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Lawrence R. Klein and the making of large-scale macro-econometric modeling, 1938-1955

Erich Pinzón-Fuchs¹

Abstract

Lawrence R. Klein was the father of macro-econometric modeling, the scientific practice that dominated macroeconomics throughout the second half of the twentieth century. Therefore, understanding how Klein developed his identity as a macro-econometrician and how he conceived and forged macro-econometric modeling at the same time, is essential to draw a clear picture of the origins and subsequent development of this scientific practice in the United States. To this aim, I focus on Klein's early trajectory as a student of economics and as an economist (from 1938-1955), and I particularly examine the extent to which the people and institutions Klein encountered helped him shape his professional identity. Klein's experience at places like Berkeley, MIT, Cowles, and the University of Michigan, as well as his early acquaintance with people such as Griffith Evans, Paul Samuelson, and Trygve Haavelmo were decisive in the formation of his idea on how econometrics, expert knowledge, mathematical rigor, and a specific institutional configuration should enter macro-econometric modeling. Although Klein's identity defined some of the most important characteristics of this practice, by the end of the 1950s, macro-econometric modeling became a scientific practice independent of Klein's enthusiasm and with a "life of its own," ready to be further developed and adapted to specific contexts by the community of macroeconomists.

Keywords: Lawrence R. Klein, history of macro-econometric modeling, history of macroeconomics, history of econometrics, Cowles Commission

JEL Codes: B22, B23, B31, B41

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Lawrence R. Klein y la construcción de la modelización macro-económica de gran escala, 1938-1955

Erich Pinzón-Fuchs²

Resumen

Lawrence R. Klein es el padre de la modelización macro-económica, la práctica científica que dominó la macroeconomía durante la segunda mitad de siglo veinte. En este sentido, estudiar la manera en que Klein desarrolló su identidad como macro-econometrista y la manera en que concibió y forjó una nueva práctica científica es esencial para entender los orígenes y el subsecuente desarrollo de esta práctica en Estados Unidos. Para tal fin, me concentro en estudiar la trayectoria de Klein como estudiante de economía y economista (entre 1938 y 1955) y, en particular, el efecto que tuvieron las personas e instituciones que Klein conoció en esta etapa de su vida para dar forma a su identidad profesional. La experiencia de Klein en lugares como Berkeley, MIT, Cowles y la Universidad de Michigan, así como el haber conocido a personas como Griffith Evans, Paul Samuelson, and Trygve Haavelmo fueron hechos decisivos en la formación de su idea sobre la manera en que la econometría, la experticia, el rigor matemático y una configuración institucional específica deberían hacer parte de la modelización macro-económica. A pesar de que la identidad de Klein definió algunas de las características más importantes de esta práctica, para finales de los años 1950, la modelización macro-económica se había convertido en una práctica científica independiente del entusiasmo de Klein y con una “vida propia”, susceptible de continuar su desarrollo y adaptación a contextos específicos al interior de la comunidad de macroeconomistas.

Palabras clave: Lawrence R. Klein, historia de la modelización macro-económica, historia de la macroeconomía, historia de la econometría, Cowles Commission

Códigos JEL: B22, B23, B31, B41

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Lawrence R. Klein and the making of large-scale macro-econometric modeling, 1938-1955

1. Introduction

Lawrence R. Klein was the father of macro-econometric modeling, a novel scientific practice that dominated macroeconomics throughout the first decades of the second half of the twentieth century. Building on Klein's early work, governmental institutions, university departments, and private organizations significantly contributed to the construction and further development of large-scale macro-econometric models along the 1960s, 1970s, and 1980s. Although Klein's efforts focused on the construction of specific and individual models to intervene and understand the economy, his utmost contribution was the creation of a novel and reasoned way of producing knowledge through the intervention of organized teams of experts who maintained, adjusted, and used these models within an institutional framework to provide contextualized judgments and interpretations on policy and scientific matters. Understanding how Klein developed his identity as a macro-econometrician and how he conceived and forged a new scientific practice of macro-econometric modeling is essential to draw a clear picture of Klein's importance in the creation and further development of this economic practice. To this aim, I focus on Klein's early trajectory as a student of economics and as an economist, and particularly examine to what extent the people and institutions that Klein encountered helped shape his image of economics, his identity as an economist, and the new scientific practice.

To understand how this practice evolved and was disseminated, I provide a characterization of macro-econometric modeling in its early stages, studying how Klein built specific institutional

configurations allowing the practice to have a “life of its own” and to continue its development detached from Klein and under the wing of different institutions and teams of experts. I characterize Klein’s creation as a new way to produce scientific knowledge that consisted in the construction and use of complex tools (macro-econometric models) within specific institutional configurations (econometric laboratories) used for explicit policy and scientific objectives, in which well-defined roles of experts (arranged in scientific teams) were embodied within a new scientific practice (macro-econometric modeling) (Pinzón-Fuchs 2017).

In 1938, Klein embarked in a career as a mathematical and statistical economist in Berkeley that was rapidly reinforced with the completion of his Ph.D. at the Massachusetts Institute of Technology (MIT) under Samuelson’s supervision and with his recruitment at the Cowles Commission in 1944 to build his first macro-econometric model. At Cowles, Klein learned not only cutting-edge econometrics, but also that the building of macro-econometric models was not a lonely exercise but needed the sheltering of an institution, the creation of a team with a specific division of labor, the disposition of material and equipment conditions, and the practice of an ongoing reasoned activity to revise, re-estimate and re-specify the model in the light of new information and data to reach concrete and useful results for policymakers and the industry. Klein was able to assemble all these elements in the University of Michigan between 1951 and 1955 with the creation of the Research Seminar in Quantitative Economics (RSQE) and the consequential development of the Klein-Goldberger model. By the end of the 1950s, macro-econometric modeling was a new and rather autonomous scientific object, prepared to be adapted to specific contexts and further developed both by Klein and the community of macroeconomists working in government agencies, university departments, and private think tanks and institutions. I provide a chronological account of Klein’s intellectual life in which the development of his

identity as an economist and the further evolution of macro-econometric modeling are intertwined.

