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BY

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CENTER FOR THE
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AT DUKE UNIVERSITY

Macroeconometric modeling as a "photographic description of reality" or as an "engine for the discovery of concrete truth"? Friedman and Klein on statistical illusions

Erich Pinzón-Fuchs[1]
(September, 2016)

Abstract

This paper discusses a longstanding debate between two empirical approaches to macroeconomics: the econometrics program represented by Lawrence R. Klein, and the statistical economics program represented by Milton Friedman. I argue that the differences between these two approaches do not consist in the use of different statistical methods, economic theories or political ideas. Rather, these differences are deeply rooted in methodological principles and modeling strategies inspired by the works of Léon Walras and Alfred Marshall, which go further than the standard opposition of general vs. partial equilibrium. While Klein'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, 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.

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> My own hunch […] is that […] attempts to proceed now to the construction of additional models along the same general lines [of the Cowles Commission] will, in due time, be judged failures.
>
> *Milton Friedman, 1951*

> Contrary to Friedman's tastes, other researchers have sought improvement in the Keynesian consumption function through the introduction of new variables. There are great limits to the extent to which one can come upon radically improved results by juggling about the same old variables in a different form. Instead of adhering to the "rule of parsimony," we should accept as a sound principle of scientific inquiry the trite belief that consumer economics, like most branches of our subject, deals with complicated phenomena that are not likely to be given a simple explanation […] I venture to predict that much good work will be done in the years to come on adding new variables to the consumption function and that it will not be illusory.
>
> *Lawrence R. Klein, 1958*

Introduction[2]

Through his structural macroeconometric modeling approach Lawrence R. Klein pursued a precise lifelong purpose: to build large-scale macroeconometric models that would reflect the complete structure of the economy through a system of simultaneous mathematical equations. It was evident to Klein that structural macroeconometric modeling would little by little reveal the structure of the economy through painstaking team effort, through the use of improved economic data and much tinkering, as well as through the revision of model specification and re-estimation of parameters. For a very optimistic Klein macroeconometric models would, eventually, be able to provide an accurate description and representation of economic "reality."[3]

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[2] I want to thank Marcel Boumans, Kevin D. Hoover, Harro Maas, Emilie Lefèvre, the participants of the Albert O. Hirschman seminar of the University Paris 1 Panthéon-Sorbonne, the HOPE Workshop, and the 2016 HISRECO Conference for their valuable comments and remarks on earlier drafts of this paper.

[3] "Representation" in the case of models can be understood in two senses: (1) the sense used in this introduction indicates representation as the establishment of a direct correspondence (resemblance or denotation) between the model and the "real world." This is what Frigg (2010) calls t-representation – "t" for the target of reality the model seeks to correspond to. Another way of understanding "representation" stems from "pretence theory" as presented by Walton (1990) and also by Morgan (2012). Representation, in this sense, (2) indicates an object that is imaginable within a particular set of rules defined in a model system. Frigg (*ibid.*) calls this kind of representation p-representation, "p" for prop. "Reality" too, can be understood in two senses. Here reality will not be understood in the sense of classical ontology as an external and objective entity, but rather in the sense of "historical ontology" (Hacking 2002) or even in the sense of some kind of "imagined ontology" that is possible to conceive, only under the rules that are set to build the model system.

Defending a Marshallian approach, Milton Friedman would radically disagree with this position characterizing Klein's Walrasianism as a "photographic description" of reality (Friedman 1949). His criticism, however, did not rely on the classical opposition attributed to Walras and Marshall of general equilibrium *versus* partial equilibrium (see for example De Vroey 2009; 2016). In fact, there is a far more profound methodological claim standing between Friedman's disagreement with Klein. Ever since Friedman started to attend the Cowles Commission seminars in the 1940s to discuss about econometric modeling (Epstein 1987), he continuously expressed his skepticism about this approach, claiming that structural macroeconometric models *à la* Klein would "in due time be judged failures" (Friedman 1951, 112). Friedman's main criticism actually derived from the fact that he had a fundamentally different view on the role that economic models should play both within the economics discipline and within the political sphere.

Klein's conception of models, admittedly inspired by Léon Walras, focused on the idea that models should be capable to represent and capture the essential structure of the economy as a description of reality. Friedman, however, in his own conception inspired by Alfred Marshall, considered that economic theories – or models – should be perceived as a way to construct systems of thought through the observation of specific parts of the economy, and only as selective representations of it. [4] Hence, economic theory and, in this case, macroeconometric models, should be considered as introducing "systematic and organized methods of reasoning" (Marshall 1885, 159) allowing for a better understanding and analysis of the economy. These methods of reasoning would therefore constitute an "engine for the discovery of concrete truth" (159).[5]

Friedman sustained a long debate with several members of the Cowles Commission between the early 1940s and the late 1950s, coinciding with a period of high proficiency in his work on economic methodology. To illustrate the undergoing change in the discipline and to make a parallel to Shackle's (1967) book, one could say that during these years economics

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[4] In twentieth century economics, it is very difficult to make a clear differentiation between a "theory" and a "model" for at least three reasons. First, understood as "systems of thought," both theories and models fulfill a similar function: they serve as "engines" to produce knowledge, and allow for the understanding of the economy. Second, standardization of the term "model" was not reached by the 1940s and 1950s, and so economists like Friedman and Klein used these terms, sometimes, in an undifferentiated way. Third, and this is the point of the present paper, the introduction of tools like econometrics reconfigured the relationship between pure theory, application, policy, and data in macroeconomics, leaving not much space for the differentiation between theories and models.

[5] Strikingly enough, these methodological approaches would place Friedman in a closer position to John Maynard Keynes, while they would distance Klein from the English economist. See Lawson and Pesaran (1985), and Carabelli (1988) for a detailed discussion of Keynes's methodology.

found itself in a transition stage from *The Years of High Theory* of the 1930s to what could be called *The Years of High Methodology*.[6] Partially responsible for the remarkable quality of the methodological work Friedman produced is the empirical turn of economics, and more particularly, the emergence and further development of econometrics at Cowles.[7] Friedman's discussions with Oskar Lange, Jacob Marschak, Tjalling Koopmans, and finally Lawrence Klein, elucidate in concrete terms the difference between the purposes and the uses of economic modeling between what one could call US-Walrasian and US-Marshallian economists.[8]

I will argue that both Friedman's and Klein's modeling practices yield a system of thought (or an engine) allowing for the production of knowledge.[9] Following Frigg (2010), I will use the term "model system" to refer to Marshall's "engines."[10] "Model systems can be (and often are) used to represent a target system, but the intrinsic nature of the model system does not depend on whether or not this is the case; model systems are objects of sorts and as such can be studied in themselves" (Frigg 2010, 252). The way Klein and Friedman would construct their model systems is, however, very different. The difference relies not only on the purpose that the authors assign to their constructed systems (economic planning or mere understanding of the economy), but also on the actual target that the system is supposed to represent and explain (the whole economy or a fraction of it like a particular market).

Contrary to Friedman's opinion, I will argue that even if the goal of Klein's models was to provide a representation as close as possible of the economy, Klein's modeling does not yield a naive "photographic description of reality." In fact, it produces a whole new scientific practice that considers the model system as an ever-evolving object. The system's evolution does, of course, not occur by itself, as it could arguably be the case for a biological organism. Rather, the assemblage of objects, practices and people around the system are the driving force of its evolution. Additionally, Klein thought that his model system should attain as high a

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[6] There have been, of course, many other periods that could be described as "years of high theory" in economics, as well as other periods that could be called "years of high methodology" too. For instance, the turning of the twentieth century was marked with important works in methodology (see Neville Keynes's work) as well as by paramount controversies such as the *Methodenstreit*.

[7] Franciso Louçã (2007) calls this period, in fact, *The Years of High Econometrics*.

[8] For a more detailed account of Friedman's debates with the members of the Cowles Commission see Boumans (2013), DeVroey (2009a; 2009b), and Epstein (1987).

[9] My argument goes well in line with Mary Morgan's (2012, 38) thesis that "modeling is not an easy way to find truths about the economy, but rather a practical form of reasoning for economists, a method of exploration, of enquiry, into both their ideas and the world."

