the University of Michigan was extended to the Department of Economics.

After Carl Christ left CC in 1950 there was no one left at CC working on models. There might have been some mixed feelings about that. An important new field had been pioneered and then abandoned. On the other hand new topics for work at CC were popping up all the time. Klein gave a

seminar lecture at CC on “Sample Surveys and Household Behavior” in May 1950. Koopmans had become research director after Marschak and in talks with Klein during his visit the idea of involving Klein in further modeling work at Cowles Commission was raised. Klein responded a few days later:

“I am playing with the idea of embarking on a complete reworking of the empirical studies on econometric model building and forecasting. I have in mind a much more elaborate project, on the empirical side, than the former research and would like to know if the Cowles Commission has any interest in such a scheme. I think that ample funds could probably be raised freshly for a large scale project along these lines. For the moment I have only vague idea about such future work, but if the Cowles Commission is interested in taking up the econometric model building problem again, the question of my cooperation with you could be raised at a slightly later date.” (L.R. Klein to T.C. Koopmans, May 29, 1950.)

He followed up by sending a sketch of a new modeling project. The Cowles Commission asked Tinbergen and Haavelmo for a review of Klein’s proposal. Tinbergen concluded in a brief statement that the proposal was excellent. Haavelmo wrote that he found the project feasible and that it made good sense. He added a further comment:

“I may perhaps add a word on the subject of choosing a good model: I think that it will be essential to include explicitly in the model certain things that many economists would call ‘non-economic’ factors, such as the repercussions upon population of the economic opportunities in the country, and the growth education as a part of ‘capital accumulation’. I also think that the notion of human inputs of productive effort has to be refined, in particular by distinguishing between those inputs that are ‘productive’ from the point of view of the individual and those that are ‘productive’ from the point of view of the whole society. Finally, I think that, on the demand side, greater attention has to be paid to the fact that in a modern society most of the wants are of a social nature rather than of the Robinson Crusoe type. I am sure that Klein is aware of these ideas – I have discussed them with him before – but I just wanted to take this opportunity of reminding him. (Haavelmo to William B. Simpson, CC, 28. november 1950.)

Cowles Commission considered proposing cooperation with NBER about a modeling project, perhaps because Klein was involved with NBER. In the end Klein turned down the idea of coming back to CC as he already was involved with two other institutions. As a consequence Cowles Commission turned its back on macro modeling, instead pursuing other ideas such as linear programming and game theory which had the advantage of opening up for military support.

There was, however, interest in Ann Arbor to get Klein involved in modeling there. This worked out and resulted in what became known as the Klein-Goldberger model, a further development of Klein’s original model in cooperation with Arthur Goldberger who wrote a PhD thesis related to the model. Computers had played no role in modeling until then. The first model simulations were undertaken on the Klein-Goldberger model 1953-54. The Klein-Goldberger model became an influential prototype for modelers in several countries in the ensuing years.

Klein’s sojourn in Ann Arbor ended after Klein had testified in 1954 in a Detroit hearing that he had been a member of the Communist Party in 1946. The University of Michigan subsequently reneged on its intention of promoting Klein to full professorship. (Mariano 2008). He was not the only future

Nobel Laureate in the USA to be who was attempted victimized by this particular and bizarre American way of political repression. Klein decided to leave for Oxford University where econometrics hardly existed. He found new cooperating partners and under Klein's leadership the first macroeconomic model for Great Britain was developed, published as Klein, Ball, Hazlewood and Vandome (1961). Klein had left already in 1958, however, when he was appointed professor at the University of Pennsylvania.

Marc Nerlove made in the early 1960s a commendable effort at systematizing the properties of a macroeconomic model as a synoptic table. Nerlove (1966) summarized 25 such models, Tinbergen's 1939 model plus 24 postwar models of USA, Canada, the Netherlands, West Germany, Italy, Japan, and India. Six of these models had Klein as (on of) the author(s) of the model documentation but quite a few others had marks of strong Klein influence.

7. Conclusion

The years after Klein moved to the University of Pennsylvania in 1958 became very expansive with regard to macroeconomic modelling. There were still few countries for which there existed a proper model. Klein played an influential role in the dissemination of macroeconomic modeling. At the Wharton School of University of Pennsylvania Klein with others created an international center for macroeconomic modeling. Klein was a central participant in the huge SSRC-Brookings model project in the 1960s. Around 30 econometricians were involved as part of different teams working on parts of the model which comprised 400 simultaneous equations including many non-linear ones. At the University of Philadelphia the Wharton Econometric Forecasting Associates (WEFA) sold model services on commercial terms to government and private clients, plowing the profit back into research, computer development and grants. At the same time Klein with Bert Hickman and others launched Project LINK, models for an increasing number of countries linked together to constitute a global modeling system. After some years Project LINK got UN as one of its key supporters. Klein continued as an indefatigable participant in the semiannual meetings.

Klein's activities after 1958 contributed to opening up macroeconomic modeling much beyond what has been mentioned above. Japan was opened up already in the 1960s, China came later. Klein visited both countries an endless number of times. To a large extent Klein also left very practical advice for those who came after him. A good source on Klein's contributions and activities is Klein and Mariano (1987), while Bodkin, Klein and Marwah (1991) outline main features of macroeconomic model development in a number of countries. Many of Klein's works are published in Marquez (1985) and Marwah (1997). Good surveys of his modeling-theoretic contributions are Bell (1981) and Visco (2014).

Klein's cumulated contributions to econometric modeling cannot easily be summarized. Many surely worked on their own on models without realizing that the techniques and procedures they adhered to and benefited from ultimately had been pioneered by Lawrence Klein. He was generous as a person and as a scholar.

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