Chapter 2

The making of a macroeconometrician: from the "Dwellers-on-the-bluff" to the Ivy League

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> The true theorist in economics has to become at the same time a statistician.
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> *Ragnar Frisch, 1930*

> I used to say that after Klein's Ph.D., it was diminishing returns all the way […] Best of all Laurie, all that you accomplished you did your own way. Imagine being at all such places as Evans's Berkeley, MIT, Chicago's Cowles, Ann Arbor in the Musgrave, Boulding era, Oxford, Oslo, Rotterdam, and Penn. Time could not stale Cleopatra's charms or your sagacities.
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> *Paul A. Samuelson, letter to Lawrence R. Klein, 31 January 2006*

1. Introduction[2]

Lawrence R. Klein was born in the mid-western city of Omaha, Nebraska, on September 14, 1920.[3] Survived by his wife Sonia, his three daughters Hannah, Rebecca and Rachel, and his son Jonathan, Klein died on October 20, 2013 in Gladwyne, Pennsylvania, at 93. At the summit of his career and just a month after he had turned 60, Klein was awarded the 1980 Nobel Prize in Economics "for the creation of econometric models and their application to the analysis of

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[2] This is a first (and incomplete) draft of the second chapter of my dissertation, which I will defend in March of 2017. Please find an outline of the dissertation with a summary of the chapters in the "Annex 2."

[3] The word Omaha means "dwellers-on-the-bluff" (Mathews 1961, 91).

economic fluctuations and economic policies" (Nobelprize.org 2014).[4] Besides acknowledging his important contributions, the Nobel Prize also laid bare Klein's fascinating academic life, throughout which he always seemed to be in the right place, at the right moment, and with the right people.

To begin with, he completed his BA in economics in 1942 at Jerzy Neyman's and Griffith Evans's University of California, Berkeley, and his Ph.D. on *The Keynesian Revolution* (Klein 1944) under Paul A. Samuelson's supervision at MIT in 1944. He then joined Jacob Marschak's Cowles Commission, before he went to Europe to work with Ragnar Frisch and Trygve Haavelmo, where he also met Richard Stone and Jan Tinbergen (Klein, 1980a). In 1948, and after his one-year stay in Norway, England and the Netherlands, Klein accepted an invitation by Arthur Burns and returned to the United States to work at the National Bureau of Economic Research (NBER), just at the zenith of the "Measurement without theory" controversy between the Bureau and Cowles. Attracted by the Survey Research Center, Klein moved to George Katona's and Richard Musgrave's University of Michigan in Ann Arbor in "November or December of '49" (Klein 1980b), where he built, together with Arthur Goldberger, his famous Klein-Goldberger model (Klein and Goldberger 1955). Harassed by the anti-democratic pressures of the post-WWII period, Klein suffered from the effects of McCarthyism, and was forced to leave the country "for the peace and academic freedom of Oxford" University in 1954. After constructing a macroeconometric model of the United Kingdom (Klein et al. 1961), and once he had set his mind to stay in Oxford, Klein eventually returned to the US, accepting his new appointment as professor at the University of Pennsylvania in 1958 where he stayed until his retirement. This early stage of his career, in which he opened up his space as a macroeconometrician among some of the most prominent

[4] Klein was awarded the *Sveriges Riksbank Prize in Economic Sciences in Memory of Alfred Nobel*.

members of the economics community, culminated with his awarding of the John Bates Clark Medal in 1959, just before he turned 40.

Since the very first stages of his undergraduate studies starting in 1938, Klein felt an "early fascination with higher mathematics" which "blossomed into speculative thinking that could provide a basis for dealing with economic issues" (Klein 1980). First, at Los Angeles City College, and then at Berkeley the "teachings of the mathematics faculty […] provided [him] with great stimulus." In addition, "the onset of World War II, with all the associated disturbances leading up to it, made a tremendous impression on [his] thoughts about socio-politico-economic interrelationships" (Klein 1980), marking his later convictions about intervention, economic planning, and social reform.

Klein begun his studies in economics and mathematics at a time of important changes in both the US-American university landscape, and the economics discipline (Morgan and Rutherford 1998). Although still 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.[5] Yet, this transformation in the education and training of US-economists was neither abrupt nor homogeneous. Quite on the contrary, each academic establishment underwent a particular process of transformation characterized by its own specificities and personae, which provide complex stories at the level of each institution and individual.

This ongoing transformation did not occur only at the educational level, however. The standards of what was considered to be scientific and objective in economics was also changing.

[5] For an account of the transformation occurred at the MIT department of economics, for example, see Weintraub (2014a). Emmet (1998) provides an account of the transformation at the University of Chicago, "as part of a transformation that narrowed the boundaries of 'legitimate' economics in the attempt to entrench it in its area of scientific competence" (135).

The new boundaries expanded towards a more technical or mechanical type of objectivity represented in the construction of routinized and standardized practices that attempted at rendering scientific results and analyses impersonal, neutral, and value-free.[6] Yet, this transformation was not immediate either, and 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 can be representative of the kind of scientific and professional education any student in economics of this generation might have received.[7] This particular socialization taught him how economic science was done, which was the role, self-image, values, norms and behaviors of an economist during the 1940s and 1950s. Yet, Klein's experience is also the story of a personal academic life, and so it must be understood in its uniqueness too. 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 macroeconometrician.[8]

By the time Klein embarked on his economics education in 1938, both econometrics and macroeconomics had hardly been established as fully-fledged disciplines. While the Econometric Society had been founded in 1930 and *Econometrica* in 1933, the term "macroeconomics" became of common use only during the 1950s.[9] Compared to econometricians and macroeconomists,

[6] For a history of the notion of "mechanical objectivity" in the natural sciences see Daston and Galison (2007), and Porter (1995).
[7] For an account on the importance of pedagogy in the making of scientists see Kaiser (2005).
[8] For an account on the formation of an identity as scientist and of questions of scientific credit in the lives of Kenneth Arrow, Lionel McKenzie and Gérard Debreu see Düppe and Weintraub (2014).
[9] A quick search on Ngram viewer shows how the terms "econometric" and "macroeconomic" appear in English book titles only in the late 1940s (see graphs 1 and 2 in annex 1). This does not mean, of course, that there were no econometric or macroeconomic works before and during this period. Yet, the self-identity

macroeconometricians were even more scarce. Only Jan Tinbergen in the distant and occupied Netherlands, provided a clear example of what it meant to be a macroeconometric modeler. In a nutshell, during these early years Klein not only forged his own identity as a macroeocnometrician, but also crafted a new scientific practice of macroeconometric modeling. In this chapter I want to understand how specific personae, events and institutions marked Klein's own identity as a macroeconoemtrician, and how this identity contributed to the construction of the new scientific practice of macroeconometric modeling.

2. The Neyman-Kuznets-Evans connection at Berkeley: statistical testing, empirical work, and mathematical rigor, 1940-1942

Klein arrived at the University of California, Berkeley, in 1940 after completing two years at Los Angeles City College. Although Berkeley "was not a leading center in mathematical economics" at that moment, it provided "a rather good environment […] to someone interested in mathematical economics and econometrics" (Klein and Mariano 1987, 410). This good environment to learn mathematical economics was stimulated mainly by the presence of, at least, four important figures that marked Klein's vision on the use of mathematics and statistics at a very early stage: Jerzy Neyman, George M. Kuznets (Simon Kuznets's younger brother), Griffith C. Evans, and Francis Dresch.

2.1. Mathematical statistics at Berkeley

At the time of Klein's arrival in Berkeley, UC Berkeley was not recognized as a strong institution in statistics. Yet, only two years before, in 1938, Berkeley had hired the already internationally renowned statistician Jerzy Neyman.[10] After four years as a reader in statistics at the University

of "macroeconomists" or "econometricians" was still weak and the community was still at an embryonic stage.