2. Forging a new identity as a macro-econometrician, 1938-1944

Klein was born on September 14, 1920, in the Midwestern city of Omaha, growing up during a difficult time for the United States. These were the years of the Great Depression, which affected the entire country, hitting particularly strongly the Midwestern states. As documented by John Steinbeck in *The Grapes of Wrath*, hunger, unemployment, displacement, and bankruptcy in the Midwest forced people to move to the West, predominantly to California, in search of better fortune. Although there is no definitive evidence to point at hunger and poverty as the primary reasons behind Klein's move to the West in 1938, California, and UC Berkeley, turned out to be a land of opportunity for Klein. The kind of economics department Klein encountered in the thirties in Berkeley was one where "the graduate students [...] were uniformly radical and the most distinguished were Communists" (Galbraith 1997, 49). Besides, although the economics department had been traditionally strong in agricultural economics, it was in the slow process of becoming an important center for studying mathematical and statistical economics, Klein's areas of interest.

2.1 Mathematical statistics at Berkeley

Since the very first stages of his undergraduate studies first in 1938 at Los Angeles City College then in 1940 at Berkeley, Klein felt a "fascination with higher mathematics" which "blossomed into speculative thinking that could provide a basis for dealing with economic issues" (Klein 1980). Not only did the "teachings of the mathematics faculty [at Berkeley] provide [him] with great stimulus," but "the onset of World War II, with all the associated disturbances leading up to it [also] made a tremendous impression on [his] thoughts about socio-politico-economic interrelationships." In addition, Klein began his studies in economics and mathematics at a time of

important changes both in the US-American university landscape and in the economics discipline (Morgan and Rutherford 1998). Although not dominant at the time (Backhouse 1998), technical teaching that included econometric, statistical and mathematical methods were gradually gaining prominence in undergraduate and graduate economic programs across the country. Yet, these changes in the education and training of US-economists were neither abrupt nor homogeneous. Instead, each academic establishment –especially Berkeley and MIT – underwent a unique process of transformation characterized by its own specificities and leading figures, providing complex stories at the level of each institution and individual.

This ongoing transformation did not occur only at the educational level, however. What was considered scientific and objective in economics was changing too. The new boundaries expanded towards a more technical or mechanical type of objectivity represented in the construction of routinized and standardized practices that attempted to make scientific results and analyses impersonal, neutral, and value-free. Yet, many of the interwar values of pluralism, of “moral committed to ensure scientific inquiry, and [of] evenhanded objectivity” (Morgan and Rutherford 1998) kept playing a major role in the definition of economists’ ethos.

In a sense, both Klein’s socialization as an economist and his educational and training trajectory are representative of the kind of scientific and professional education any student in economics of this generation might have received in institutions such as Berkeley or MIT. This specific way of socialization taught Klein not only how scientific economics was done, but also what should be the role, self-image, values, norms, and behaviors of an economist during the 1940s and 1950s. Klein’s experience is also the story of a personal academic life, and, in that sense, it must be understood in its uniqueness as well. Both the people he encountered and the institutions he visited are part of Klein’s personal process of becoming an economist, of recognizing himself

as such, and of forming (and inventing) his own identity as a rather new type of economist: a macro-econometrician.

At the time of Klein's arrival in 1940, UC Berkeley was not a strong institution in statistics. Only two years before, in 1938 however, Berkeley had hired the already internationally renowned statistician Jerzy Neyman, who wanted to build a statistics program "in his own lines" (Kendall *et al.* 1984, 163), which meant a program with a strong focus on mathematical statistics. According to Francis Dresch, it was only with Neyman's arrival that he and his colleagues started to learn "real statistics together":

[We] had some kind of smattering [...] of somebody else's notion of statistics, on a very elementary level, so we all attended Neyman's lectures [who] established a kind of workshop [which] was a kind of bonehead session on miscellaneous bits of mathematics [...] required for statistics [...]. George Dantzig was running that so-called lab section. [...] Neyman was also conducting a seminar in economics [...] which was held at [Griffith C.] Evans's house [and attended by] Klein (Dresch in Reid 1998, 168).

Besides attending Neyman's seminar, Klein also "worked with [...] Neyman's students," getting acquainted with mathematical statistics and hypotheses testing *à la* Neyman-Pearson from an early stage in his career (Klein and Mariano 1987, 410). The influence Neyman must have exerted on students interested in statistics at Berkeley must have been remarkable. After all, Neyman was one of the most important statisticians in the world, especially after his 1933 publication, together with Egon S. Pearson (Neyman and Pearson 1933), which had marked a milestone for new statistical testing procedures.

One of Neyman's disciples Klein worked with was George M. Kuznets, Simon's bother. In

1941, Kuznets had completed his Ph.D. in psychometrics in Berkeley, and joined the department of agricultural economics, embarking on empirical research to estimate demand functions for lemons in California. At Berkeley, Klein spent one summer (in 1942) working with the Giannini Foundation as an assistant to Kuznets, who “was a very good statistician, though his degree was in psychology” (Klein and Mariano 1987, 411). Despite the differences between the work that Klein performed during that summer, which consisted in an applied microeconomic exercise, and his future macro-econometric work, the experience he gained in analyzing real data marked Klein’s long-lasting enthusiasm for statistical work in economics.

2.2 Mathematical economics and rigor

Another influencing personality for Klein at Berkeley was Griffith C. Evans. With hindsight, “it is not unreasonable [...] to see [...] Klein as linked to [...] Evans” and to argue that Evans’s image of mathematics passed on to Klein, influencing his practices as a macro-econometrician (Weintraub 2002, 71). E. Roy Weintraub (2002, chapter 2) identifies Evans with a tradition in mathematics that presents a close relation with application, going back to the Italian mathematician Vito Volterra and the French polymath Henri Poincaré. For this tradition, “the kinds of values that a mathematician ought to exhibit in his work” were “not just a mathematical sophistication and power of analytical reasoning but a deep and thorough understanding of the scientific basis and connection of those mathematical ideas.” Scientific reasoning, then, should not be based on “the free play of ideas, or axioms, or abstract structures,” but “directly and specifically on the underlying physical reality,” which was “apprehended through experimentation and observation” and was “thus interpersonally confirmable” (48).