[10] I prefer "model systems" rather than "engines," because "systems" refer to a more complex kind of object that seems to be more flexible, conceding for the possibility of evolution and adaption. "Systems" might be more "organic." "Engines," on the contrary, seem to be too close to mechanics, giving the impression of being rather inflexible, producing more or less the same output repeatedly.

degree of complexity as possible, since the model system would perform as a tool to understand and to act on the target system.[11]

Friedman's model system is different from Klein's in that the target system he wants to illuminate is less ambitious. Indeed, the ultimate goal of Friedman's model system is to understand the economy, not to act on it; this understanding of the economy can (and should) happen only through the exploration of a small part of it. Such an approach would yield a model system allowing for the understanding of the most important fundamental relations that the author can subsequently extrapolate to the rest of the economy, without any ambition of representation, but solely as an instrument to grasp reality.[12] In other words, Klein sees the model system not only as a complex tool for understanding the world and for acting on it, but also as an important (and necessary) element to persuade policy makers and other economists of his own policy recommendations. Friedman sees his model system as a simple tool considering only a modest target system from which to observe the behavior of variables. This partial observation on the modest target system should illuminate the researcher's understanding of the behavior of the variables in the whole economy.

The purpose of this paper is to compare both Klein's and Friedman's methodological positions and to give a historical account of the controversy between these two authors (and the approaches they represent). This longstanding debate shows at least one clear thing: the fact that economics became a "tooled" discipline during these years completely changed the relations between the spheres of what Walras (1954) [1874] called "pure economic theory," "applied economics" and "social economics."[13] I will argue that rather than just "bridging the gap between theory and data" (Spanos 2014) the introduction of econometrics radically transformed the preeminence of theory over application, data and political issues in economics. Independently from the economist herself and from her purpose, 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).

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[11] To Klein, macreconometric modeling was a powerful, scientific, and pluralistic tool for social planning (see Pinzón Fuchs, 2014).
[12] Friedman's (1944; 1949; 1953) emphasis will be on the capacity of the system to predict.
[13] Mary Morgan (2003) would prefer to refer to economics as a "tooled-based discipline" rather than as a "tooled" discipline.

I. Friedman's debate with the Cowles Commission

The relevance of empirical work for the development and testing of theories, and the inapplicability of highly formalized systems

The debate between Friedman and the Cowles Commission occurred mostly during the time the headquarters of the Commission were based in Chicago, between 1939 and 1955. These years also coincide with Friedman's most active period in his writing on economic methodology (Boumans 2011). The debate must be situated within a period where (at least) two programs of empirical research were confronted, each of which would claim to provide the best empirical approach to economics (*ibid.*). This confrontation particularly took place between (1) the "statistical economics" approach stemming from the tradition of the National Bureau of Economic Research (NBER) and particularly from Wesley Claire Mitchell, and (2) the "econometrics program" inspired, among others, by the works of Ragnar Frisch, Jan Tinbergen, and the Cowles Commission.

Even if economic theory played an important role in the approach of the statistical economists of the NBER like Mitchell, their emphasis was rather on the observation of "facts."[14] Taking into account the problem of "multiple hypotheses" for the particular case of the business cycles, Mitchell (1913, 19-20) considered that there was a "better prospect of rendering service if we attack" directly the task of observing, analyzing and systematizing the phenomena of prosperity, crisis, and depression, instead of taking the "round about way of considering the phenomena with reference to the theories." On the contrary, the econometric approach led by Frisch, Tinbergen and the Cowles Commission would emphasize on the predominance of theory (both economic and statistical) over the observation of "facts." The empirical and observational phase would come only after the establishment of theory.[15]

The relation between Friedman and the Cowles Commission is an old and longstanding one. Although I think that mutual respect was always a common denominator, this relationship has been, for the most part, one of conflict and disagreement, and therefore one of abundant

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[14] For a more detailed account of the statistical economics tradition see Morgan (1990, chapter 2.2) and Hammond (1996, chapter 1).

[15] It is difficult to provide a clear-cut definition and differentiation between these two approaches that would make justice to all the authors, since the real differences can be seen as a matter of emphasis between the relative importance that either economic statisticians or econometricians would attribute to the theory or to the observation of facts. The differentiation of the two approaches becomes even more complicated when one thinks about "theory" in two different ways: economic and statistical theory.

fertility.[16] One cannot forget, on the one hand, the problematic relationship that existed between the Department of Economics of Chicago University and the Cowles Commission since its move to Chicago, and, on the other hand, the difficult relationships between Cowles and the NBER.[17] One cannot forget either that Friedman was an emblematic figure in both the Economics Department and the NBER.

One of the first encounters between Friedman and one of the members of Cowles happened at the beginning of the 1940s after the publication of Oskar Lange's (1944) *Price Flexibility and Employment.*[18] Lange's goal "was to examine the Keynesian issue of whether a decrease in the money wage could restore full employment in the face of involuntary unemployment" (DeVreoy 2004, 3). In 1946, Friedman seemed to be very concerned about the existence of a multiplicity of theories explaining economic phenomena – or as the econometricians put it, about the problem of "multiple hypotheses." This problem of multiple hypotheses concerns the specification of a model and so the problem of "model selection." Friedman was also concerned about the problem of "identification" of a theory, "whether statistical estimation could lead to the desired relationship derived from non-mathematical economic theory, and whether statistical estimation could help discover true economic relationships" (Qin 1993, 96). Both the specification and the identification problem are important since, they represent two of the stages at which the econometrician needs to make proof of her economic intuition. In other words, the identification stage is when the pure statistical side of econometrics takes a secondary role and the model gets confronted with economic theory.

Friedman's criticism of Lange's approach, was that Lange's highly abstract and mathematical methods would lead him to forget about the real world, and so he would not be able to give "form and content" to his "abstract functions." Lange would use only "casual observation" to evaluate the relevance of the proposed functions. This approach would produce systems with an infinite number of possible specifications that could be obtainable through the

[16] It is worth noting, for instance, that Friedman was nominated to be part of the Cowles Commission. In September 1942, when Thoedore O. Yntema (research director of Cowles from September 1939 to December 1942) resigned the Cowles's research directorship, the economics department presented Friedman's nomination (Bjerkholt 2015, 22). Besides Jacob Marschak and Paul Samuelson nominated by Yntema himself for the position of research director, the names of Milton Friedman, Allen Wallis, Arthur Burns and Abraham Wald were suggested by the department of economics (*ibid.*, 23). However, Friedman never made it to the short list of candidates, which was composed of Marschak, Gottfried Haberler, Arthur Burns and George W. Terborgh. In the end, Marschak was appointed research director starting on January 1, 1943.
[17] For a description of the relations between the Department of Economics and Cowles see Mirowski and van Horn (2009). The complicated relations between the NBER and Cowles are explained in Mirowski (2002).
[18] It is worth noting that Oskar Lange was a researcher at the Cowles Commission from 1938 to 1945, although he was absent between 1942 and 1944, since he went to Columbia University as a visiting professor (see Boumans 2013).

permutations and combinations of the equations. To Friedman (1946, 618), Lange used theory "as a taxonomic device,"

> [starting] with a number of abstract functions whose relevance – though not their form or content – is suggested by casual observation of the world-excess demand functions (the orthodox demand schedule minus the orthodox supply schedule) for goods and money, the variables including present and future (expected) prices. He [Lange] then largely leaves the real world and, in effect, seeks to enumerate all possible economic systems to which these functions could give rise. The kind of economic system and the results in that system will depend on the specific character of the functions and their interrelations, and there clearly are a very large number of permutations and combinations.

Friedman criticized Lange for focusing too much on the formal structure and on the logical interrelations of the parts, considering "unnecessary to test the validity of his theoretical structure except for conformity with the cannons of logical analysis, [and] not empirical application or test" (618). Lange reached conclusions that no observed facts could contradict, providing formal models of imaginary worlds rather than generalizations about the real world. In a nutshell, "the resulting system of formal models has no solid basis in observed facts and yields few if any conclusions susceptible of empirical contradiction" (619). This emphasis and "inappropriate" formalizing of theories by Cowles's approach would prove that their models would not be relevant for policy advice. If the researcher using this abstract approach wanted to give some policy advice or understand the world, she would be obliged "to escape from the shackles of formalism" and to abandon the (highly abstract) theory, being confined to commit disastrous errors of logic. That kind of theory or modeling would be worthless. To Friedman (1946, 631),

> A man who has a burning interest in pressing issues of public policy, who has a strong desire to learn how the economic system really works in order that that knowledge may be used, is not likely to stay within the bounds of a method of analysis that denies him the knowledge he seeks. He will escape the shackles of formalism, even if he has to resort to illogical devices and specious reasoning to do so. This is, of course, a poor way to escape the shackles of formalism.

According to Friedman, "[a] far better way [to escape the shackles of formalism] is to try to devise theoretical generalizations to fit as full and comprehensive a set of related facts about the real world as it is possible to get" (631). Friedman's criticism of Lange's work contains two major points that will be brought up throughout his debates with the Cowles Commission.