[10] Jerzy Neyman (1894-1981) was born in Bendery, today's Moldavia from a Polish family. He studied mathematics at the University of Kharkov, Ukraine, between 1912-1916. "During the turbulent years that

College London, Neyman had rejected an offer from "another prestigious [US] institution where [statistics] was richly developed" and had chosen to go to Berkeley where he wanted to build a statistics program "in his own lines" (Kendall *et al.* 1984, 163), which meant to build a program with a strong focus on mathematical statistics.[11] Neyman did not have major problems in building his own program and, in 1945, after only a few years in California, he had founded the Statistical Laboratory, and the Berkeley Symposia, which consolidated Berkeley's position as a top university for the study of mathematical statistics (Stigler 1999).

Although Klein was not a student of Neyman's, he "worked with a lot of Neyman's disciples at that time" (Klein and Mariano 1987, 410), getting acquainted with mathematical statistics and hypotheses testing *à la* Neyman-Pearson from a very early stage in his career. The impression that 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, "On the problem of the most efficient tests of statistical hypotheses," which had marked a milestone for statistical testing procedures.

One of Neyman's disciples Klein worked with was George M. Kuznets, Simon Kuznets's younger brother.[12] In 1941, Kuznets completed his Ph.D. in psychometrics at Stanford University

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followed [Neyman] was arrested as an enemy alien, and later forcibly sent to Poland in an exchange of prisoners. Thus the random fortunes of war forced [Neyman] to leave the land of his birth and take up residence in the land of his fathers, and these same events also caused him to abandon a career in pure mathematics and turn to statistics" (Kendall *et al.* 1984, 163). In 1921, he found a job at the Institute of Agricultural Research at Bydgszcz, moving later "to Warsaw as Head of the Biometric Laboratory in the Nencki Institute." In 1934, he was appointed lecturer and reader in Statistics at University College London. Four years later he left for the University of California, Berkeley.

[11] Economics was not the only science that experienced a strong debate and transformation because of its increasing mathematization. Statistics, too, was itself immersed in this debate. See Yule, Pearson, Kendall, quoted in Vining (1949).

[12] George M. Kuznets was born in Kiev in 1909, and died in 1986. He moved to the United States in the 1920s, earning his BA in psychology in 1933, and his Ph.D. in 1941. It is not clear whether he obtained his Ph.D. from Stanford University (Grilliches, 2000, 1969) or whether he obtained it from the University of California, Berkeley (American Journal of Agricultural Economics 1982). From 1937 to 1939 he taught psychology and education at Stanford University and in 1941 he joined the department of Agricultural

(Griliches 2000, 169), and joined the Department of Agricultural Economics at Berkeley, where he embarked in empirical research to estimate demand functions for lemons in California. Klein spent one summer (in 1942) working at the Gianini Foundation as an assistant to George M. Kuznets, who "was a very good statistician, though his degree was in psychology" (Klein and Mariano 1987, 411). This collaboration between Kuznets and Klein ended up in Klein's first publication: *A statistical analysis of the domestic demand for lemons: 1921-1941*.[13] Despite the differences between the work that Klein performed during that summer, consisting on an applied microeconomic exercise, and his future macroeconometric work, this experience of getting into the field to analyze real data marked Klein's enthusiasm for statistical work in economics, which he never abandoned.[14]

2.2. Mathematical economics and rigor

The other important personality at Berkeley was Griffith C. Evans.[15] Just as in Neyman's case, Klein "didn't study with him," but he "did a lot of work with people that were his students" (Klein and Mariano 1987, 410). With hindsight, "it is not unreasonable […] to see […] Klein as linked to Griffith C. Evans" (Wintraub 2002, 71) and to argue that Evans's image of mathematics passed

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Economics at UC Berkeley, where he became a professor in 1952, only to retire in 1977. His main contributions were in the fields of agricultural economics and applied microeconometrics. For a brief account on Kuznets see (American Journal of Agricultural Economics 1982).

[13] This publication constituted a report for the Giannini Foundation of Agricultural Economics, UC Berkeley, by the California Agricultural Experiment Station.

[14] Zvi Griliches (see Griliches 2000) and Arnold Zellner (see Rossi and Zellner 1989) also remember G. M. Kuznets as an important figure in their careers. Yet, besides a few mentions, there is not much information about the influence that Kuznets actually exerted on these econometricians in terms of their econometric practice. There is no account either that relates or compares the quantitative work of the two Kuznets brothers, which seems to be quite different at a first glance. While George was very enthusiastic about the development in econometrics at Cowles, Simon worked at the NBER.

[15] Griffith Conrad Evans was born in Boston on 11 May 1887, and died on 8 December 1973. He studied mathematics at Harvard where he completed his BA (1907), MA (1908), and Ph.D. (1910). After completing his Ph.D., he spent two years in Rome working with the mathematician Vito Volterra, returning to the United States in 1912 to join the faculty of the Rice Institute (now Rice University) in Houston, Texas, where he stayed until 1934. Since 1930, Evans played an important role in the creation of the Econometric Society. During his last twenty years of his active academic career (1934-1954) he became chair of the Department of Mathematics at the University of California, Berkeley. For a detailed account of Evan's contributions and life see Weintraub (1998a; 2002, chapter 2), Duarte (2016), and Simon (2008).

on to Klein, influencing his practices as a macroeconometrician.[16] To understand the kind of image of mathematics that Evans transmitted to Klein it is important to locate his position within the landscape of mathematics.

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, among others. 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 would be "directly apprehended through experimentation and observation" being "thus interpersonally confirmable" (48).

These visions are definitively consistent with Klein's own idea of the use of mathematical tools and statistics.[17] While 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 1991, 113-114), he sustained that "the adoption of more powerful methods of mathematical statistics [was] no panacea" (Klein 1960, 867). To Klein, "if econometric results are today more useful than in the past, this is only partly a result of the particular method of estimation but much more significantly a product of painstaking research of a more pedestrian nature." In addition,

[16] Here the notion of "image" makes reference to Leo Corry's (1989; 2004) framework of image and body of knowledge as used in Weintraub (2002). The image of knowledge of a discipline is formed by a set of "second-order questions" concerning the methodology, philosophy, history, or sociology of any particular discipline.

[17] See chapter 4 for an account on Klein's image of econometrics.

Klein expected "marginal improvements of [only] five or ten per cent through the use of more powerful methods of statistical inference" (867).

This vision on the use of mathematics was strongly related with an idea about 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 would not be provided by abstract ideas, or axiomatization, but by the constraints imposed by the real phenomena themselves, which should mold the mathematical models. Weintraub (2002, 71) refers to this kind of rigor as "materialist-reductionist quantification."

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). Also Haavelmo praised a kind of rigor consistent with this vision. To Haavelmo (1958, 352), one of the leading figures of Chicago's Cowles Commission and a close friend and collaborator of Klein's, 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."

To these rigorous mathematical economists, however, the use of mathematics in economics had to be carried on with caution. To be clear, rigor, in Evans's sense, must guide the use of mathematics in economics, and so the use of mathematics had to be based on the observed and studied reality. But the researcher had to go beyond the mathematics, for once economic theories are expressed in those terms, they might become rigid structures that would not let any new classes of phenomena enter into the minds of the researcher, diminishing her imaginative and creative

capacities. Evans (1930, 110) was conscious of this problem, warning economists of the dangers of the use of mathematics to form economic theory:

> General principles are apparent in the particular phenomena which we have studied, or at least, there are some general methods, which we can make use of in unifying those separate treatments. Nevertheless we must adopt a cautious attitude towards comprehensive theories. They do of course, in their special applications, 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.