These visions are consistent with Klein’s own idea of the use of mathematical tools and statistics. Indeed, Klein placed more faith on the introduction of a broader type of analysis to

improve his econometric results, which included analysis of the “data base, economic analysis (both institutional as well as theoretical), political insight, and attention to the steady flow of information” (Klein 1991a, 113-114), and sustained that “the adoption of more powerful methods of mathematical statistics [was] no panacea” (Klein 1960, 867). To Klein, “if econometric results [were] today more useful than in the past, this [was] only partly a result of the particular method of estimation but much more significantly a product of painstaking research of a more pedestrian nature.” This vision on the use of mathematics was strongly related to an idea of mathematical rigor, where “the mathematical models are not free but are rather tightly constrained by the natural phenomena themselves” (Weintraub 2002, 70). Both in Evans’s and Klein’s understanding, rigor was not provided by abstract ideas, or axiomatization, but by the constraints imposed by the phenomena themselves, which molded the mathematical models. As noted by Weintraub, “Evans’s views on mathematical modeling are the views of an econometrician or applied economist today,” or those of “one who insists that the assumptions and conclusions of an economic model [...] must be measurable or quantifiable” (70).³ To these mathematical economists, the use of mathematics in economics had to be carried out with caution. Rigor, in Evans’s and Klein’s sense, must guide the use of mathematics in economics, and so the use of mathematics must be based on the observed and studied reality. Yet, the economist needed to go beyond the mathematics, since once an

³ Trygve Haavelmo, another influencing figure for Klein, also praised a kind of rigor consistent with this vision. To him, the use of mathematics must be related both to practical application and to observed real-world phenomena, and so “contrary to what many people seem to think, it is in the practical application of theories to facts, in attempts to draw conclusions on the concrete level, that the need for stringent logic and fancy mathematics really shows up” (Haavelmo 1958, 352).

economic theory was expressed in mathematical terms, it could become a rigid structure that did not let any new classes of phenomena enter the minds of the researcher, diminishing his imaginative and creative capacities.⁴

Evans was an “end-of-the-nineteenth-century rationalist, a Harvard pragmatist who [believed] in reason with a human face, and man’s capacity to understand the world in which he [lived]” (Weintraub 2002, 53). Apart from the “end-of-the-nineteenth-century” and “Harvard” bits, Klein was very much like Evans.⁵ He considered the highly sophisticated mathematical and statistical methods that he used throughout his career only as a rational way to understand the world, but never as the ultimate or infallible tool. Despite his use of sophisticated models and methods, to him too, reason had always a human face. Reason was not mechanical, but was always an expert (whether in the form of an individual or institution) who should direct the construction of the large-scale macro-econometric models based on knowledge that goes beyond mathematics and statistics;

⁴ Evans (1930, 110) was particularly worried about this problem, warning economists of the potential dangers of using mathematics to form economic theory: “we must adopt a cautious attitude towards comprehensive theories. They do of course [...] suggest the treatment of particular problems, as well as classify them. Yet this comprehensive character, which they may have as sorts of inductive syntheses of previously studied situations, may precisely in that way circumscribe our ideas, and prevent from entering our minds the observation of other classes of phenomena. We may thus consider only one part of our subject, while we are under the impression that our study is general.”

⁵ Although Klein did not go to Harvard, he maintained a close relation to that institution through Edwin B. Wilson.

and it was also the expert and his team who should discuss and interpret the models' results both to adjust them through reasoned tinkering, and make context-dependent policy recommendations.⁶

2.3 Becoming technical at MIT

After two years at Berkeley, Klein was launched into a career of mathematical and statistical economics. His contact and collaboration with Kuznets, Neyman, Evans, and Dresch literarily opened him the doors of MIT, capturing the attention of the young professor Paul A. Samuelson.⁷ Klein arrived at MIT in September of 1942, as part of the second entering class of the new graduate program of economics, becoming the first economics Ph.D. recipient of this program in 1944 (Klein 1991b; Duarte 2014). MIT had experienced an important restructuring (Backhouse 2014), passing from an “undergraduate engineering school to [a] full-fledged research university” (75) during the 1930s. The economics department, however, lagged some years behind, embarking in this transformation only in 1940. MIT’s inauguration of a new graduate program in economics, its openness to Jews (Weintraub 2014) at a time when anti-Semitism “was woven into the fabric of academic institutions” (Backhouse 2014, 73) and when Harvard was clearly anti-Semite

⁶ This knowledge that Klein and Haavelmo called “a priori knowledge,” contained a great deal of economic theory as well, and a broader understanding of the institutional and historical arrangements of the economy.

⁷ Klein remembers that “in [his] correspondence with faculty and staff in Cambridge, [he] found that [his] work as a research assistant to members of the Berkeley faculty, especially in mathematical statistics and in mathematical economics had been of interest to Paul Samuelson. [Samuelson] was interested in the work of Berkeley professors Dresch [...] and Neyman” (Klein 2011, 502-503).

(Backhouse 2015, 74; Weintraub 2014), together with the development of a specific and more technical way of doing economics (Cherrier 2014), marked the rise of MIT economics.

By 1942, however, even if Samuelson and Harold Freeman were responsible for the branch of mathematical statistics, MIT's economics department was not able to teach advanced topics in mathematical statistics, offering only general courses in these topics through the mathematics department (Klein 1991b, 320). Together with his classmate Joseph Ullman, Klein "felt the need for extra knowledge about mathematical statistics" and decided to organize a series of seminars with external speakers. This is how the Statistics Seminar came to life between 1942 and 1943 (Klein 1991b; Bjerkholt 2014, 768-769; Backhouse 2017, chapter 17). It was on this occasion that Klein met Trygve Haavelmo for the first time, in 1943, marking the beginning of a fruitful and friendly relationship, further cultivated between 1946 and 1948, when they overlapped as research assistants at Cowles, and during the year Klein spent in Oslo, from 1948-49.

Klein was assigned assistant to Samuelson, presumably because Samuelson, impressed by Klein's references and work in mathematical and statistical economics, insisted on keeping him close.⁸ To Klein (1980), in any case, "working as an assistant for Samuelson was something [...] very hard to duplicate anywhere in the world," because "he generates ideas so fast." "It was a very exciting time" characterized by "a whole succession of ideas concerning Keynesian macroeconomics and econometrics and the development of mathematical methods in economics [...] and [Klein] felt very fortunate to be in that background" (411).