First, the exaggerated focus on internal logical rigor and on the formalization of the system; second, the neglect of the role that empirical observation should play both in the construction of a theory or model and in its testing procedure, rather than as a mere indicator of the relevance of including a particular variable or not in the model (as Lange allegedly did).

The construction of a tool for economic planning

The debate between Friedman and the Cowles Commission evolved as well, mainly because the "scientific objects" and the approaches both sides were producing were in constant evolution. Lange's theory was, in fact, not completely in line with the approach adopted in the mid and late 1940s at Cowles, when empirical work enjoyed a short but fruitful impulse. Since Jacob Marschak's appointment as research director in January 1, 1943, the Commission had set a clear new goal: to advise firms and government agencies, or as Marschak himself put it, to perform "social engineering" (Epstein 1987, 61).[19] [20] To Marschak, the major problem in economics was that there was no economic theory accounting for a complete and causal explanation of macroeconomic phenomena.[21]

Furthermore, in the case of business cycles, there was a plethora of theories and pseudo-theories pretending to provide some accurate explanations of the phenomenon. Yet economists were not equipped with the necessary tools to distinguishing between "good" and "bad" theories:[22]

> Any specification of *the* theory would, at present, mean merely setting one's mind on preconceived ideas affected by emotional preference, as in the case of the role of wage rigidity, monopolies, income distribution, and public spending (Marschak to Robert Redfield, February 1944, quoted by Epstein 1987, 65)[23]

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[19] For a more detailed account of Marschak's life and career see Hagemann (1997; 2011).

[20] The term "social engineering" was rapidly "toned down to economic policy, [however,] probably to avoid connotations of 'central planning'" (Epstein 1987, 61-62).

[21] This plethora in economic ideas, however, might be attributable to a certain image of the economic discipline of the time, characterized by a high degree of pluralism. As Morgan and Rutherford (1998, 4) put it: "It was genuine pluralism, to be taken in a positive sense. Pluralism meant variety, and that variety was evident in beliefs, in ideology, in methods, and in policy advice […] Economists felt at liberty to pursue their own individual combinations of ideas. Pluralism […] describes not only the difference between individuals; pluralism was in each economist."

[22] See Morgan (1990) in particular Part I for an account of the situation with business cycle theories in the 1920s and 1930s. See also Haberler (1937) for a contemporary account of this plethora of theories.

[23] To Klein too, "it [was] desirable to provide tools of analysis suited for public economic policy that are, as much as possible, independent of the personal judgments of a particular investigator. 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 1947, 111).

In other words, the "problem of multiple hypotheses" and of model selection was a major and urgent issue for economists to resolve, since it represented an obstacle for the main purpose set by Marschak: to make sound economic policy recommendations. Economists were in need of a tool that would allow them to choose between theories. The first goal of the Cowles Commission during the 1940s was, therefore, to provide economists with that particular tool.

The theoretical construction of the method needed the active participation of highly skilled groups of economists and statisticians. By 1944 with the publication of Haavelmo's "The Probability Approach in Econometrics," the purpose of Cowles was, if not reached, at least on good track, allowing for optimism about the possible future applications and results of the program. By 1949, however, actual application and results were still expected by the community. In mere abstract terms, although Cowles's methods seemed to be fulfilling an important gap, they were rather confusing for economists in general (Wilson 1946, 173). Yet, the usefulness of these methods was still to be demonstrated. As Vining (1949, 77) put it,

> While these [Cowles's] methods are intriguing and the results of their application will be awaited with keen interest, they are as yet untested. Acceptance of them as the only, or the best, method for reaching economic truth must hinge on results, not on any advance statement, no matter how persuasive, of their potential merits. Until such evidence is available, they must be considered an exceedingly narrow class of methods, and an insistent appeal to use them, and them alone, as an invitation to put a strait jacket on economic research.

The NBER Conference on Business Cycles and the conception of "naïve models"

With the arrival of Marschak to the research directorship of the Cowles Commission, a research seminar was established, which was "initially [held] every three or four weeks but [which was soon organized on] a bi-weekly schedule" (Boumans 2011, 3). Not only the members of the Cowles Commission were regular attendants to this seminar. There were also numerous researchers coming from other institutions that would present their own work at the seminar. [24] Eminent economists coming from other institutions, like Jan Tinbergen, John von Neumann, Richard Stone, John R. Hicks, Ragnar Frisch, Karl Menger, Harold Hotelling, John Nash, or Collin Clark, among many others, did participate in that seminar. The most active presenters and participants, after the members of the Commission themselves, of course, were

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[24] A complete list of the papers presented in the seminar from 1943 to 1955 can be found on the website of the Cowles Foundation under the heading "Commission Seminars": http://cowles.yale.edu/commission-seminars.

the members of the University of Chicago (mainly, but not exclusively, from the Economics Department). Jacob Viner, Donald M. Fort, Martin Bronfenbrenner, Rudolph Carnap, James "Jimmie" Savage, Louis Thurstone, and later Earl Hamilton and Gary S. Becker, among many others, also participated at the Commission's seminar.

Milton Friedman participated in the Cowles's seminar too, and was, apparently, one of the most active and assiduous participants. It was "[a]fter attending the Cowles seminars [that Friedman] introduced the idea of the naïve model" (Epstein 1987, 109).[25] The aim of these naïve models was to compare the predictive performance of a structural macroeconometric model such as those provided by Klein. [26] Friedman's emphasis on testing the predictive performance of the macroeconometric models pointed out a very sensible issue, casting doubt, indeed, on the results obtained by the Cowles Commission and especially by Klein's models.

According to Klein (1951c, 1), his

> main objective [was] to construct a model that [would] predict, in the [broader] sense of the term. At the national level, this means that practical policies aimed at controlling inflationary or deflationary gaps will be served. A good model should be one that [would] eventually enable us to forecast, within five percent error margins roughly eighty percent of the time, such things as national production, employment, the price level, the wage level, and the distribution of the major shares of national income – wages, industrial profits, and agricultural income.[27]

Despite the fact that the first macroeconometric modeling attempts obtained some important results, both Klein and the other members of Cowles had serious reservations about the validity of these results (Marschak 1946 quoted by Epstein 1987, 105; Klein 1991). It was, of course, a first attempt in which a lot of effort had been invested, but much work had still to be done. Klein, however, remained optimistic long after the Cowles's "retreat" from the macroeconometric modeling project (see Epstein, 1987; Bjerkholt, 2014) and continued pursuing his goal. Yet, important criticisms were still to be faced.

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[25] In this seminar, Friedman presented a paper "Utility Analysis of Gambling and Insurance" on October 23, 1947, and two papers in 1952, the first on January 10 "Price, Income, and Monetary Changes in Three Wartime Periods" and the second on November 20 "The Effect of Individual Choice on the Income Distribution" (Cowles Commission, 1947; 1955). Most importantly, Friedman's idea of the "naïve models" might have appeared when criticizing the Cowles's approach.

[26] Roy Epstein (1987, p. 109) claims that naïve models were developed "to compete with the structural models and [that Friedman] even claimed a structural interpretation for it." However, as I will show later, the naïve models were conceived from the beginning just as a way of assessing the predictive performance of macroeconometric models.

[27] "Related objectives [of his project were] the testing of alternative business cycles and the description of history." (*ibid.*).

One of these criticisms came from inside Cowles itself, and, once Klein had left the Commission.[28] Carl F. Christ, whose background was in physics, entered the Commission in 1947 as a SSRC Fellow (Bjerkholt 2014, 779). His principal work at Cowles was to revise Klein's models. Christ claimed that his "problem was to choose the 'best' [structure,]" which would be the one giving "the most accurate predictions of the future" (Christ 1949, 3). Following Friedman's suggestions – as well as Andrew Marshall's[29] (1949) – Christ (23) added that,

> In order to be completely happy with a model, we would like to know that it meets one additional qualification: its errors of prediction, i.e. its calculated reduced forms disturbances v^*, should be no larger, on the average over a number of years if not in every year, than the errors made by the same naïve noneconomic hypothesis such as 'next year's value of any variable is equal to this year's value plus a random disturbance.'