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 the "Harvard" bits, Klein was very much like that too.[18] 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 sophisticated models and methods, reason had always a human face, it was always an expert (whether an individual or an institution) who should direct the construction of the large-scale macroeconometric models based on knowledge that goes beyond mathematics and statistics; and it was also the expert and her team who should discuss and interpret the models' results both to adjust them through reasoned tinkering, and to make context-dependent policy recommendations. This knowledge that Klein and Haavelmo called "a priori knowledge," contained as well a great deal of economic theory, and a broader understanding about the institutional and historical arrangements of the economy.

[18] Although Klein did not go to Harvard, he maintained a close relation to that institution during his period at MIT as I comment later, in particular with important figures such as Edwin B. Wilson and Alvin Hansen.

Imagination and creativity also played an important role in this heuristic practice of building macroeconometric models.[19] [Francis Dresch…]

3. Becoming technical at MIT: Samuelson, the Statistics Seminar, and Wilson, 1942-1944

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 the Massachusetts Institute of Technology (MIT), capturing the attention of the young professor Paul A. Samuelson:

> In my correspondence with faculty and staff in Cambridge, I found that my 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. He knew, by reputation or personal contact, about their interests in and direct contribution to new trends in economics, taking the subject into more intensive use of mathematics and statistics. He was interested in the work of Berkeley professors Francis Dresch (mathematical economics and statistics) and Jerzy Neyman (mathematical statistics) (Klein 2011, 502-503).

It was Norman S. Buchanan, R. Aaron Gordon and William J. Fellner who provided letters of recommendation in support of Klein's application to MIT.[20] According to Samuelson, they all agreed that Klein "was one of the best undergraduates they had ever had." (Samuelson, letter to Alice Bourneuf, Board of Governors of the Federal Reserve System o September 12 1944, PASP box 45). Klein's passing through MIT was paramount in his sharpening of mathematical and statistical techniques, but also in his consolidation as one of the rising economists of the early 1940s.

[19] Chapters 3 and 4 discuss both the limitations of the macroeconometric models under the light of the Keynes-Tinbergen controversy, and Klein's and Haavelmo's use of a priori knowledge to overcome the limitations imposed by the use of mathematical and statistical methods in economics.

[20] Klein had assisted Fellner in his "Treatise on Wartime Inflation: present policies and future tendencies in the United States" published by the University of California in 1942.

3.1. Draft status 4-F: Klein's scholarship at the new MIT program

In the late Spring of 1942, while "straightening a desk […] in the main room of the economics facilities" at Berkeley, Klein "noticed a group of new announcements of economics faculty attractions at the Massachusetts Institute of Technology" (Klein 2011,502), and felt immediately attracted to the new program. At that time, Klein had been attending government recruitment meetings to resolve his military situation, and had been almost resigned about accepting "a secondary job in the U.S. government," which would have probably consisted on a "desk job" at the military. Due to a childhood accident, Klein's military draft status was 4-F, "disabled," and so the new program at MIT opened up a fantastic opportunity for him after "the government recruiters kept stressing that [he] could be an expensive disability to the government" (502).

Klein arrived in Cambridge, MA, in September 1942, one year after MIT had started its Ph.D. in economics that would become a top program only in the 1960s (see Weintraub 2014a). In fact, the department of social sciences had been focused on providing teaching for engineers until 1940. Klein made part of the second entering class of the new graduate program of economics at MIT, and became the first economics Ph.D. recipient on October 9, 1944 (Klein 1991b; Duarte 2014).[21] The whole Institute had experienced important changes of restructuration (Backhouse 2014a), passing from an "undergraduate engineering school to [a] full-fledged research university" (75) during the 1930s. The economics department, however, lagged some years behind and only embarked in this transformation in 1940. The Institute's inauguration of a new graduate program in economics (Duarte 2014), its openness to Jews (Weintraub 2014b) at a time when anti-Semitism "was woven into the fabric of academic institutions" (Backhouse 2014, 73) and when Harvard was

[21] Duarte (2014) provides an account of the early history of the MIT graduate program in economics.

clearly anti-Semite (Backhouse 2015, 74; Weintraub 2014b), and the development of a more technical way of doing economics (Cherrier 2014), marked the rise of MIT economics.

W. Ruppert McLaurin was instrumental in the transformation of the department and in helping Harold Freeman hiring Samuelson (see Backhouse and Maas 2016).[22] Only 25 years old, and with a Ph.D. earned at Harvard in the field of "mathematical economics," Samuelson was a very good fit for the Institute given that every student there was required to study mathematics and physics. Samuelson proved his value contributing to the rise of MIT as one of the most important economics departments in the country. Yet in the early 1940s, Samuelson was reluctant to accept MIT's offer because he wanted to stay in the, by the time, stronger department of economics at Harvard (Backhouse 2014; 2015).[23]

This hiring process of promising economists was not consolidated until the end of the 1940s. First, Samuelson's own permanence at MIT was seriously threatened by tempting proposals from other universities at least until 1949. The most important of these proposals was led by Theodore Schultz in Chicago (see Maas 2014), Samuelson's birth area and undergraduate school. Also, other important and representative figures of MIT's transformation were not hired until the second half of the 1940s. These enrolments included Cary Brown's (1947), Robert Bishop's (1949), Charles Kindleberger's (1948), Morris Adelman's (1948), and Robert Solow's (1949) (Cherrier 2014, 20). In a nutshell, the economics department at MIT that Klein integrated was still in a very embryonic stage. Yet, at a time where mathematical economics was still struggling to become dominant and

[22] Although Samuelson appears very often as the most visible figure in this story, Backhouse and Maas (2016, 424) have recently argued that the initial driving force "behind the transformation of the department, which grew in size and expanded the range of its activities" was McLaurin. The importance of this finding resides in the fact that MIT economics might have been developed in a less technical and more interdisciplinary way, if McLaurin's project on innovation and technical change funded by the NBER would not have failed. M

[23] For a comprehensive account of Samuelson's move to MIT see (Backhouse 2014).

to be considered more than a narrow specialty in economics (Backhouse 1998), MIT seemed to be the perfect place for somebody like Klein to complete his Ph.D., above all because of Samuelson's presence.

By 1942, even if Samuelson and Freeman were responsible for the branch of mathematical statistics, the economics department was not able to teach advanced topics in mathematical statistics, but offered only general courses in this topic through the mathematics department (Klein 1991b, 320). Together with his MIT classmates, and especially with 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 into being between 1942 and 1943 (Klein 1991b; Bjerkholt 2013, 768-769).[24] It was at the occasion of this seminar that Klein met Haavelmo for the first time, in 1943. This was the beginning of a fruitful and friendly relationship, further cultivated between 1946 and 1948, when they overlapped in Chicago as research assistants for the Cowles Commission, and during the year Klein spent in Oslo, Norway, after his period at Cowles.[25]

3.2. From "Prof. Samuelson" and "assistant Klein," to "Paul" and "Laurie"

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. To

[24] A complete list of the speakers with abstracts of their presentations is available in Klein (1991b). The speakers included: Kenneth J. Arnold on "Spherical Probability," Albert H. Bowker on "Enumeration of Latin Squares," William Feller on "Stochastic Processes," Harold A. Freeman on "Bayes' Theorem and Testing Hypotheses," Trygve Haavelmo on "Some Problems of Statistical Inference Arising in Econometrics," Klein himself on "Neyman's Smooth Test for Goodness of Fit," Paul A. Samuelson on "Gram-Charlier Series," Dirk J. Struik on "The Foundations of the Theory of Probabilities," Richard von Mises (Ludwig's brother) on "The Probability of Occupancy," Abraham Wald on "A Problem of Multivariate Analysis," Norbert Wiener on "Ergodic Theory," and Edwin B. Wilson on "Contingency Tables."