When Klein arrived at MIT, Samuelson was predominantly working on consultancy projects,

⁸ For a detailed account on the relationship between Samuelson and Klein in this period see Backhouse (2017, chapter 24).

including one for the National Resources Planning Board (NRPB) (Maas 2014a, 279- 282). Yet, “to a surprising degree, [Klein was] able to go ahead on his own steam in these disorganizing war years” (Samuelson to Marschak, dated October 28, 1944, PASP box 45). Despite Samuelson’s multiple engagements, Klein (2011, 505) remembers that “Samuelson interacted closely with graduate students on a larger and larger scale, playing (poker) card games together and getting some professional papers written.” In this collaborative environment, Klein might have inherited Samuelson’s idea of “becoming a technical expert” in which the model, and not the economist himself, gave the impression of being the one who “speaks” and makes recommendations. To Samuelson “technicality implied impartiality and detachment” (Maas 2014a, 273). In fact, a few years later, Klein (1950) showed that “it [was] possible to develop [the same macro-econometric model] from the un-Marxian principles of utility and profit maximization, but [...] also [...] from purely Marxian principles.” According to Klein, “the same model can be consistent with a multiplicity of hypotheses” (63-64). Furthermore, “emphasizing the operational significance of economic theory” provided “another way to defend [...] ideological neutrality” (Maas 2014a, 276). For instance, while “Samuelson presented his Keynesian message not as a policy creed but as a technical assessment” (286) Klein’s advocacy of Keynesian policy measures during the 1940s can also be considered a result of his technical work rather than of his political agenda. At the time, indeed, Klein had Marxist political inclinations as well as a quite critical position about the Keynesian approach. To Klein, however, econometrics provided “tools of analysis suited for economic policy that [were], as much as possible, independent of the personal judgments of a particular investigator.” Indeed, “econometric models [were] put forward in this scientific spirit, because these models should lead all investigators to the same conclusions, independent of their personal whims” (Klein 1947a, 111).

Apart from their affinity regarding “technical economics,” time and again, Samuelson reiterated that Klein had been one of his best (if not the best) students and that he had set the bar too high for the future generations of MIT students. “Often in public lectures [Samuelson] had to say that, if MIT pursued the maximand *average* quality of [its] Ph.D. graduates, [it must] have stopped with Lawrence Klein, [its] first graduate!” (Samuelson to LRK, May 22, 1985. PASP, box 45). Samuelson also insisted on how “we at MIT have always appreciated the key role [Klein] played in getting our graduate program off the ground. [Indeed, Klein had been] not only a first Ph.D., but also a first Nobel” (Samuelson to LRK, June 21, 2005. PASP, box 45).

During this period, Klein also published his first important papers, which reflected not only the technicality of Klein’s economics at the time, but also the sophistication in his analysis of broader questions of economic policy and political reform. In his first paper, Klein (1943) engaged into a controversy with Mordecai Ezekiel on the estimation of future investment, arguing that, in general, future investment was estimated “by means of a regression equation relating investment to income, a trend variable, and [...] a variable which introduces a lagged income effect” (246). According to Klein, however, this type of estimation could entail a serious problem of identification, since the “observed data on savings, investment, and income are [...] the coordinates of the intersection of [the statistical savings schedule and the statistical investment schedule]” (246). Rather than estimating these curves using “classical regression methods” like the method of least squares, Klein proposed a “much more elegant approach” (251) following Haavelmo’s (1941; 1943) probability approach to econometrics. In his second paper, Klein (1944b) provided an important contribution to the postwar question of “how much a full social insurance and assistance program [would] cost” (423) for the United States, showing, as Samuelson put it, that “his feet were on the ground and not in the clouds.” Inspired by the British

Beveridge Plan, Klein calculated what such a plan would cost if implemented in the US for the years 1945-1965. Even if he recognized that the discussion of other plans including the “Wagner-Murray-Dingell bill and the Security, Work and Relief Policies Report of the National Resources Planning Board may be thought to be more relevant at the [time],” he considered that the Beveridge scheme was “so polished and simple that it can well serve as a model for postwar planning in many countries” (423).

The other important work that Klein produced during his MIT years was his Ph.D. dissertation (Klein 1944a). Although Klein’s dissertation included several mathematical models of Keynes’s different works, the piece could be considered a contribution to the history and methodology of economics. Contrarily to what one could imagine beforehand, Klein approached Keynes’s theory and methodology from a critical (Marxian) point of view, stating for instance, that “Keynes did not really understand what he had written, and chose the wrong thing to publicize as his innovation” (Klein 1944a, 95), i.e. that wage rigidities and market imperfections provided the explanation for the existence of unemployment. Instead, Klein thought that Keynes’s innovation was the rejection of the classical theory of interest, and his contributions to the multiplier theory and the theory of the determination of effective demand (86). Another example not only of Klein’s critical tone towards Keynes, but also of his enthusiasm for Marx’s theories, social reform, and economic planning is to be found in chapter VII “Keynes and Social Reform,” added as the last chapter in the 1947 published version. In this chapter, Klein argued that “our program of social reform must continue even after we have solved the problem of unemployment.” Yet, even if “Keynesian economics gives us a set of tools with which to work on the unemployment problem, [...] it does not deal at all with many other important socio-economic questions that also deserve a

large share of our attention and study” (186).⁹

3. *Remaking Tinbergen at Cowles, 1944-47*

After these years in Cambridge, MA, Klein submitted his dissertation in October 1944, eager to obtain an academic job. Then like now, however, going to the job market was a hard experience, especially for mathematical economists and econometricians who still “operated in an academic underground [where] job opportunities were scarce [and postgraduate] scholarships were not abundant” (Klein 1991a, 112). Samuelson was concerned with the situation of his first Ph.D. graduate and made an important effort to contact several people and ask for the possibility of an available opening for this “very promising, able, young economist” which did not yield any positive results (Samuelson to Marschak, October 28, 1944, PASP box 45).

Fortunately for Klein, getting a job did not depend exclusively on his supervisor’s efforts. Klein submitted his “thesis paper” for the Econometric Society meeting in Cleveland on September 13-15, and presented it in a session chaired by Jacob Marschak who had been director of the Cowles Commission since January 1943, and who was assembling a team to embark on an ambitious research project. It was during that session that Marschak pronounced his famous

⁹ Backhouse (2017, chapter 24) suggests that the influence between Samuelson and Klein was mutual and that “their relationship was less unequal than Klein suggested” (522). Indeed, Klein helped Samuelson change his view on Keynes, passing from “seeing Keynes as having added the concept of the multiplier to a body of literature that adopted a more dynamic perspective on business cycle theory,” to seeing him as “creator of a new system” and “key figure in the New Economics.” This vision was more consistent with Klein’s idea of the *Keynesian Revolution* (*ibid.*, 522-5).

sentence to Klein that “what this country needs [...] is a new Tinbergen model, a fresher approach to it.” The Tinbergen model Marschak referred to was the macro-econometric model that Tinbergen had prepared for his report to the League of Nations in the late 1930s; the “fresher approach” was the use of the latest advances in econometric theory not available six years before at the time of Tinbergen’s publication: Haavelmo’s (1941; 1943; 1944) probability approach to econometrics.