Christ would argue that "if this condition is not met, then we will want to use the naïve model (as [Andrew] Marshall calls it) instead of our complicated econometrics, or at least to revise our econometric analysis at certain points" (*ibid.*). Even if his tone seemed that of a discouraged and unconvinced researcher (at least compared to Klein's optimistic tone) he made another statement to defend the econometric approach: "even if such a naïve model does predict about as well as our econometric model, our model is still preferable because it can predict consequences of alternative policy measures and of other exogenous changes, while the naïve model cannot" (*ibid.*). Christ used two naïve models to test the accuracy of prediction of Klein's models:

Naïve Model I: $\quad\quad y_t = y_{t-1} + \varepsilon_t$

Naïve Model II: $\quad\quad y_t = y_{t-1} + (y_{t-1} - y_{t-2}) + \varepsilon_t$

In November 1949, the NBER organized a *Conference on Business Cycles*, where Christ's results were presented. Both Friedman and Klein commented on these results. Klein (1951a, 114-115) was not particularly pleased about the conclusions reached by Christ, reacting forcefully and rejecting "any personal responsibility" for this work. His reaction was based on three counterattacks:

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[28] Klein left the Cowles Commission and Chicago in the middle of June1947 for a fruitful sojourn in Europe, where he met among others with Ragnar Frisch, Trygve Haavelmo and Jan Tinbergen. See Bjerkholt (2014).
[29] Andrew Marshall was a student in Chicago whose Masters' thesis consisted on the testing of Klein's model. It was Marshall who coined the name of "naïve models" to refer to the kind of models proposed by Friedman to test the accuracy in prediction of macroeconometric models.

Carl Christ has presented a splendid methodological account of a procedure for testing the validity of econometric models, but like many other econometric contributions of recent years it is weak in empirical or substantive content. I shall argue that his time series data contain an obvious gross error, that he has not chosen a desirable postwar revision of my prewar econometric model, and that his forecasting technique is both wrong and inefficient. Let me make matters quite clear at the outset, I do not accept any personal responsibility for anything that Christ has done. I participated to a negligible extent in his work.

Klein was willing to accord an important part of the modeling activity to the empirical content of the model, recognizing that econometric work had hitherto been poor in terms of empirical results. To him, Christ had made important errors in the empirical content in the revision of his own model. During the late 1940s, the U.S. Department of Commerce had undertaken an important revision of the national accounts (Klein 1951, 115).[30] Klein claimed that the "most serious deficiency in Christ's work is in the data he used for 1946-47 to revise [his] model and bring it up to date. These are critical observations since they provide the basis for revisions and in samples of 20-25 annual observations can play an important statistical role. In addition, these data enter as lags in the forecasting for 1948" (115). To Klein (1951, 116), Christ's results could not be accepted as a standard to judge the accuracy in prediction of macroeconometric models, since Christ's use of the method was not efficient:

If we want to make a sound judgment about the use of econometric models for predicting some of the main economic magnitudes, we ought to reserve opinion until the most efficient use of the technique with available information has been tested. To forecast in the social sciences is difficult, and it is not likely that we shall get useful results with an inefficient application of any method. Christ's paper represents an inefficient application in many respects, and on the matter of data alone there are numerous things that he must do before he can draw any conclusions. The only really satisfactory approach open to him in the interests of efficiency is to revise all his series to agree with the new data of the Department of Commerce.

Not only would Klein not accept Christ's results and way of using the econometric approach; he would also reject Friedman's comments about Christ's paper. In his comments

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[30] This would not be the last time that the data revisions of the U.S. Department of Commerce would oblige Klein to undertake major revisions of his own models. This was also the case of Klein and Goldberger's (1955) model. This kind of data revisions were not those of the routine type made by these institutions to get more accurate figures; these were revisions that changed the basic concepts to be measured and the definitions of national accounting identities.

on Christ's revision, Friedman (1951, 107) claimed that "[t]he fact that the results suggest that Klein's experiment was unsuccessful is in some ways less important than the example they set the rest of us to go and do likewise. After all, most experiments are destined to be unsuccessful; the tragic thing is that in economics we so seldom find out that they are." Friedman was suggesting that after Christ's results, the whole econometric program *à la* Klein should be abandoned. But, this claim was unacceptable for Klein, since "Christ ha[d] not shown that econometric models break down as forecasting devices" (Klein 1951, 117) and, to Klein, it was only Christ's revision that had proved a poor application of the econometric method.

Friedman had accused the macroeconometric models of being able of fitting only the data from which they had been derived. To him this was "a test primarily of the skill and patience of the analyst; it [was] not a test of the validity of the equations for any broader body of data." He continued saying that "such a test [of the validity of the equations] is provided solely by the consistency of the equations with data not used in their derivation, such as data for periods subsequent to the period analyzed" (Friedman 1951, 108). But Klein (1951, 117), again, claimed that even if this might be the case of Christ's models, it was not the case of his own models:

> The only things for which I [Klein] assume any responsibility are the construction of the prewar model and the forecasts, from it, for 1946 and 1947. My extrapolation to 1946 (*Econometrica*, April 1947, 134) estimated net national product in 1934 prices to be $121.6 billion. Christ's figure for the observed value is $115.2 billion. In terms of the customary accuracy involved in economic forecasts, this is not a bad correspondence. It is certainly in the right direction for the postwar situation. My forecast for fiscal 1947 (*ibid.,* 133) was $104.5 billion. Christ's figure for calendar 1947 is $103.3 billion, showing that my fiscal year forecast of real output was undoubtedly near the observed value. Since both my forecasts were made before the events occurred they had to use estimates of the relevant predetermined variables. Some of the estimates were not correct, but that, of course, is the case in any realistic forecasting situation.

In any case, the introduction of naïve models was the only way of assuring strong results of prediction for Friedman. He described these naïve models not as "techniques for actually making predictions" or "competing theories of short-time change." To him, the "function [of these models was] quite different. It [was] to provide a standard of comparison, to set the zero point, as it were, on the yardstick of comparison" (Friedman 1951, 109). If the "appropriate test of the validity of a hypothesis is the adequacy with which it predicts data not used in

deriving it [...] how shall we assess the adequacy of prediction? Obviously we need not require perfect prediction; so the question is when are the errors sufficiently small to regard the predictions as unsuccessful?" The purpose of the naïve models is to provide this "standard of comparison" without which the researcher would not know "how small is small" when comparing the errors.

In the end, Friedman's criticisms of large-scale macroeconometric modeling pointed out to an important methodological problem. To him, however high the degree of complexity the econometrician could accomplish in her model in terms of economic theory; however large the number of variables and relationships; however sophisticated the mathematical forms of the equations and of the methods of estimation, no econometric model would be able to get rid of the arbitrariness of this sort of complex approaches to economics. Since there was – and probably there would never be – any theory able of representing reality in an accurate way, the most important efforts in this direction would look pretty skinny compared to the real world. In Friedman's (1951, 112) words:

> we know so little about the dynamic mechanisms at work that there is enormous arbitrariness in any [economic] system set down. Limitations of resources – mental, computational and statistical – enforce a model [of simultaneous equations] that, although complicated for our capacities, is yet enormously simple relative to the present state of understanding of the world we seek to explain.

To Friedman, the only way of getting rid of this arbitrariness would be by changing this complex empirical approach of Cowles (inspired by Walras), and by embracing an empirical approach based in his own Marshallian view. This Marshallian methodology would consist on the illumination of a particular part of the economy through careful observation of it, allowing the economist for the construction of an "engine" or model system. The study of this model system, constructed from a particular part of the economy, could, little by little, reveal some of the most fundamental mechanisms of the whole economy.

II.   The Marshallian and the Walrasian approaches to modeling

Friedman (1949; 1955) makes an enriching distinction between Marshallian and Walrasian methodology, which goes further than the standard distinction between partial and general equilibrium. To illustrate his point, Friedman uses a metaphor of engines and photography. The metaphor is unfortunate in its photographic sense, but it is rather enlightening in its engineering

sense, although, again, the word "engine" might seem too "mechanistic." These Marshallian and Walrasian views, however, are specific to the US economists of the 1940s and 1950s. In fact, the terms "Marshallian" and "Walrasian" can be better described as "labels," rather than as actual direct interpretations of the works of Marshall and Walras. The labels Marshallianism and Walrasianism have to be situated historically and geographically. In a way, one could make a parallel between the situation of US-Keynesianism, Walrasianism and Marshallianism. Albert O. Hirschman (1988) explains that US American economists received, interpreted and then created a particular kind of Keynesianism adapted to the US context and to the US necessities, which was later re-exported to the world. I want to present both Walrasianism and Marshallianism as following a similar path of reception, reinterpretation, reconstruction (and eventually re-exportation) of a particular interpretation of two methodological approaches. These reinterpretations would produce almost completely new approaches, making "Walrasianism" and "Marshallianism" appear, again, just as "labels." In fact, there are good reasons to think that rather than following a well-defined tradition these interpretations allowed US economists to free themselves from a strict European heritage.[31]

Friedman's unfortunate metaphor of the "photographic description of reality"

Friedman's (1949) metaphor of the "photographic description of reality" is not very appropriate at least for two reasons:

(1) If a theory is completely general, it cannot be photographically exact (Hoover 1988, 276). A "photographic description of reality" means that the description of the theory must be completely exact in the sense that the theory should be able to "capture" reality in an instant, characterizing every single detail of the "target system," i.e. of economic reality. This complete exactness in description would hinder the theory from being general. Indeed, a general theory is expected to describe the main tendencies or laws, hence it cannot fully account for exactness. At the same time, Friedman's metaphor does not really match with Walras's ideal purpose. The photographic description seems too "empirical" and it establishes a relation of direct representation between the model and reality. Walras's concept of pure theory is not empirical, but ideal, and his purpose is not positive, but clearly normative.[32] Furthermore, as Hoover (1988, 276) puts it "[g]enerality permits numerous possibilities; a photograph presents just one of them."