[25] Klein joined Cowles on November 1944 and stayed there until September 1947. Haavelmo joined Cowles in early 1946 and returned to Oslo in the Fall of 1947.

Klein (1980), in any case, "working as an assistant for Samuelson was something that is 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 I [Klein] felt very fortunate to be in that background" (411).

When Klein arrived at MIT, Samuelson was very busy working on important consultancy projects, including one for the National Resources Planning Board (NRPB) (Maas 2014, 279-282). According to Samuelson, "to a surprising degree, [Klein had] been able to go ahead on his own steam in these disorganizing war years" (Samuelson in a letter to Marschak, dated October 28, 1944, PASP box 45). Yet, despite Samuelson's multiple engagements, Klein (2011) 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" (505). The difference of only 5 years might have facilitated this friendly relationship between Samuelson and Klein, despite Samuelson's Chicagoan methodology (perhaps adopted from Jacob Viner) of "suddenly [directing] attention to a relevant question, asking for more complete information from a single student, who might be well prepared in Samuelson's approach, and who should have been prepared to elaborate on the implied questions in his statement." This approach "made many students uncomfortable and often frightened that they would be singled out as unprepared," driving "students to careful and detailed [and collective] study before class" (504).

Klein might have inherited Samuelson's idea of "becoming a technical expert" in which the model, and not the economist himself, would provide the impression of being the one that who "speaks" and makes recommendations.[26] In his *Economic Fluctuations in the United States economy, 1921-*

[26] In particular, Klein (1947a; 1950) are examples of this, as I explain in chapter 4.

1941, for instance, Klein (1950) showed that "it [was] possible to develop [the same macroeconometric model] from the un-Marxian principles of utility and profit maximization, but […] also […] from purely Marxian principles." According to Klein, then, "the same model can be consistent with a multiplicity of hypotheses" (63-64). As noted by Maas (2014) to Samuelson, "technicality implied impartiality and detachment" (273). Furthermore, "emphasizing the operational significance of economic theory" provided "another way to defend […] ideological neutrality" (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 theory and policy during the 1940s can also be seen as 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. Econometrics, however, provided tools of analysis suited for economic policy that are, as much as possible, independent of the personal judgements of a particular investigator." In fact, "econometric models are 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 on the subject of doing "technical economics," time and again, Samuelson repeated 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 in economics. "Often in public lectures I've [Samuelson] had to say that, if MIT pursued the maximand *average* quality of our Ph.D. graduates, we'd need to have stopped with Lawrence Klein our first graduate!" (Samuelson, letter to LRK, May 22 1985. PASP, box 45). Samuelson also insisted on how "we at MIT have always appreciated the key role you [Laurie] played in getting our graduate program off the ground. You were not only a first Ph.D., but also a first Nobel" (Samuelson, letter to LRK 21 June, 2005. PASP, box 45).

During his two years at MIT, Klein also published his first important papers. The first in *Econometrica,* Klein (1943), and the second in *The Quarterly Journal of Economics,* Klein (1944b).[27] These papers reflected not only the technicality of Klein's economics already at the time, but also the sophistication and sagacity of his analysis of broader questions of economic policy and political reform. In the first paper, "Pitfalls in the Statistical Determination of the Investment Schedule" Klein (1943) engaged in a controversy with Mordacai Ezekiel on the estimation of future investment. He argued 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).[28] 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 co-ordinates of the intersection of [the statistical savings schedule and the statistical investment schedule]" (246). Instead of estimating these curves through the use of "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. The second paper, "The Cost of a 'Beveridge Plan' in the United States" (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."[29] Inspired in the British Beveridge Pan, Klein calculated, indeed, 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

[27] From that point onwards, and until the end of the period studied in this dissertation, 1959, Klein published an impressive amount of more than twenty papers in top journals like *Econometrica, The Journal of Political Economy, The American Economic Review, The Review of Economics and Statistics*, and some twenty more in other journals.
[28] In particular, Klein criticized Ezekiel (1942).
[29] Samuelson to Alice Bourneuf, September 12, 1944, PASP, box 45.

bill and the Security, Work and Relief Policies Report of the National Resources Planning Board may be thought to be more relevant at the moment," 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," since it is "more comprehensive than any of the American plans [...] more specific than the National Resources Planning Board's program [and] more finished in structure than the Wagner bill" (423).

The other important work that Klein produced during his stay at MIT was his Ph.D. dissertation (Klein 1944a), of course. According to Samuelson (1995, 3), it was Klein who coined the term "Keynesian Revolution" in his Ph.D. dissertation, which was published with the same title three years after its completion, in 1947.[30] [31] Although Klein's dissertation included several mathematical models of Keynes's different works, the piece could be considered a contribution in the history of economic thought and economics methodology. Contrarily to what one could imagine beforehand, Klein approached Keynes's theory and methodology from a very 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 1947b, 83), 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,

[30] Throughout their lives, both Klein and Samuelson always expressed words of mutual admiration, respect and friendship. Yet, despite a life-lasting friendship maybe also boosted by the small age difference of only 5 years, their correspondence gives the impression that a certain hierarchy remained, where Samuelson never ceased to be the professor and Klein the student. In the preface of his Ph.D. dissertation Klein wrote that "the following pages could never have appeared in their present form were it not for the stimulation afforded by Professor Paul A. Samuelson, at whose feet have sat for two years […] for those arguments which represent real contributions, Professor Samuelson deserves much of the credit. Oftentimes I feel that I have in many cases done nothing more than paraphrase what I have learned in classes and innumerable discussions with Professor Samuelson" (Klein 1944, i).

[31] To Klein, it was clear that the term "Keynesian Revolution" had been used by "a number of economists" especially in England, but also in Western Europe and in the United Stated, where "interesting books and articles appeared with [this] title" (Klein 2011, 506).

and his contribution 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, and for 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. There, 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).[32]

3.3. Cambridge connections: the influence of Wilson's views on science and economics

Another important figure for Klein during his years as an MIT student was Edwin B. Wilson.[33] Although professor of mathematics at Harvard, Wilson kept a strong and close relation to economics and to MIT, particularly through Samuelson who "encouraged [his students] to visit

[32] Klein's Marxian inclinations were also source of debate with Samuelson. In a letter dated November 2, 1945 in which Klein talked about his year at Cowles and in particular about his aggregate macroeconometric model of the US economy, Klein asked Samuelson (presumably as a way of teasing him) whether he "would know how to get in touch with Peter Elias [since Klein had] his three volumes of Marx's *Capital*, which [he had] been reading from time to time" (PASP box 45). Yet, at the occasion of the publication of the second edition of *The Keynesian Revolution* in 1961, although with a few years of delay, Klein sent the latest edition to Samuelson whom in a letter dated July 1, 1966 expressed his gratitude "for the new edition. It is a classic, and with two new chapters it is two-sevenths more valuable" (PASP, box 45).

[33] Edwin Bidwell Wilson was born in Hartford, Connecticut on April 25, 1879. He was considered a polymath, for his knowledge in physics, mathematics, statistics and economics. He graduated *suma cum laude* from Harvard with a major in mathematics in 1899, and went to Yale University where he completed his Ph.D. in 1901. After a one-year leave to the *École Normale Supérieur* to study mathematics between 1902-1903, he came back to the United States and to Yale where he became an assistant professor in 1906. In 1907, he left to MIT where he became full professor of mathematics and physics in 1911, and head of the department of physics in 1917. In 1923, Wilson moved to the Harvard School of Public Health and became professor of Vital Statistics, retiring in 1945. See Hunsaker and Mac Lane (1973) for a biographical essay on Wilson. For an account on E. B. Wilson and his relation with Samuelson see Backhouse (2014) and Carvajalino (2017).