About a month after the meeting, Marschak wrote to Samuelson telling him that he “was favorably impressed by Klein’s article in *Econometrica* and by the paper he read at the Cleveland meeting,” and that there was “a possibility of offering [him] a job [at Cowles] on conditions which may satisfy [him]” (Marschak to Samuelson, October 25, 1944, PASP box 45). Marschak wanted Samuelson to confirm or contest his impression of Klein “as one of the best men of his age available for econometric work.” Samuelson answered almost immediately appraising Klein as “a very promising, able, young economist with an excellent training” and “very well qualified to work on [Marschak’s project].” Marschak offered Klein a three-year position, with funding secured by the Commission for the first two years, and on the one condition that Klein applied for a Social Science Research Council (SSRC) fellowship for the last year. Klein accepted Marschak’s proposal “without any hesitation,” and “moved to Chicago [to] build an empirical system that could be used for extrapolation into the new peace time world” (Klein to Weintraub, 11 October 2010, LRKP, box 30). Klein joined the Commission on November 21, 1944, to begin one of the most influential periods of his academic life.

The positive impression he had exerted on Marschak in Cleveland, was soon corroborated by Klein’s work and personality. In fact, in a confidential statement written by Marschak to support Klein’s application for the SSRC fellowship (quoted at length in Bjerkholt 2014, 771-772),

Marschak confirmed that “[his] collaborators and [himself had] found in [Klein] 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 had also

a good eye for the essential [and] his goal [was to get] a logically consistent explanation of observed facts. He [would] 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).

4. Macro-econometric models as scientific tools for economic planning, 1947-1948

In 1947, after culminating three important years at Cowles, Klein obtained another SSRC fellowship to travel to Europe and visit Ragnar Frisch (who was delighted to have Klein in his institute) and other prominent European economists (Bjerkholt 2014, 778). Klein spent most of the time of his European sojourn in Oslo, where he closely observed the implementation of economic planning in the context of postwar reconstruction. Klein also spent some time at Tinbergen’s Central Planning Bureau in the Netherlands, making short trips to Denmark, Sweden, Switzerland, France, and England.

Klein had met Frisch in February of 1947 during Frisch’s visit to the University of Chicago when he gave a talk at the Cowles seminar on “Some basic formulae on demand analysis.” In Oslo, Klein did not only work with Frisch but continued close collaboration with Haavelmo who, after spending much of the war years in the United States, was finally back in Oslo since March 1947. Haavelmo had been promised a position as an economics professor at the University of Oslo under the condition that he worked during the first year at the Ministry of Finance preparing the National Budget for 1948. Klein was fascinated by the work Haavelmo was doing on Norwegian economic

planning: “The whole thing is interesting and is carried out in a more comprehensive peacetime scale than anything we have ever witnessed. [Haavelmo] is busy with drawing up the National Budget for 1948 [covering] planning in nearly every phase of economic activity [...]: manpower, production, consumption, investment, imports, exports, foreign exchange, fiscal policy, prices, rationing, etc.” (Klein to Samuelson, December 4, 1947, PASP box 45).

Although Klein wrote three lectures on the theory of planning, only one was published in 1948. In this publication, Klein (1948, 811-812) not only studied how economic planning was performed in the Norwegian economy but also defended the close relationship there should be between planning and econometric methods:

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 [Haavelmo’s] 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.

Planning was such an important topic for Klein that he continued to work on and think about it after his return from Europe, sticking “to [his] point [...] that planning is superior to competition because it can [affect] a modification of the constraints upon the system” which was not “open to the private enterprise economy.” Klein insisted on the relation between econometrics and economic planning, making “the purely formal point that one grand, planned production function gives more degrees of freedom in the maximization process than do the separate production

functions.” He went on explaining that “what [he gets] by planning (pooling of production constraints), is something that competition cannot be relied on to achieve,” and concluded that he still believed “that there are large gains to be made in a completely planned economy on logical and theoretical grounds” since “planners don’t have to use the same technological constraints that private entrepreneurs use” (Klein to Samuelson, 10 December 1948, PASP, box 45).

5. Building bridges of communication: Klein’s relation with the NBER

Klein returned to the United States at the end of 1948, after an invitation by Arthur F. Burns to join the NBER. This must have been a strange moment for a former Cowles researcher to be at the Bureau since the “measurement without theory controversy” was at its zenith. Yet, Klein was the only person who could rebuild bridges of communication between the different types of work conducted in these institutions. Indeed, Klein understood the importance of his colleagues’ work at Cowles, and especially of Haavelmo’s, which he described as “the inspiration for the research focus at the Cowles Commission” (Klein 1991a, 113). Also, he had always admired the “painstaking tradition of Simon Kuznets” (115), and the unusual attention that he and his team at the NBER paid to data. In other words, Klein’s empirical work based on the sophisticated econometric theory developed at Cowles during the 1940s was, in fact, a middle way alternative to the approaches confronted in the controversy between Cowles’s abstract approach and the NBER’s “empiricist” approach, as Tjalling Koopmans (1947) put it.

These methodological quarrels cast important concerns about the NBER’s leading place in empirical research, especially at the Rockefeller Foundation, the most important funding source for the NBER (Rutherford 2005, 121). Aware of these criticisms, the director of the Social Science Division of the Rockefeller Foundation, Joseph Willits, asked Burns to “give him his view on econometric models and Keynesian economics.” Burns answered that he had “recently set one

investigator to work on econometric models, not because any member of [his] group [had] much faith in them but because [they wished] to check [their] judgment and give this approach an opportunity to prove its merits” (Burns, quoted in Rutherford 2005, 122). This investigator was Klein, according to whom his “econometric interests were tolerated but not enthusiastically monitored” at the Bureau. Klein felt that he “was treated somewhat as a curiosity – an outsider who might eventually view the NBER approach in a more favorable light” (Klein to Rutherford, August 27, 2002, *ibid.*). Yet, Klein’s experience at the NBER left him with the impression that there was no “real conflict between the econometric work [he wanted] to do and the work of the business cycle staff of the Bureau.” In fact, to him, the “National Bureau technique,” which was largely “non-parametric” was a necessary first step to a final parametric study like that undertaken by most econometricians, and that there was “actually an econometric school of thought that [fell] in between the work of [Cowles] and the National Bureau,” represented in the works of Gerhard Tintner, Richard Stone, and Herman Wold (Klein to Burns, January 23, 1950, *ibid.*).