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[31] There was, of course, another tradition that can be labeled as US-American: American Institutionalism.
[32] I will come back to a more detailed discussion of Walras's project. See also Lallement (2000) for a concise description Walras's methodology and theory.

(2) I will argue that the Walrasian approach eventually creates a model system or an engine to "produce knowledge." Even if the construction of this model system, its purpose, and its use might be completely different from the model system that Friedman would like to construct on Marshallian bases, this does not mean that a system, constructed on Walrasian grounds, would not yield a "practical form of reasoning, […] of enquiry, into both […] ideas and the world" (Morgan 2012, 38). I will come back to this point.

Engines versus Cameras: Friedman's view of the divide between Marshallian and Walrasian methodologies[33]

Although the separation between Walras and Marshall has been mostly understood as a divide between general and partial equilibrium, both approaches have often been considered complementary. Prominent economists such as John R. Hicks or George J. Stigler adopted this "complementary view" between the two approaches (DeVroey, 2009b). Complementarity would consist on the assignment of "the study of isolated parts of the economy to the Marshallian approach and [on the assignment of] the task of piecing these partial results together to the Walrasian approach" (711). Yet, Friedman provides a more comprehensive explanation on the differences between Walras and Marshall, which has nothing to do with this widespread opposition, and which "separates [Friedman himself] from the new classicals" (Hoover 1988, 219). According to Friedman (1949), both Walras and Marshall viewed economic phenomena as being very complex and depending on "everything else." Both authors, then, would think in terms of general equilibrium.[34]

> The distinction commonly drawn between Marshall and Walras is that Marshall dealt with 'partial equilibrium,' Walras with 'general equilibrium.' This distinction is, I [Friedman] believe, false and unimportant. Marshall and Walras alike dealt with general equilibrium; partial equilibrium analysis as usually conceived is but a special kind of general equilibrium analysis – unless, indeed, partial equilibrium analysis is taken to mean erroneous general equilibrium analysis (Friedman 1949, 490).

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[33] Note that the sense of the metaphor of "engines and cameras" is very different from the sense used in Mackenzie's (2008) book, where "engines" are supposed to have a performative effect on reality. The sense of "engines" in this paper is that of "model systems" provided in the introduction, not that of theories performing reality.

[34] DeVroey (2004; 2009a) argues against the complementary vision. For him, as for Friedman, the approaches of Walras and Marshall are incompatible. And so, Friedman would prefer the Marshallian approach because it would not only yield a simpler model, but it would also be more "'useful' than its rival for dealing with practical problems" (Yeager 1960, 54).

Friedman also quotes Marshall's 1908 letter to John Bates Clark, in which the English author explains that "[his] whole life has been and will be given to presenting in realistic form as much as [he] can of [his] Note XXI" (Marshall 1956, 417). As noted by Friedman (1949, 490), Note XXI "presents a system of equations of general equilibrium." In this note, Marshall "take[s] a bird's-eye view of the problem of joint demand, composite demand, joint supply and composite supply when they all arise together, with the object of making sure that [his] abstract theory has just as many equations as it has unknowns, neither more nor less" (Marshall 1890 [1895],808). He ends his note saying that: "however complex the problem may become, we can see that it is theoretically determinate, because the number of unknowns is always exactly equal to the number of equations which we obtain" (809). According to Friedman, this shows that Marshall's general understanding of the economy is a complex one where everything depends on everything else: basically, a general equilibrium framework.

"The important distinction between the conceptions of economic theory implicit in Marshall and Walras" according to Friedman, "lies in the purpose for which the theory is constructed and used. To Marshall […] economic theory is 'an engine for the discovery of concrete truth.' The 'economic organon' introduces 'systematic and organized methods of reasoning'" (Friedman 1949, 490).[35]

In his review of William Jaffé's (1954) translation of Walras's *Elements of pure economics*, Friedman (1955) characterized Walras's problem as one "[…] of form, not of [empirical] content," and as one "of displaying an idealized picture of the economic system, not [as one] of constructing an *engine* for analyzing concrete problems" (Friedman 1955, 904, my emphasis).[36] Friedman always doubted that Walras could have been able to solve what Hoover (1988) calls Cournot's problem and thought that "there is a fundamental, if subtle, difference between the task Cournot outlined and the task Walras accomplished" (*ibid.*).

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[35] As noted in the introduction, Friedman and Keynes would find themselves in a closer position in this case. Keynes would understand and assess economic models depending on their usefulness as an *instrument of thought*. More important than their functions as representations of reality, models should be sees as ways to inquire and to act on reality. Furthermore, according to Keynes, it would be dangerous to think that an instrument like an econometric model might be turned into something rigid and general, since, in this case, the usefulness of the model as an instrument would be lost. Keynes, critically referring to Tinbergen's macroeconometric modeling illustrates this idea in the following way: "In [the] natural sciences the object of experiment is to fill in the actual values of the various quantities and factors in an equation or a formula; and the work when done is once and for all. In economics that is not the case, and to convert a model into a quantitative formula is to destroy its usefulness as an instrument of thought. Tinbergen endavours to work out the variable quantities in a particular case, or perhaps in the average of several particular cases, and he then suggests that the quantitative formula so obtained has general validity. Yet, in fact, by filling in figures, which one can be quite sure will not apply next time, so far from increasing the value of his instrument, he has destroyed it" (Keynes CW XIV, 300, quoted by Lawson 1985a, 129)

[36] See also Hoover (1988).

Furthermore, Friedman thought that "failure to recognize the difference seems to [him] a primary source of methodological confusion in economics" (*ibid.*). To understand Friedman's claim about the difference between Augustin Cournot and Léon Walras, it is necessary to present *Cournot's problem* in an explicit way (see also Hoover 1988, 218-220). Cournot (1838) describes the following methodological problem:

> So far we have studied how, for each commodity by itself, the law of demand in connection with the conditions of production of that commodity, determines the price of it and regulates the incomes of its producers. We considered as given and invariable the prices of other commodities and incomes of other producers; *but in reality the economic system is a whole of which all the parts are connected and react on each other* […] *It seems, therefore, as if, for a complete and rigorous solution of the problems relative to some parts of the entire system, it were indispensable to take the entire system into consideration.* But this would surpass the powers of mathematical analysis and of our practical methods of calculation, even if the values of all the constants could be assign numerically (Cournot, quoted by Friedman 1955, 903-904)

To Friedman, the primary source of methodological confusion in economics committed by Walras and Walrasians is that the economist must know the entire economic system in order to be able to study any particular phenomenon. No economic phenomenon, in the Walrasian view, could possibly be studied in a separate and independent way.[37]

One possible solution to Cournot's problem, more than a century after its statement, could be that the development of a more sophisticated mathematical analytical approach and better practical methods of calculation would allow for its overthrowing. This way of looking at the problem, however, would overlook the "fundamental methodological confusion" that Friedman claimed. Klein, for instance would reject this way of solving the problem. He was never too optimistic about the benefits that the evolution of the mathematical power and of the calculating tools would bring about. Instead, Klein was convinced of the fact that the economists should seek to analyze the economy taking into account all of its complexity.

> 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 […] I would expect marginal improvements of five

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[37] However, this is not the methodological approach adopted by Cournot. The object of chapters XI and XII of Cournot's *Recherches* is to show to what extent one can elude this difficulty and provide an approximate account of the system that would allow for a useful analysis of the most general questions in economics through the use of mathematics (Cournot 1838,146-147).

or ten per cent through the use of more powerful methods of statistical inference […] *The adoption of more powerful methods of mathematical statistics is no panacea* (Klein, 1960, p 867, my emphasis).