Wilson, [and] urged [them] to learn how Wilson's views on economic analysis were related to his own" (Klein 2011, 504-505).[34] Wilson was "not strictly an economist, but an older scholar with wide interests in many subjects in science and in economics as well as in higher education in general, either in the direct pedagogical sense or in terms of academic influence in general" (504-505). His ultimate purpose in economics was to see "economic thinking better controlled by analysis of the facts of the economic world, and the facts themselves better collected under control by economic thinking," promoting a pedagogical approach to explain applied mathematics in economics and, especially, econometrics under the light of "some important, particular, concrete problem" (Wilson 1946, 173). Although sympathetic to the econometric project in general and to Haavelmo's ultimate objective in particular, Wilson wrote a quite harsh review of Haavelmo (1944), in which he criticized the "extremely abstract and metaphysical" approach of the Norwegian, and his emphasis on "ideal worlds" and "hypothetical or abstract illustrations," in order to develop his econometric theory. Wilson preferred, instead, that econometricians focus on the application of mathematical and statistical methods to relevant and concrete problems through the use of simpler methods, which had "satisfied Maxwell, Boltzman, Gibbs, and Jeans as a basis of their work on the theory of gases and statistical mechanics" (173). According to Wilson,

> there is a small group of econometricians who are well trained in mathematics and who apparently choose to write for one another rather than for economists (or even econometricians) in general. I believe they have something important to say – important not alone for the further development of a purely mathematical dialectic but for economics in the large. Furthermore, I believe that not much will be accomplished by them in the development of economics until and unless economists in general can understand why their contributions are important (173).

As I show in chapter 4, Klein too was critical of the highly abstract and sophisticated methods

[34] For an account on the relations between the economics department at MIT, at Chicago University, and the Cowles Commission see Orozco (2016).

promoted by his colleagues at the Cowles Commission, and advocated for the application of mathematical and statistical methods to concrete economic problems.

4. Redoing Tinbergen's macroeconometric model at the Cowles Commission, 1944-1947

After two years in Cambridge, Klein finally handed in his dissertation on October 1944, eager to get a good position on the job market. Then like now, however, the job market was a hostile place, especially for mathematical economists and econometricians who still "operated in an academic underground [where] job opportunities were scarce [and] post graduate scholarships were not abundant or generous" (Klein 1991a, 112). Samuelson was concerned with the situation of his first Ph.D. graduate, and made an important effort contacting several people to ask for the possibility of an available opening for this "very promising, able, young economist" (Samuelson to Marschak, October 28, 1944, PASP box 45).

4.1. A meager job market for mathematical economists and econometricians

On September 12, 1944, Samuelson sent a letter to Alice Bourneuf of the Board of Governors of the Federal Reserve System, telling that he had "an excellent person […] well trained in statistics, but unlike most statisticians […] also very well trained in economics […] who might be interested in [taking a job] in international economics." One week later, Samuelson also reached out to Howard S. Ellis, insisting on his idea of having Klein recruited by the FED.[35] Despite the enthusiastic responses on Klein's profile, nothing definitive came out of this correspondence in terms of real recruiting options.

[35] Howard S. Ellis earned his Ph.D. in Harvard in 1929. As a faculty member at UC Berkeley from 1938 to 1965, Ellis was president of the American Economic Association in 1949. In 1944, however, he was focused on providing advisory to the FED, rather than on "academic issues." Most of Ellis's work was on monetary and macroeconomic issues. For an account of Elli's work during the 1940s see Herren (2001).

Quite confident about the good impression that his former professors had certainly retained from Klein, Samuelson contacted Berkeley professors Norman S. Buchanan, whom, he believed, "knew [Klein] best at California," and Joe S. Bain, to whom he "heartedly recommended [Klein]" as "really a first-class man." In his letter to Buchanan, Samuelson explained that Klein's work "might be considered to be equally in the fields of business cycles, theory, and money and banking" and that he was "extremely well grounded in mathematical statistics." Indeed, Samuelson insisted that if "the work of a man like Neyman is not well integrated with that of the Department of Economics, Klein could be useful in this sector," and that "although he [had] not done much work in the field of mathematical economics, he could certainly build up a fruitful liaison with the work of Evans and his students in the Mathematics Department." Samuelson emphasized, however, that Klein was "primarily, […] an economist, and a good one." There is no evidence of a response of Bain or Buchanan, but Prof. Malcolm M. Davisson, chair of the Department of Economics at Berkeley, contacted Samuelson on October 8.[36] Davisson explained to Samuelson that "while [he was] not at this time in a position to make a definite offer, [he was] interested in Klein" and that he "had for some time been considering the possibility of adding someone to [the] staff who could work on a cooperative basis with Evans and certain other members of the Mathematics Department." This response, which opened up a real possibility for Klein, was, at the least encouraging.

Davisson asked Samuelson for additional information about Klein's teaching ability, about the possible development of his research interests, and, more specifically, about the possibility of Klein working in the field of insurance. He explained to Samuelson that "Professor [Albert Henry] Mowbray, who for many years [had] carried most of the work in statistics and insurance, [was]

[36] Davison had earned his BA (1928) and first MA (1929) from the University of California, and a second MA (1930) and his Ph.D. (1931) from Harvard.

within a few years from retirement," and that he was "interested, therefore, in someone who might either devote a large part of his time to work in the field of statistics […] or who would be able occasionally […] to offer undergraduate or graduate work in this field" (Davisson to Samuelson, October 7, 1944, PASP box 45). Samuelson's response to Davisson's inquiries was plain and honest. According to him, Klein would "continue to have interests in statistics, mathematical or otherwise, all his life," but he expressed serious doubts about Klein being "interested in teaching actuarial mathematics and insurance more than very occasionally" (Samuelson to Davisson, October 17, 1944, PASP, box 45).[37] In any case, Samuelson asked Ralph E. Freeman and Douglas W. Brown to send their impressions about Klein's ability to teach. Even if there were a number of favorable conditions at UC Berkeley for Klein to pursue a career as an econometrician in California (in particular with the presence of people like Evans and Neyman), it was clear that the Economics Department was rather interested in getting somebody with particular abilities to teach and whose research was centered in a very specific area of actuarial mathematics and insurance theory. Despite Samuelson's effort, it seemed that neither Klein was the right person for this position, nor was Berkeley the right place for him to be other than as a student.

Fortunately for Klein, getting a job did not depend only on his supervisor's efforts. During the Econometric Society meeting in Cleveland on September 13-15, Klein had presented a paper based on his dissertation in a session chaired by Jacob Marschak (Bjerkholt 2014, 769).[38] Marschak had been research director of the Cowles Commission since January 1943, and was assembling a team to embark in a very ambitious research program. It was during that session that Marschak

[37] Concerning Klein's ability to teach, Samuelson responded that he had "had no contact with Klein's undergraduate teaching in economic principles." He went on to say that he would fail to declare "that [Klein was] the best teacher of his generation" and say instead that "he is as good or better than the average."

[38] The title of the paper was: "From the Treatise to the General Theory: A Study in Keynesian Economics." For a detailed account of Klein's recruitment at Cowles see Bjerkholt (2014).

said to Klein that "what this country needs […] is a new Tinbergen model, a fresher approach to it." The Tinbergen model Marschak referred to was, of course, the macroeconometric model that Tinbergen had prepared for his report to the League of Nations in the late 1930s. The "fresher approach to it" was the use of the latest advances in econometric theory not available six years before at the time when Tinbergen published his work, specifically Haavelmo's (1941; 1943; 1944) probability approach to econometrics. Marschak also mentioned to Klein his plans to recruit Haavelmo at Cowles for this project (Bjerkholt 2014, 769).