6. *Pursuing macro-econometric modeling beyond Cowles: the consolidation of a new scientific practice, 1949-1955*

Klein remained associated to the Bureau until 1951, but starting in 1949 he received a Carnegie fellowship and became a research associate of the Survey Research Center (SRC) of the University of Michigan, in Ann Arbor. In 1950, his affiliation to this university expanded to the economics department when he was appointed Lecturer. The type of empirical work in which Klein embarked at Michigan at the beginning was different from anything he had done before. Whereas Klein had been mainly confronted with the treatment of time series to build his macro-econometric models, in Michigan he faced survey data produced by an ambitious project on consumer behavior led by George Katona. Katona’s work exerted a long-lasting effect in Klein who, still in the 1970s and

1980s, defended the use of survey techniques to study not only consumer's but also investor's expectations as an alternative to the rational expectations program, which was gaining ground at the time (Klein and Mariano 1987, 442).

Around 1950, Koopmans and Klein toyed with the idea of reviving the macro-econometric project at Cowles, but despite two favorable reviews by Tinbergen and Haavelmo the project never took off (Bjerkholt 2014). In fact, Klein possibly thought that it was too much of a risk to embark in such an ambitious project in an institution that, like Cowles, was rapidly changing its research interests from econometrics to activity analysis, and chose to take a safer route in Michigan "where the attraction was the Survey Research Center" (Klein's interview of 25 January 1980, TUMA, box 5).

Indeed, the University of Michigan offered a great opportunity for Klein, consisting in the establishment of a "marriage of econometrics with Survey Research – one to give breath and the other depth" ("A Proposal for a *RSQE*," TUMA, box 5, 1). This marriage was represented by the creation of the Research Seminar in Quantitative Economics (RSQE) which, supported by a Ford Foundation grant, started functioning on October 1, 1951, with the "objectives of training faculty and students in quantitative methods of economic research producing substantive results on important empirical problems" (RSQE 1952, 1).¹⁰ The first project that the seminar undertook consisted in studying the "reconciliation of microeconomic and macroeconomic patterns of behavior." Its main objective was to assess whether or not the two sets of information, microdata

¹⁰ The RSQE research staff included Howard Raiffa, James Morgan, Daniel B. Suits as research and faculty associates, and Arthur Goldberger and Stephan V. Vail as research assistants (RSQE Annual Reports 1951-52 and 1953-54, TUMA, box 5).

obtained from survey methods and macro data obtained from market reports or social accounts led “to mutually consistent estimates or behavior patterns and the extent to which one set may reinforce the other” (2). An important characteristic of the seminar was its interdisciplinary nature, which involved “the wholehearted cooperation” between economists, sociologists, psychologists, and statisticians.

Similar to the Cowles’s seminar, the RSQE held weekly meetings “at which seminar members [discussed] research problems and techniques,” complemented with less frequent meetings where external speakers presented papers on relevant subjects for the projects of the seminar. The purpose of the RSQE was to reinforce the econometrics of an economics department that “was relatively thin” with “no formal course in econometrics,” other than Daniel Suits’s. At first, however, the department was not desperately eager to go too generous on this new field of econometrics. “The chairman [Leo Sharfman] was very cautious about introducing something very new, [confirming] that he had always wanted to get something started in a very modest way” and that Klein would give only one course in econometrics.

Yet, the big push for the department came just a bit later when the “Ford Foundation announced that it was giving a million dollars to [the University of Michigan]” (Klein, *ibid.*, 2). The money would come in soon, but the economics department did not seem to know what to do with it. Whereas the psychologists and sociologists “were more attuned to project work and knew how to spend [this amount of money],” the economists “were having precious few positive suggestions” (2), because they “were all [...] the lone scholar [working] in his study” (3). Sitting on the sidelines of the department as a newcomer, Klein suggested Kenneth Boulding that they designed a research project to spend that money and ended up putting forward the idea of the RSQE: “the idea of a research seminar was [...] Boulding’s,” that of “quantitative economics was

[Klein's]" (2). Klein became director of the seminar whose central project was "to build a macro model of the US." The seminar got "\$20,000, which [...] looked like a lot of money for a small research operation in the early '50's" (Klein's interview of January 25, 1980, TUMA, box 5, 2).

With the funding secured for the RSQE, Klein proposed a way to work that was new to the economics faculty, "except for [those] who were associated with the Survey Research Center." Morgan, John Lansing, Katona, and the professors in sociology and psychology "were attuned to that kind of [project] research," but the economics professors "were not project research oriented" (3). With both the RSQE and the SRC working together, the economics department wanted to "do something to get more involved in public affairs [and] to get more money into the place for a research program." A few faculty, including Klein, Suits, Katona, Richard Musgrave, and Gardner Ackley decided to organize a conference on the economic outlook and "started calling on friends in Detroit, who were economists in industry." They "got them down at Ann Arbor for a meeting on a Saturday afternoon" to organize the conference for the spring of 1953 (4):

We sent letters all over. And we put our own program with the econometric model being used, with the [SRC] Consumer attitudes forecast, and then a lot of forecasts from industry. And we got economists from companies all over [...] the nation to come [and] held the first of the outlook conferences in [...] October of 1953.

The economic outlook conference was the occasion at which the RSQE "unveiled the first use of [its] model," forecasting a small recession for 1953-1954 after the end of the Korean War. This work, an early version of the soon-to-be-famous Klein-Goldberger model (Klein and Goldberger 1955), was the basis for writing a press article that gave Klein and his team "enormous publicity." In fact, on November 16-17, 1953, Colin Clark (1953a; 1953b) published a two-parts-article in the *Manchester Guardian* with a very appealing title: "Danger signs of slump." In that article, Clark

claimed that the US was heading to a new recession like that of 1929, which had been expected by the end of World War II, but which had successfully been delayed by the new Korean war. Now that this war was over, the recession was ahead of the US economy. But Klein and Goldberger's projections prepared for the economic outlook conference yielded different results. A recession was indeed ahead of the US, but it would be small and modest. Klein and Goldberger (1954) mailed out their response to Clark's article, which was published on January 4, 1954. This publication gave the Michigan econometricians (and particularly Klein) a tremendous boost and world recognition, above all because Clark's results were not based on "dull financial journalism" as he himself put it (Clark 1953a, 4), but on a system of mathematical equations he had put together and published in *Econometrica* in 1949. Clark's (1949) system, which had proved capable of mimicking the observed US trade cycle, including the dramatic slump of 1929, had been improved with the inclusion of an equation dealing "with the effect of high construction costs upon the demand for houses and other buildings" to better suit the postwar era. The well-established Oxford Professor was optimistic about his system of seven equations and considered that it sufficed "to predict – and [...] control – the movement of the business cycle" (1953a, 4). "With the greatest respect for the distinguished work of Mr. Clark," the young Lecturer Klein and his student Goldberger "felt obliged to lay some of the forecasts for 1954 from their mathematical model," recognizing a minor decline in the US economy, but not at the rate of Clark's pessimistic projections.