Great faith was placed on the ability of sophisticated statistical methods, particularly those that involved advanced mathematics, to make significant increments to the power of econometric analysis. I [Klein], personally, place more faith on 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)[38]

Not many economists thought that Cournot's problem could be solved by means of a higher sophistication in their techniques.

But there were also other criticisms about Friedman's divide between Walras and Marshall. In a translator's note, Jaffé criticized Friedman's divide, because Friedman would have not focused on the really important difference between the two authors. To Jaffé, "[a] more valid and important distinction between [Walras and Marshall] resides in the fact that [Walras] always took great care not to confuse pure theory with applied theory, while [Marshall] gloried in fusing the two" (Jaffé in Walras 1954, 542). Friedman (1955, 905) responded to this criticism by casting doubt on the "superiority" of Walras's pure theory, ignoring this important issue of the "fusion" of pure and applied theory. To him, Jaffé was speaking "as a true Walrasian in methodology," who

first constructs a pure theory, somehow on purely formal considerations without introducing any empirical content; […] then turns to the 'real' world, fills in empty boxes, assigns numerical values to constants and neglects 'second-order' effects at this stage. As I have argued extensively elsewhere [particularly in Friedman (1946; 1953)] this seems to me a basically false view. Without denying the importance of what Jaffé and Walras call 'pure theory' […] I deny that it is the whole of 'pure theory.' (Friedman 1955, 905)

Noteworthy, is the eagerness of both Jaffé and Friedman to stick to the concept of "pure theory." Apart from Jaffé's comment on Marshall "fusing" pure theory and "applied science," there is not much reflection on the effect that the empirical turn and the new econometric tools

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[38] This was also the view of Ragnar Frisch, one of the founding fathers of econometrics: "I do not claim that the technique developed in the present paper will, like a stone of the wise, solve all the problems of testing 'significance' with which the economic statistician is confronted. No statistical technique, however refined, will ever be able to do such a thing. The ultimate test of significance must consist in a network of conclusions and cross checks where theoretical economic considerations, intimate and realistic knowledge of the data and a refined statistical technique concur" (Frisch 1934, 129, quoted in Boumans 2013, 5).

would have had on the traditional separation between theory and application. And yet, Jaffé's claim about the "fusion" of pure and applied theory would hold not only for Marshall, but also for both Klein's and Friedman's tooled approach. Economists in the mid-twentieth century, assisted not only to an "empirical turn" in the discipline, but, most importantly, they also assisted to a reconfiguration of the (hierarchical) relationship between theory, application, and policy, or between what Walras would call "science, art and morals" (Walras, [1874] 1954).

A particular interpretation of Walras's work

One of the ways general equilibrium theory entered the United States in the 1930s was through Harvard University, more exactly through the "Pareto Circle" (Cot, 2011). The version of general equilibrium that made his way through Harvard was not Walras's, though. It was actually Vilfredo Pareto's version. Pareto, who was supposed to represent Walras's intellectual inheritance in Lausanne, thought, like many others, that the *Elements of pure economics* was Walras's sole important contribution. His *Trattato di sociologia generale* published in 1916, gained considerable importance in Harvard through a seminar organized by Lawrence J. Henderson, "biochemist and polymath of great note" (Merton 1985, quoted in Cot 2011, 132). According to Cot, the importance of the "Pareto Circle" is that it transformed general equilibrium into a "boundary object." Boundary "objects […] are plastic enough to be adaptable across multiple viewpoints, yet maintain continuity of identity" (Star 1989, 37, quoted by Cot 2011, 150). As a "boundary object," general equilibrium travelled from one discipline to another providing the bases for the creation of an "epistemological credo," praying that "[w]ithout a conceptual scheme, thinking seems to be impossible" (Henderson 1970, 86, quoted in Cot 2011, 145). The conceptual scheme for any science would be general equilibrium as Pareto and Henderson understood it.

Not only had Walras's work been interpreted partially, but also the reception of general equilibrium theory had happened through Pareto's filter. And yet, these are not, I think, the most relevant facts impeding the macroeconometricians of the 1940s to fulfill Walras's program. The most important event that happened during the 1930s and 1940s impeding the macroeconometricians to fulfill Walras's program is that they were armed with a tool with which they could not possibly view either economics or the economy as Walras did.[39] The fact

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[39] Of course this is not the only aspect hindering the macroeconometricians to share Walras's views. Their "scientific cultures" were completely different as well, let it only be because of the differences in the political problems they were facing, or because of the social position that economists occupied in the late XIX century France and in the mid XX century United States.

that economics became a "tooled-based" discipline (Morgan, 2003) determined a particular way in which economists could understand Walrasian economics and the economy. The tool would not make it possible for macroeconometricians to remain faithful to the original project of Walras, since the econometrician would not be able to separate pure, applied, and social economics. The three spheres were embedded in the tool.

The purpose of structural macroeconometrics was, again, to produce a system of simultaneous equations within a general equilibrium framework. However, in doing so through the use of a tool like structural macroeconometrics, econometricians were necessarily reconfiguring the relationship between theory, application and policy. This relationship certainly understood as a whole by Walras, but identifiable in a separate way, would fuse inside the econometric model. The empirical turn that occurred in twentieth century economics generated a change in the hierarchical relation between these three spheres.

Walras's normative project was very different from Klein's pragmatic and political project.[40] Economic theory or econometric models, in Klein's view, do not represent the ideal towards which society should tend, as does theory in Walras's project; they rather represent an instrument to act and to intervene the economy.[41] Throughout the second half of the twentieth century, the macroeconometric tool became not only the model system to understand and to act on the economy through a "scientific" approach; it was also a necessary rhetorical element that economists could not dispense of, in order to be credible both in the academic and political arenas.

Klein's large-scale macroeconometric model system

When it comes to assess the effect of Walras's work on twentieth century economics most attention is directed to Walras's legacy as a source of inspiration for models of the Arrow-Debreu-McKenzie-type.[42] This, of course, is more than understandable since it is in this field where the Walrasian approach is more evident and visible. Yet, I am interested in another sub-discipline where the Walrasian approach exerted an important influence that might seem less evident: macroeconometric modeling. Following Renault (2016) I will argue that Walras's

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[40] I am associating the term policy in Klein's thinking with what would be the sphere of morals in Walras's theory although this might be quite problematic. Again, I do not seek to make a completely accurate description of Walras's project.

[41] Note that for Walras economic theory is also an instrument, not to act on the economy, but allowing for the understanding of the economic world. It is also a normative reference towards which the economy should converge.

[42] See for instance Weibtraub (2002).

influence in macroeconometrics was that of establishing an anchor and a reference framework from which to build models and understand the economy: the general equilibrium framework.[43] Every macroeconometrician, and especially Klein, would be obliged to refer to this "pillar" as a way of understanding the economy providing a solid framework from which to construct and structure the macroeconometric model. Therefore, if the macroeconomists wanted to build macroeconometric models *à la* Klein, she would have to conceive the economic system "as describable by a set of simultaneous equations expressing all the interrelationships among the measurable economic magnitudes which guide economic behavior" (Klein 1950, 2).

This kind of models would yield systems that, in the absence of external shocks, would, essentially be monotonic, stable and linear. The econometric tool would introduce a dynamic component into the general equilibrium system, but this dynamic component would remain stable. This can be seen in a clearer way in Irma and Frank Adelman (1959) examination of the Klein-Goldberger (1955) model by means of the IBM 650 high-speed computer. The Adelman's examination of the dynamic properties of the model consisted in extrapolating the exogenous variables from the model and in solving the equations for a hundred years. The first stage of this examination – relevant for my purpose here – consisted in asking "what sort of time path will these equations generate in the absence of additional external constraints or shocks?" (Adelman and Adelman 1959, 601). As figure 1 shows, the "first machine data decided the issue unequivocally. After a brief 'settling-down' period, the system is quite monotonic and essentially linear. There is no hint whatever of any internally generated business cycle, and, indeed, even in the first few years, the shock of start-up is not sufficient to induce more than a single turning point in any variable" (602). In a nutshell, in the absence of external shocks, the system represents a system of general equilibrium.

Figure 1: Klein-Goldberger Time Paths (without any shocks).