More than a month after the meeting, Marschak wrote to Samuelson to tell him that he "was favorably impressed by Klein's article and in *Econometrica*, and also by the paper he read at the Cleveland meeting" and that there was "a possibility of offering a job [at Cowles] on conditions which may satisfy him" (Marschak to Samuelson, October 25, 1944, PASP box 45). Marschak also 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 (on October 28), although his letter was considerably shorter than the one he had written before to the Berkeley professors. Nevertheless, Samuelson appraised Klein as "a very promising, able, young economist with an excellent training" and "very well qualified to work on [Marschak's project], most unusually so for a man of his age." Klein joined the Cowles Commission on November 21, 1944, and begun one of the most influential periods of his academic life.

4.2. Klein, the new prodigy at Cowles

4.3. "Sleeping with the enemy": Cowles's relation with the Department of Economics in Chicago

4.4. The student becomes the master: "No recession for the US postwar economy"

[Klein forecasted in 1946 that there would not be a recession in the US economy after WWII, contradicting Samuelson's own prediction "Unemployment Ahead" of September 18, 1944 published in the *New Republic*.]

5. Pursuing the large-scale macroeconometric program after Cowles, 1947-1956

Although the period between 1947 and 1956 was one of relative instability, it was also marked by important personal encounters, institutional visits, and upheavals in Klein's career. After culminating three important years at Cowles, Klein went to Europe where he met with Ragnar Frisch and other important European economists. Together with his wife Sonia, he spent time of the time of his European trip in Oslo, where he closely observed the implementation of economic planning in the context of the postwar reconstruction. Klein also spent some time as well at Tinbergen's Central Planning Bureau in the Netherlands, making short trips to Denmark, Sweden, Switzerland, France, and England. In Sweden he met Herman Wold, Ragnar Bentzel, Erik Lindahl, and Erik Lundberg (Bjerkholt 2014, 781).

5.1. Planning issues in postwar Europe

In 1947, Klein obtained an SSRC fellowship to travel to Europe, and to visit Ragnar Frisch in Oslo. Frisch was delighted with the idea of having Klein as a researcher in his Institute (Bjerkholt 2014, 778). They had met in February of 1947 at the occasion of Frisch's short visit to the University of Chicago to give a talk at the Cowles Commission seminar on "Some basic formulae on demand analysis."[39] After spending much of the war years in the United States, Haavelmo,

[39] During his short 1947 visit to the United States, Frisch showed himself more favorable to the type of empirical work done at the NBER, and less enthusiastic about the of the Cowles Commission (Bjerkholt 2013, 778).

Klein's friend and collaborator, was finally back in Oslo since March 1947. Haavelmo had been promised a position as an economics professor in Oslo, but he had to work during the first year at the Minister of Finance preparing the National Budget for 1948.[40] Klein was fascinated by the work Haavelmo was doing: "I am just getting oriented in the problems of Norwegian economic planning. The whole thing is interesting and is carried out in a more comprehensive peacetime scale than anything we have ever witnessed. Trygve [Haavelmo] is busy with drawing up the National Budget for 1948. This document covers planning in nearly every phase of economic activity here -- manpower, production, consumption, investment, imports, exports, foreign exchange, fiscal policy, prices, rationing, etc." (Klein, letter to Samuelson, December 4 1947, PASP box 45). In a letter to Marschak one month later, Klein wondered if "the average person realizes how much his life is affected by Trygve's decisions" (Klein, letter to Marschak, February 14, 1948, quoted in Bjerkholt 2014, 780).

Indeed, Klein showed himself very enthusiastic about the possibility of co-authoring a paper on welfare economics and the theory of planning (Bjerkholt 2014, 780). The co-authored paper was never written in the end, but Klein produced three lectures on the topic, "Econometric Tools for Planning", an essay on "The Case for Planning," and a paper published in 1948 on the "Planned economy in Norway" (Klein 1948b).[41] In this paper, Klein (1948b, 811-812) not only studied how economic planning was being performed in the Norwegian economy, but he also defended the close relation 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

[40] For a comprehensive account of Haavelmo's return to Norway see Bjerkholt (2015).
[41] The three lectures and the essay are part of the archival material of the Institute of Economics in Oslo, but I have not been able to access them.

> 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."

An important correspondent to discuss with about the issues of planning for Klein was Samuelson who "jotted down a number of comments" on Klein's manuscript on "The Case for Planning," for which Samuelson apologized if he had been "perhaps over critical" (Samuelson, letter to Klein, November 29, 1948). A few days later, on a letter of December 10, 1948, Klein answered that he was "still sticking to [his] point […] that planning is superior to competition because it can effect a modification of the constraints upon the system, this modification not being open to the private enterprise economy." Here, again, he 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." Klein 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," arguing that "planners don't have to use the same technological constraints that private entrepreneurs use" (Klein, letter to Samuelson, 10 December 1948, PASP, box 45).

Klein's Norwegian experience was not only about enthusiastic discussions and memorable encounters, however. At the end of his sojourn, Samuelson came to Oslo wanting "to experience more than tourism in Norway; he plainly and openly wanted a significant visit with Professor Frisch, who lived in the outskirts" of the city (Klein 2011, 509). Yet, a "meeting between the two great economists" did not take place. "Professor Frisch, from his suburban home, made it clear

that he did not want to meet Professor Samuelson," sending "messages to the heart of Oslo, insisting that he would not exchange greeting with the world's rising economics champion from the United States. A leg injury was the stated reason for [Frisch's] inability to visit us" (510). Bjerkholt (2013) has described this as a "minor event" (781), and yet to Klein, Frisch's unwillingness "to offer as much as a handshake to Professor Samuelson […] was hard to watch and hear […] It was painful to experience the attitude and actions of Ragnar Frisch." Klein was also sure that "the other *first* Nobel winner in economics would have provided true hospitality on such an occasion in his own country" (Klein 2011, 510).[42]

5.2. Michigan's happy marriage: The Survey Research Center and the Research Seminar in Quantitative Economics

Klein returned to the United States at the end of 1948 after an invitation by Arthur Burns to join the National Bureau of Economic Research. It is worth noting that 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 (see chapter 4). Yet, if there was somebody that could narrow the gap between the type of work done at both institutions it was Klein. On the one hand, Klein understood the importance of the work of his colleagues at Cowles, and especially of Haavelmo's work which he described as "the inspiration for the research focus at the Cowles Commission" (Klein 1991a, 113). On the other hand, he had always admired the "painstaking tradition of Simon Kuznets" (115), and the unusual attention that him and his team at the NBER payed to data. In other words, Klein's empirical work using the very sophisticated econometric theory developed at Cowles during the 1940s, was in fact, a middle way alternative to the approaches confronted in the

[42] Sonia Klein, Haavelmo and Klein himself, felt even more deceived by Frisch's attitude when they learned that he had indeed met with Alva Myrdal (Gunnar Myrdal's wife) the same day he refused to see Samuelson. Besides, even if they "tried to substitute" they could not do so, and had to see Samuelson feeling let down, and going alone to watch a Norwegian film in dialect (Klein 2011, 511).

"Measurement without theory" controversy, between a very abstract approach of the Cowles, and an NBER's "empiricist" approach (Koopmans 1947).