Although the Clark episode certainly improved the legitimacy of the RSQE and the SRC, a macro-econometric model could not live from single-time accuracy results only. Given the growing activity and importance of the modeling project, it was also necessary to build up the calculation equipment at the university. Indeed, the millionaire Ford grant "included an equipment

budget that was administered by the chairman of the Psychology Department” (Brazer, TUMA, box 5, 115). Klein insisted that acquiring an electronic desk calculator for the RSQE would facilitate their job, and so an expenditure was approved for up to \$750 to buy the machine. Although the price of the Monroe electronic calculator was somehow lower (\$637.50), Sharfman still thought that Klein was asking for “a lot of money.” Indeed, \$640 represented a significant amount of money in the early 1950s, but compared to the Ford Foundation’s \$1 Million grant, the reticence to buy the calculator out of that money suggests only how much skepticism was raised by this new exercise of econometric modeling. In the end, the request was passed on under the condition that the RSQE shared the “new machine with the statistics classes, to which it would be carried for demonstration, and that it would also be available on request to other faculty members in the department” (115).

It was also in Michigan that Klein started to play around with the possibility of connecting econometrics and computers for the first time. Apart from the electronic desk calculator, and the IBM (electromechanical) tabulator in the basement of the Rackham School, a nascent computation center was available there as well: The Willow Run Research Center in Michigan, which counted with the Michigan Digital Automatic Computer (MIDAC). After the enthusiastic results of the economic outlook conference, Klein and his team tried to make the most out of this center and “started fiddling around with computers.” They “spent a lot of time working [...] on how to automate econometric models and use them in the computer mode”:

We did a lot of talking [and] thinking on that subject, [but] we never did get a successful implementation [...] [We] started doing some things by hand, some things by computer. We mixed the process, but we began to get oriented in the computerization of econometric models (Klein’s interview of January 25, 1980,

TUMA, box 5, 4).

Within this environment of favorable forecasts, growing computation facilities, funding reassurance, a fully-fledged working team, and a brilliant assistant, Klein continued his project of building a new macro-econometric model of the US economy enthusiastically and, with hindsight, recognized the pioneering character of the Michigan project:

The public policy process could not operate as it does today without a very important model input [...] Every respectable university throughout the world has classes [...] workshops [...], projects in econometrics, all the things we were doing then in terms of teaching, team research, project research, use of models with private sector and public policy [...] blossomed in the '60's and '70's [but] we were doing them in the early '50's. [...] The Michigan group was laying the ground work (Klein's interview of January 25, 1980, TUMA, box 5, 5).

Apart from the creation of both a team around the RSQE and the economic outlook conference, this pioneering work of macro-econometric modeling took symbolic significance after the publication of the Klein-Goldberger model. This model became later a landmark for large-scale macro-econometric modeling around the world, setting the bases for the teaching of macro-econometric modeling and the "growing econometric forecasting industry." Yet, during the 1950s, the model was not used as a teaching device but was considered a cutting-edge research object in constant evolution and a forecasting tool. The research seminar was "based around this project team research effort" and was understood as a continuous program in that "new data, reformulations, and extrapolations [were] constantly being studied" (Klein and Goldberger 1955, 1). In short, the distinctive feature of the project was "that the task [was] not [...] seen as a once-

and-for-all job” (1).

With 15 structural equations, five identities, and five tax-transfer auxiliary relationships, the resulting model was a “medium-size” model “truly intended [...] to be an up-to-date working model, applicable to economic problems like those encountered in the business cycle forecasting” (Bodkin et al. 1991, 57). As Goldberger put it, “the model [was] constructed as a working, aggregative system with an eye towards its use in the future as a trunk on which to graft disaggregated sector equations, special industry models, and up-to-date observations” (Goldberger 1952, RSQE TUMA, box 5, 11). Goldberger remembered that Klein’s “work at Michigan was a continuation of the work he [had] done at Cowles” and that “what [is] called the Klein-Goldberger model is really Klein Model IV” (Goldberger et al. 1989, 135). With hindsight, Goldberger saw himself as “a clerk on the model,” and considered that Klein had been “generous enough to put his name on it” (135). This characterization of Klein as a generous person might well fit his personality. Yet, without denying this trait, Klein’s generosity in this case also reflects his own image of macro-econometric modeling as a teamwork endeavor that must be carried out with a clear division of labor and under a specific institutional configuration.

After four years of hard teamwork, revisions, and data updates, Klein and Goldberger published their model with a five-page-long caveat as a preface, where they explained that the model was now outdated “by the basic revisions of the national income accounts made by the US Department of Commerce in mid-summer 1954.” This, however, did not invalidate the whole approach, but “simply made the problem of parameter re-estimation more urgent” (Klein and Goldberger 1955, vii), bringing to light some of the most important features of the practice of macro-econometric modeling: that it is an ongoing activity, based on teamwork effort, where everything is not about the model, but where other factors come into play such as the quality of

data, the policy demand, or the institutional configuration in which the model is built. Klein's important effort at the University of Michigan was ended in an indecorous way by the McCarthy era, however. After suffering an internal investigation for his "un-American activities" during the 1940s and after being denied tenure, Klein decided to move to Oxford, where academic freedom was granted, and start his macro-econometric modeling project anew (see Pinzón-Fuchs 2017, chapter 2).

Concluding remarks

The period between 1938 and 1955 marked an important milestone in the creation and development of macro-econometric modeling as a new way to produce macroeconomic knowledge, and in the formation of Klein's identity as a macro-econometrician through his encounters with important institutions and personae. Macro-econometric models, however, did not emerge as a way to "apply" or "validate" economic theories in the light of data. Rather, these models were the essential tools of a more complex system of reasoning, which incorporated a team of experts, policy issues, theory (economic and statistical), data, and routinized procedures, but also creativity and improvisation within the new scientific practice of macro-econometric modeling. This new practice marked a transformation in the production of macroeconomic knowledge in which theory, application, data, expertise, and policy issues could not be neatly separated. Instead, all these elements were embodied within the new system of reasoning, yielding a powerful and rigorous way to understand and intervene the economy. Klein's formative years formed the bases for the emergence of large-scale macro-econometric modeling and paved the way for it to develop as an autonomous scientific practice that no longer depended exclusively on Klein's enthusiasm, but which now made part of the community of macroeconomists and of several institutions and teams of experts. In short, by the end of the 1950s, macro-econometric

modeling existed as a novel, promising, and rather autonomous object, ready to be adapted and further developed by the macroeconomists' community.