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[43] Renault makes a similar claim in the case of macroeconometricians of disequilibrium, especially in the case of Edmond Malinvaud.
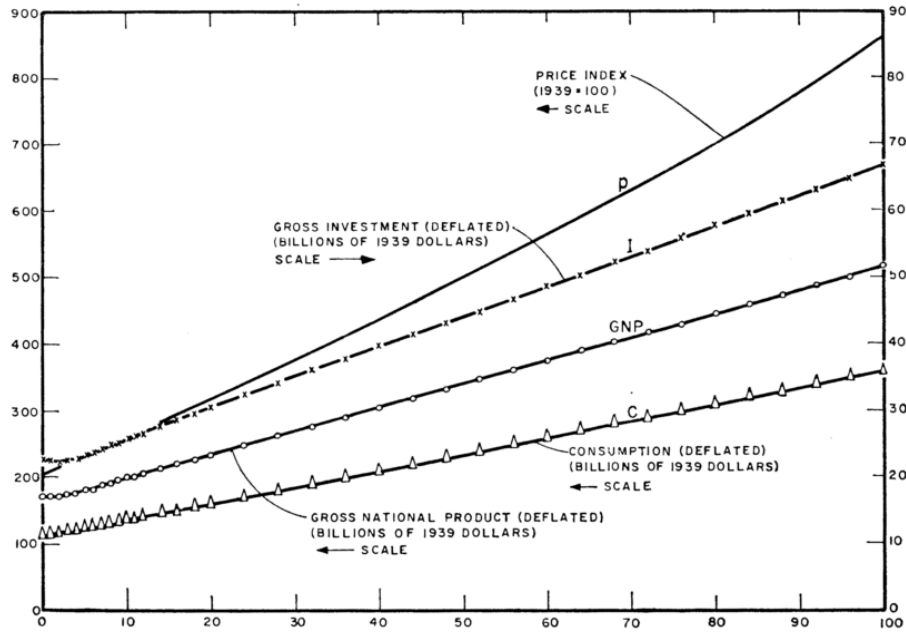
Source: Adelman and Adelman (1959, 601).

The choice of treating the economy in general equilibrium terms, then, was not only a matter of a preference in economic theory, but also (and perhaps more importantly) a matter of technical adequacy. Klein (1950, 11) explains that:

> Formerly, econometricians singled out an isolated equation of the economic system and attempted to estimate the structural parameters by the methods of least squares or some other simple method whose statistical properties were not usually satisfactory. When the earlier statisticians fitted their equations to the data by the method of least squares they seldom knew in which direction they should minimize the sum of squares; i.e., which should be the 'dependent' and the 'independent' variables. They were aware of the problem of identification, but they failed to solve it adequately. Now many of these difficulties are eliminated. If we specify both the economic and statistical properties of the model and treat the set of equations as a unit, instead of treating each equation in isolation from the rest of the system, we are not faced with the problems that formerly were so troublesome.

Also, Klein (1957, 1-2) would say in a more explicit way that:

> Following the great ideas of Walras, we view the economic system as capable of being described by a system of simultaneous mathematical equations. In actual practice we shall, of course, drop the enormously refined detail of the Walrasian system but retain the main

as picturing the economy as model written in the form of a system of mathematical equations. Our problem then will be to estimate the parameters of these equations.

Note that Klein's "Walrasian" project is of a special kind. It mainly consists on considering the economy as a whole, discarding the individual analysis of equations. The idea of a mathematical representation is, of course, very important too.

Even Friedman (1955, 908) would recognize that Walras's method would provide "a framework to organize ideas" in a logical way, allowing for an understanding (even if in a particular way) of the economy.

> Walras has done more than perhaps any other economist to give us a framework for organizing our ideas, a way of looking at the economic system and describing it that facilitates the avoidance of mistakes in logic.

But the organization of ideas is not something that would happen just inside the mind of the researcher or econometrician. There must be a materialistic component with an explicit procedure that the econometrician should follow. An explicit example of this kind of procedures is what Bjerkholt and Dupont (2010, 34) call Frisch's "five types of mental activity." The important term is "activities," since it is the description of a procedure or a practice, that the econometrician should follow in order to be able first to understand, then, to act on the world. It is a series of questions that do not "naturally" come to the mind of the econometrician, but that make part of a kind of protocol that the econometrician has to follow to obtain her results.

In the case of Klein's modeling, this "mental activity" or practice would become even more explicit and materialized. Klein was conscious about the fact that macroeconometric modeling was something that had to be done in a team with a particular scheme that would make a specific division of labor. Specific procedures would have to be respected for the whole "Kleinian model system" to work.

> The whole group was broken into subgroups. There was one team working on the treatment of simultaneous equation problems. Another group worked on putting the model together, some from the point of view of economic theory and some from the point of view of data availability. Another group worked on computing. We carved up the problem. We had very heated and intensive seminars, and everybody was extremely enthusiastic, but it was very well orchestrated (Klein 1987, 413).

Figure 2: Cowles Commission's seminar around 1946.

Herman Rubin, Gershon Cooper, Lawrence Klein, Jacob Marschak, Jack Hartog, and Tjalling Koopmans. Source:

"That indeed was the way we started out in the Cowles Commission, but it then became a routinized team effort. Somebody had to be responsible for the data files, someone had to be responsible for system design, and someone had to be responsible for forecasting and applications" (415). Throughout his career at Cowles, Michigan, Pennsylvania, and during his participation in the construction of the Brookings model or of Project LINK, Klein conceived econometric modeling as a team effort that established clear tasks for each of the team members, where every member had to follow a specific procedure.

Figure 3: Cowles Commission Computational Laboratory in Chicago.

Source: Cowles Commission *Economic Theory and Measurement. A Twenty Year Report, 1932-1952* (98).

This practice also needed of a specific location (that one could almost call a laboratory), of human computers first, then of specific material conditions and machines, like mechanical or electronic calculators, and later of electronic computers. It also needed of a specific method and of a particular methodology. The setting of this particular kind of model system would produce an "engine" for understanding phenomena, but also for the development of new methods and ideas.

> An interesting development in connection with the Brookings model project was that it functioned as a team effort in which each person on the team had responsibility for a certain piece of the model. Although we did not put together the definitive model we wanted, I think we learned a tremendous amount about model building from that venture. In particular, we developed best practice methods for parts of the economy. The work on the investment function was Dale Jorgenson's and Bob Eisner's contribution, best practice for dealing with housing was Sherman Maisel's contribution on the relation between starts and completions […] We learned from the Brookings experience how to operate models, how to maintain them, and how to test them (Klein 1987, 431-432).

Table 1: Frisch's five types of mental activities.

1. The descriptive procedure.
One set of questions the scientist has to answer is, What happened? What is the situation? What course did the events follow? In order to answer these questions he has to engage in descriptive, historical, and experimental work. In some sciences, such as economics, direct experiment is more or less impossible and the scientist must rely largely on the descriptive and historical answers to the questions here considered.

2. The understanding procedure.
Another set of questions that the scientist has to answer is, Why did it happen? Why did this situation exist? Why did the events follow the course they did? The answers to these questions constitute the rational part of the investigation. By the power of his mind the scientist tries to discern or impose some reasonable order onto what happened and the things he observed.

3. The prediction procedure.
The questions here are, What will happen? What will the course of events be in the future? For these sorts of questions to have a meaning, the phenomenon must be such that it cannot easily be controlled by man. If it can be fairly completely controlled, no forecasting problem really exists.

4. The human purpose decision.
Here the questions are, What do we wish shall happen? What do we wish the situation to be? The three first sorts of questions were exclusively of an intellectual character. On the contrary the sorts of questions here considered are ethical or moral. They cannot be answered unless we adopt a standard of social values. If the answer to such a question shall be socially significant, it must, of course, in some way or another weigh the opinions of the several individuals who make up the society. It is not a question of what you or I personally think in this matter, but of what is a socially fair position.

5. Social engineering.
The question here is, What can we do to produce certain outcomes or certain situations? This last sort of question is the most complicated we can ask. In order to give a significant answer to this sort of question, we have to build on an analysis of all the first four sorts of questions.

Source: Bjerkholt and Dupont (2010, 35).

Another important feature in Klein's model system is his preference for large and complex systems, opposed to systems that would be simple and parsimonous. Friedman's system would seek for simplicity because it would be considered as "a methodological virtue [consisting on the idea that] the most significant theories explain 'much by little'" (Caldwell in Caldwell 1984, 228). "Instead of the rule of parsimony, [Klein] prefer[s] the following rule: the largest possible system that can be managed and that can explain the main economic magnitudes as well as the parsimonious system is the better system to develop and use" (Klein 1992, xl- xli).