Klein remained associated to the Bureau until 1951, but starting in 1949 he also became a research associate of the Survey Research Center of the University of Michigan, in Ann Arbor. In 1950, his affiliation to this university expanded to the Economics Department. The type of empirical work in which Klein embarked at Michigan at the beginning was quite different from what he had done before. Whereas Klein had been mainly confronted with the treatment of time series to build his large-scale macroeconoemtric model, in Michigan he faced survey data from an ambitious project on consumer behavior led by George Katona.[43] The type of work done by Katona exerted an important effect on Klein, who during the 1970s and 1980s defended the use of survey techniques to study not only consumer's but also investor's expectations, during the time of the generalization of the notion of rational expectations in macroeconomics.

Also in 1950, Koopmans and Klein toyed with the idea of reviving the macroeconometric project at Cowles. Klein had 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" (Klein, letter to Koopmans, May 29, 1950 quoted by Bjerkholt 2013, 781). Despite two favorable reviews of the project by Tinbergen and Haavelmo, the project did never take off. Klein might have thought too much a risk to embark in such an ambitious project in an institution that was rapidly changing its research interests from econometrics to activity analysis.

Yet, the University of Michigan opened up an important opportunity, "a marriage," for Klein to fulfill his project. This opportunity consisted on establishing the possibility of a "marriage of econometrics with Survey Research – one to give breath and the other depth if a quantitative

[43] Klein had probably met Katona at Cowles in 1946 or 1947.

economics is to emerge adequate to deal with the demands made upon it solving questions of policy" (undated, anonymous document, the University of Michigan archives: "A Proposal for *A Research Seminar in Quantitative Economics*, 1). This marriage would be represented by the creation of the Research Seminar in Quantitative Economics (RSQE) which begun functioning on October 1, 1951, supported by a grant of the Ford Foundation (RSQE 1952, 1), with the "objectives of training faculty and students in quantitative methods of economic research producing substantive results on important empirical problems." The first particular and important project that the seminar proposed to undertake was on the "reconciliation of microeconomic and macroeconomic patterns of behavior." The main objective was "to know whether the two sets of information," micro data obtained from survey methods and macro data obtained from market reports or social accounts, "lead 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, involving "the wholehearted cooperation" between economists, sociologists, psychologists and statisticians.[44]

In a similar way to the dynamics at Cowles, the RSQE would hold weekly meetings "at which seminar members discuss research problems and techniques," with less frequent meeting where external speakers would present papers on relevant subjects for the projects of the seminar.

5.3. Finding stability at the University of Michigan? Obscure times for US democratic values and freedom of thought

"I understand that critical reference has been made to past leftist affiliations of Professor Lawrence Klein, and that opponents of Carter have endeavored to make political capital of this

[44] Apart from Klein and Katona (who would be in charge of cooperating on the estimation of micro- and macroeconomic patterns of behavior, forecasting economic fluctuations and on the interdisciplinary aspects of psychology and economics), the prospective personnel was composed by Musgrave, McCraken, Suits, Morgan, Stolper, Boulding, Craig, Dwyer, and Festinger.

issue. On the basis of detailed knowledge over the last 33 years, I believe that I am in an authoritative position to testify to his political moderation, ideological sagacity, and general good character as an American citizen." (Samuelson, letter to Stuart Eizenstat, Jimmy Carter Presidential Committee, October 15 1976. PASP, box 45).

5.4. "Leaving for the academic freedom of Oxford": Red Scare and HUAC

6. Joining the Ivy League: Pennsylvania University and the John Bates Clark Medal, 1958-1959

6.1. Klein's definitive return to the US: turning down Oxford for Penn

6.2. Consolidating large-scale macroeconmetric modeling at the Wharton School

6.3. Lawrence R. Klein, Clark Medalist 1959

"[…] it is my pleasant task to congratulate you and the American Economic Association on their awarding you the Clark medal. I have a fourfold interest in the matter. First, I am one of your proud teachers. Second, as a member of the Honors Committee, it is my duty to see that the Association gets the best man. Third, as a former Clark medalist, it is to my selfish interest that it not be down graded. Finally, and most important, as your friend I am glad to see you get this richly merited recognition." (Samuelson, letter to LRK, April 13 1959, PASP, box 45).

Concluding Remarks

There are two ways of reading the early years of Klein's fascinating academic life from the perspective of the history of twentieth century economics. First, at the individual level, Klein's life reads as an episode colored by countless encounters and experiences that molded his identity as an economist and as a macroeconometrician. Second, at the level of the economics discipline, Klein's academic life reads as a focal point, representative of a multiplicity of other life-stories embedded within the same historical, social, political, and disciplinary context of postwar transformations. Both readings are important for the type of history I am trying to construct in this dissertation.

On the one hand, the first reading allows us to "repersonalize" the history of macroeconometric modeling, and to recognize this practice not only as a way to understand the world, but also as an "existential project" (Düppe and Weintraub 2014, xv) in which the macroeconometrician seeks for self-identification within a particular community, while forging, at the same time, a new kind of identity marked by the individual events of her life.[45] These events, in turn, affect scientific practices and reshape the economics discipline. On the other hand, taking Klein as a focal and representative point in the history of economics, provides us with a well-informed perspective to understand the complex dynamics and transformations that were happening within the discipline.

In particular, Klein's economics career contrasts with the narrative that presents the US American post WWII period as one of "neoclassical" uniformity and domination. In fact, Klein's way of doing economics shows that a great amount of pluralism remained, inherited from the eclectic and pluralistic interwar period. It also shows that despite its relative fragile position,

[45] Given the importance of Klein's contributions to economics and his fascinating life, however, a biography of him, in the traditional sense, would be a perfectly legitimate subject to complete a Ph.D. thesis or a book.

mathematical economics and econometrics rapidly gained in importance, extending their networks from Massachusetts to California, and from Michigan to Illinois, supported by a weakened but still symbolic European network principally stemming from Norway, the Netherlands, France, and England. In addition, Klein's life helps us to understand the institutional arrangements that were at stake both to build economic laboratories in the era of "big science," and to develop new graduate and undergraduate programs in economics to teach particular econometric and mathematical methods first in specialized institutions, then in most of the US institution.

Klein "happened to be fortunate to have studied at Berkeley and at MIT, where the subject [of econometrics] was first appearing" (Klein 1991a, 112), and to encounter people like Kuznets, Samuelson, or Wilson. Rather than "fortune," however, Klein's path reveals both the formation of important institutional conditions that allowed him to conduct the kind of econometric work that inspired and defined him, and his ability to integrate networks and communities and gain scientific credit. It is not just that Klein "seemed to be" in the right place, at the right moment, and with the right people throughout his academic life, but that he quickly learned to identify which were the right places, moments, and people to be with. In addition, Klein's brilliancy and geniality (Mariano 2008), provided him with the individual attitudes necessary to grant him access to the most selective community of economists.

In a nutshell, his collaboration with mathematically oriented economists and statisticians in Berkeley was enough to get him the necessary credit to be noted by Samuelson. It was not so much the fact of being the first Ph.D. graduate of MIT which marked Klein's academic career, but it was rather the fact of being Samuelson's first Ph.D. student and friend, which truly opened up a number of important possibilities for Laurie. Later on, his work at the Cowles Commission and his collaboration with people like Marschak, Haavelmo, and Koopmans overshadowed the "disappointing" results of his first macroeconometric model. Klein's relation with the European

econometricians, symbols of the transformation of economics, as well as his awarding of the John Bates Clark Medal, were certainly central to consolidat his scientific credit, which was confirmed with the later dissemination of macroeconoemtric modeling, and with his Nobel Prize in 1980. Not all the crediting strategies worked well for Klein, however. Getting a job after completing his Ph.D. was not necessarily easy, as were not the harsh criticisms directed by Friedman since the late 1940s. Klein's attempt to get tenured in the University of Michigan was also unsuccessful, even if the reasons for this failure were external to the university and are to be found in a very obscure and anti-democratic period in the history of the United States darkened by McCarthyism. In any case, this account of Klein's early academic years, tells us how he became to be a macroeconometrician due to both the possibility conditions of educational and disciplinary transformation, as well as to his own personal search for a scientific identity.