References

Backhouse, Roger (1998) "The Transformation of U.S. Economics, 1920-1960, Viewed through a Survey of Journal Articles." *History of Political Economy*, 30(suppl.): 85-107.

_____ (2014) "Paul A. Samuelson's move to the MIT." *History of Political Economy*, 46(suppl.): 60-77.

_____ (2015) "Revisiting Samuelson's Foundations of Economic Analysis." *Journal of Economic Literature*, 53(2): 326-50.

_____ (2017) *Founder of Modern Economics: Paul A. Samuelson*. Oxford University Press.

Bjerkholt, Olav (2014) "Lawrence R. Klein, 1920-2013: Notes on the early years." *Journal of Policy Modeling*, 36: 767-784.

Bodkin, Ronald G., Lawrence R. Klein and Kanta Marwah (1991). *A History of Macroeconometric Model-Building*. Hants: Edward Elgar.

Cherrier, Béatrice (2014) "Toward a history of economics at MIT." *History of Political Economy*, 46(suppl.): 15-44.

Clark, Colin (1949) "A Systems of Equations Explaining the United States Trade Cycle, 1921 to 1941." *Econometrica*, 17(2): 93-124.

_____ (1953a) "Danger signs of slump: I- American Trends." *The Manchester Guardian*, November 16, 1953: 4.

_____ (1953b) "Danger signs of slump: II- The Lessons of 1929." *The Manchester Guardian*,

November 17, 1953: 4.

Duarte, Pedro (2014) "The Early Years of the MIT Ph.D. Program in Industrial Economics." *History of Political Economy*, 46(suppl.): 81-108.

Galbraith, John K. (1997) "Berkeley in the Thirties." In Danielle Lafrance (ed.) *Berkeley! A literary tribute*. Berkeley: Heyday Books: 44- 55.

Haavelmo, Trygve (1941) *On the Theory and Measurement of Economic Relations*. Cambridge Massachusetts.

_____ (1943) "The Statistical Implications of a System of Simultaneous Equations." *Econometrica*, 11(1): 1-12.

_____ (1944) "The probability approach in econometrics." *Econometrica*, 12(suppl.): i-vi+1-118.

_____ (1958) "The role of the econometrician in the advancement of economic theory." *Econometrica*, 26(3): 251.

Kendall, D. G. (1984) "Neyman's early life and subsequent career." *Bulletin of the London Mathematical Society*, 16(2): 160-168.

Klein, Lawrence R. (1943) "Pitfalls in the statistical determination of the investment schedule." *Econometrica*, 11(3): 246-258.

_____ (1944a) *The Keynesian Revolution*. Ph.D. Thesis, Massachusetts Institute of Technology.

_____ (1944b) "The cost of a 'Beveridge Plan' in the United States." *The Quarterly Journal of Economics*, 50: 423-437.

- _____ (1947a) “Theories of Effective Demand and Employment.” *Journal of Political Economy*, 55(2): 108-131.
- _____ (1947b) *The Keynesian Revolution*. Macmillan.
- _____ (1948) “Planned economy in Norway.” *The American Economic Review*, 38 (4): 795-814.
- _____ (1950) *Economic Fluctuations in the United States, 1921-1941*. Wiley.
- _____ (1960). “Single Equation System Methods of Estimation in Econometrics.” *Econometrica*, 28(4): 866-871.
- _____ (1980) “Lawrence R. Klein – Biographical.” *Nobelprize.org*. Nobel Media AB. Web. 31 Aug 2016. http://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1980/klein-bio.html.
- _____ (1991a) “Econometric contributions of the Cowles Commission, 1944-47: A Retrospective View,” *Banca Nazionale del Lavoro Quarterly Review*, 44(77): 107-117.
- _____ (1991b) “The Statistics Seminar, MIT, 1942-1943,” *Statistical Science*, 6(4): 320-330.
- Klein, Lawrence R. and Arthur Goldberger (1954) “‘A mild down-turn’: the American trade recession.” *The Manchester Guardian*, January 4, 1954: 4.
- _____ (1955) *An Econometric Model of the United States, 1929-1952*. North-Holland Publishers.
- Klein, Lawrence R. and Roberto S. Mariano (1987) “The ET interview with Professor L. R. Klein.” *Econometric Theory*, 3(3): 409-460.
- Koopmans, Tjalling (1947) “Measurement Without Theory.” *The Review of Economic Statistics*,

29(3):161-172.

Morgan, Mary S. and Malcolm Rutherford (eds.) (1998), *From Interwar Pluralism to Postwar Neoclassicism*, Annual supplement to volume 30 of *History of Political Economy*. Duke University Press.

Neyman, Jerzy and Egon Pearson (1933) "On the Problem of the Most Efficient Tests of Statistical Hypotheses." *Philosophical Transactions of the Royal Society of London*, 231: 289-337.

Pinzón-Fuchs, Erich (2017) *Economics as a 'tooled' discipline: Lawrence R. Klein and the making of macroeconometric modeling, 1939-1959*. Ph.D. Thesis, Paris 1 University Panthéon-Sorbonne.

Reid, Constance (1998) *Neyman*. Springer Verlag.

Rutherford, Malcolm (2005) "'Who's afraid of Arthur Burns?' The NBER and the Foundations." *Journal of the History of Economic Thought*, 27(2): 109-139.

_____ (2011) *The Institutional Movement in American Economics, 1918-1947: Science and Social Control*. Cambridge University Press.

Weintraub, E. Roy (2002) *How Economics became a Mathematical Science*. Duke University Press.

_____ (2014) "MIT's openness to Jewish economists." *History of Political Economy*, 46(suppl.): 45-59.

Manuscript Sources

(LRKP) Lawrence R. Klein Papers, David M. Rubenstein Rare Book and Manuscript Library, Duke University.

(PASP) Paul A. Samuelson Papers, David M. Rubenstein Rare Book and Manuscript Library,
Duke University.

(TUMA) The University of Michigan archives. Bentley Historical Library. Michigan Historical
Collections.