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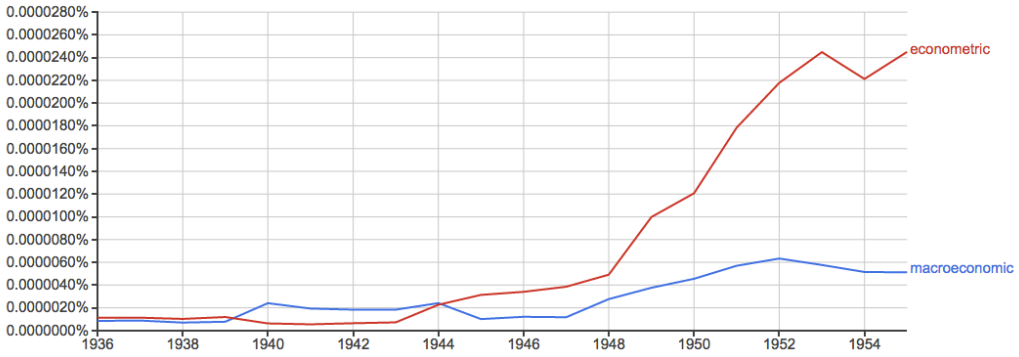
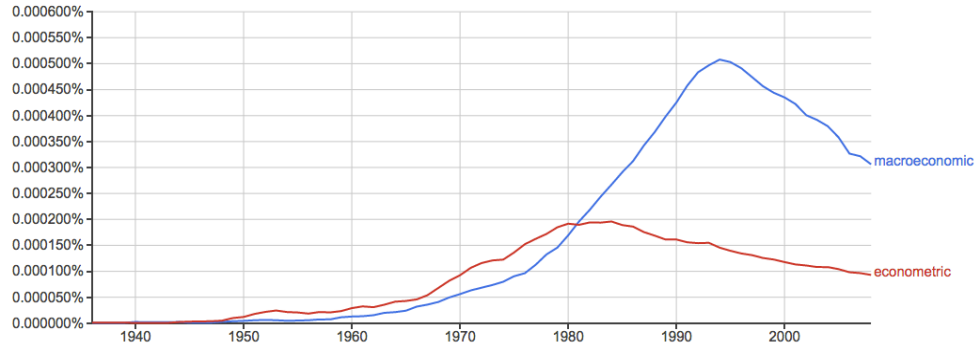
Annex 1

Graph 1: Percentage of hits of the words “macroeconomic” and “econometric” in English book titles between 1936 and 1955.



Source: Ngram Viewer

Graph 2: Percentage of hits of the words “macroeconomic” and “econometric” in English book titles between 1936 and 2008.



Source: Ngram Viewer

Annex 2

Outline of the Dissertation: *Economics as a 'tooled' discipline: Lawrence R. Klein and the making of macroeconometric modeling, 1939-1959*

Table of contents and chapter summaries

Chapter 1: Introduction

Part I: Klein's formative years

Chapter 2: The making of a macroeconometrician [Included as writing sample]

Chapter 3: Reconciling Keynes and Tinbergen?

The Keynes-Tinbergen controversy on the role of econometrics to test economic theories remains of paramount importance to understanding the evolution of macroeconometric modeling. Even after Keynes's recognition of the value of Tinbergen's work, a general impression persisted that his critique of Tinbergen was not only destructive but also uninformed, attenuating the relevance of his claims. I consider Lawrence R. Klein's project to "redo" Tinbergen's work as a well-informed reaction to Keynes's criticism, which decisively contributed to the further development of macroeconometric modeling. As an expert in Keynesian thought and as a leading figure of Cowles's macroeconometric program, Klein surmounted the difficult task of reconciling the Tinbergenian world which strove for the implementation of technical and rigorous devices from which to draw inferences with Keynes's world in which a priori theoretical claims were the only possible way for discovering the functioning of the economy.

Chapter 4: Macroeconometric models as a pluralistic scientific tool for economic planning

This chapter provides an account of Lawrence R. Klein's distinctive way of doing econometrics. Focusing on Klein's time at the Cowles Commission (1944-47), I discuss a series of fundamental publications and events that were decisive in shaping his *image* of econometrics. In particular, I argue that Klein's adoption of a flexible and practice-oriented methodology, and his endorsement of pluralistic economic theories, resulted from his participation in actual empirical model-building. Furthermore, I show that Klein's flexible approach contrasts with the prescriptive methodology resulting from the highly abstract and theoretical work led by his colleagues at Cowles. I conclude that Klein's distinctive *image* of econometrics allowed him not only to enrich the process of model specification, but also to pursue the macroeconometric program beyond the 1940s, and to remain optimistic about what he thought was the political objective of econometrics: economic planning and social reform.

Part II: The Consolidation of macroeconometric modeling

Chapter 5: Two empirical approaches to macroeconomics: The Walras-Marshall divide

This chapter revisits the Walras-Marshall divide putting it at the center of a longstanding debate between two empirical approaches to macroeconomics: Cowles's econometrics and NBER's statistical economics. The introduction of econometrics and the transformation of economics into a "tooled" discipline during the 1940s and 1950s changed the relations between economic theory, applied economics and the policy sphere. I argue that rather than bridging the gap between theory and data, macroeconometrics radically transformed the preeminence of theory over application, data and political issues in economics, and conclude that independently from the economist herself, the macroeconomic practice of the twentieth century (which implies adherence to the econometric tool) does not allow for a dissociation of theory, application and policy, but instead combines and fuses them into a single model system: macroeconometric modeling (whether structural or not).

Chapter 6: Macroeconometric modeling as a "photographic description of reality" or as an "engine for the discovery of concrete truth"? Friedman's methodological debate with Cowles

Taking as starting point the argument advanced in chapter 5, I discuss the historical development of the longstanding debate between the NBER and Cowles. I argue that the differences between these approaches do not consist only in the use of different statistical methods, economic theories or political ideas, but are deeply rooted in methodological principles and modeling strategies that raise questions on both the way macroeconometricians represent and understand the world, and on how they deal with problems of operationality and concrete-problem solving. While the Cowles's Walrasian approach necessarily considers the economy as a whole, despite the economist's inability to observe or understand the system in all its complexity, the Bureau's (and especially Friedman's) Marshallian approach takes into account this inability and considers that economic models should be perceived as a way to construct systems of thought based on the observation of specific and smaller parts of the economy.

Chapter 7: Friedman and Klein on statistical illusions

Focusing on the controversy between Milton Friedman, Gary Becker, and Lawrence Klein of the end of the 1950s, I provide an account of the early discussions on how to evaluate macroeconometric models performance. At this occasion, Friedman and Becker questioned Keynesian macroeconometric models for their inappropriate treatment of the consumption function, and for their inability to yield accurate predictions of income, resulting from the adoption of the "wrong" criterion to judge models performance. While macroeconometricians adopted reduced forms extrapolations to evaluate their models, Friedman and Becker insisted on the necessity of carrying out full model simulation to conduct sound model selection. Independently of Friedman and Becker's critical tone, I conclude that their argument can be interpreted as a constructive critique and as a precursor of a criterion to evaluate models performance that became common ground around macroeconometricians in the subsequent decades: full- or dynamic model simulations.

Chapter 8: Conclusions