Friedman's Model System

Christ (1949, 2-3) provides a general view of an "idealized procedure" to "find a system of structural equations" that was suggested to him by Friedman around 1948. The goal, of course, was to find a structure, which not only explains past observation, but also allows for making accurate predictions. "Of course there is an infinity of structures which explain any given set of observation" and so the "problem is to […] choose the 'best'" structure. The "best" structure is the one that provides "the most accurate predictions of the future, [but] we cannot know which one this is until afterwards. Therefore if we are to choose now we must do so on the basis of intermediate available criteria […]:

1) generality
2) simplicity
3) correspondence with our theoretical ideas of what to expect (but if we have a poor theory, this criterion will mislead us)
4) accuracy of explanation of past observations (though we must be careful with this criterion, because it is necessary but not sufficient…remember that it is always possible to fit an nth degree polynomial *exactly* to a set of $n + 1$ plain points, and that this very seldom makes for good prediction).

In more concrete terms, however, Friedman's construction of a model system would also involve a "laboratory" where several people would work together in the analysis of data, information and knowledge. This laboratory would take the form of the NBER. Contrary to the Cowles Commission's projects, which were conceived rather as short-term projects, the NBER would host long-term projects that would endure for decades. This was; for instance, the case of Friedman and Kuznets's (1954) joint project *Income from Independent Professional Practice*. In fact, Kuznets had begun this investigation in 1933 and Friedman took it up in 1937,

producing a "definitive" publication only in 1954.[44] Work at the NBER was highly influenced by Mitchell's methodology of "descriptive analysis." This means that empirical work played a major role in these investigations, which in no way would be deprived of theory as Tjalling Koopmans suggested in 1947 in his famous "Measurement without Theory" review. To Friedman (1952, 237), there was no dichotomy or neat separation between the "empirical scientist and the theorist," these differences were actually just a matter of degree and of emphasis. To him, the "ultimate goal of a science in any field is [to provide] a theory – an integrated 'explanation' of observed phenomena that can be used to make valid predictions about phenomena not yet observed." This was the task in which Mitchell and all the researchers in the NBER were involved, which goes well in line with the five 1920 original precepts of the Bureau, summarized by Frabricant (1989, 2-3):

1) [The NBER's] research should concentrate on determining facts, and the conections among facts, that are important in dealing with major problems of economic policy.

2) The knowledge sought should be quantitative in character, whenever possible.

3) The research should be in accordance with scientific principles.

4) The research should be done, and the findings made known, under auspices of and with safeguards that would assure the public of their impartiality.

5) To this end in particular, the Bureau should carefully abstain from making recommendations on policy.

Apart from the five precepts, the NBER analyses also included different kinds of "charts" that would be used as tools of analysis to understand the cycle (see Figure II). Work at the NBER would yield important volumes with a huge amount of descriptions of data. These descriptions were accompanied by these kinds of charts that would help the researcher and the reader to get a better picture of the movements of particular variables in the different phases of the cycle.

Yet, by 1951 Friedman was rather willing to express a conciliatory position between his NBER approach and the Cowles's econometric approach. In his concluding remarks of his comments of Christ's paper Friedman (1951, 114) attempted to bring theses approaches to a closer position. His conclusions are worth quoting at length.

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[44] This was also the case of Mitchell's (1913; 1927) and Mitchell and Burns's (1946) work on the business cycle, as well as that of Friedman and Schwartz's (1963; 1965; 1970) started in the 1920s and only completed in 1982 (Fabricant 1989, 27).

These remarks obviously have a rather direct bearing on the desultory skirmishing between what have loosely been designated the National Bureau and the Cowles Commission techniques of investigating business cycles. As in so many cases, the difference between the two approaches seems to me much greater in abstract discussions of method than it is likely to prove in actual work. The National Bureau has been laying primary emphasis on seeking to reduce the complexity of phenomena in order to lay a foundation for a theory of change; the Cowles Commission on constructing the theory of change. As the National Bureau succeeds in finding some order, some system, in the separate parts it has isolated for study its investigations will increasingly have to be concerned with combining the parts – putting together the structural equations. As the Cowles Commission finds that its general models for the economy as a whole are unsuccessful, its investigators will increasingly become concerned with studying the individual structural equations, with trying to find some order and system in component parts of the economy. Thus, I predict the actual work of the two groups of investigators will become more and more alike (Friedman 1951, 114).[45]

Figure 4: Errors in the Prediction of Consumption and Income from Different Consumption Functions.
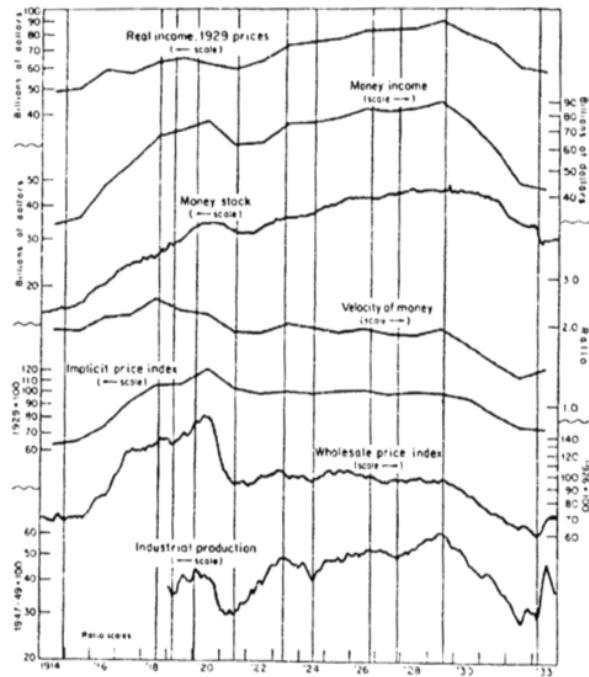
Source: Friedman and Schwartz (1965, 2).

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[45] Many years later, Klein (1991, 112) would describe this tension in a somewhat different way: "As a visiting staff member of the National Bureau during 1948-49, I could sense the tension in the dispute over methodology. It was not purely methodological, however. A central issue was that we members of the Cowles Commission were seeking an objective that would permit state intervention and guidance for economic policy, and this approach was eschewed by […] the National Bureau."

III.   The Klein-Friedman debate: more than illusions

Despite Friedman's (1951) conciliatory tone, the debate between these two approaches to empirical economics and macroeconomic modeling was prolonged throughout the 1950s. By 1958, eleven years had passed since Klein had left the Cowles Commission; he had worked at the NBER, traveled in Europe with his wife Sonia, and, together with Arthur S. Goldberger, he had developed at the University of Michigan, in Ann Arbor, what would come to be the very influential "Klein-Goldberger model."[46] In 1954, McCarthyism imposed serious obstacles on Klein. Ellen W. Schrecker (1986, 253) refers to this episode at the University of Michigan as "perhaps the most egregiously political denial of tenure." Klein's academic quality was unquestionable, and he was willing to cooperate with the House of Un-American Activities Committee (HUAC), which he in fact did on April 30, 1954. [47] Yet, none of these reasons, nor even the facts that "Michigan's administration seemed satisfied with his performance" or that he was "something of a superstar" (253-54) in the Economics Department, hindered the University to deny his tenure position. Considering this "a serious deficiency of academic freedom" (*ibid.*), Klein left Ann Arbor and went to Oxford in 1955 where "econometrics had hardly existed" (Klein and Mariano 1987, 422).

By the end of 1958, though, when the controversy with Friedman broke out, Klein had already been appointed professor at the University of Pennsylvania (Bjerkholt, 2014). By contrast, under Koopmans's directorship (1947-1955), Cowles had abandoned the macroeconometric modeling program after Klein's departure and after Christ's unsuccessful attempts to revive the project. [48] Since 1955, James Tobin had assumed the research directorship of the Commission, which had, once more, changed location, leaving Chicago for Yale University.

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[46] Also in Ann Arbor, Klein became involved with George Katona's work at the Michigan Survey Research Center, where his main interest was "to use the survey technique in econometric analysis." Klein had met Katona in 1946 at the Cowles Commission. Katona's research proved very influential to Klein, both in his conception of household behavior and hence in the specification of the consumption function, and later in his conception of the formation of expectations and on the process of decision making of investors. During the 1970s, Klein would defend his large-scale macroeconometric modeling approach from Lucas's (1976) attack, on the basis of survey research. For a more detailed description of this defense see Klein and Mariano (1987) and Goutsmedt et al. (2015).

[47] Klein testified in a Detroit hearing that he had been a member of the Communist Party for six months in 1946. After many discussions inside the University of Michigan, Klein was denied his tenure position and preferred to leave the United States.

[48] According to Bjerkholt (2014, 782) there were some conversations between Koopmans and Klein during 1950 to revive the macroeconometric project at Cowles. However, in the end, Klein "turned down the idea of coming back to the [Commission] as he already was involved with two other institutions." Cowles had embarked in another ambitious research program on linear programing and game theory.

Friedman, on the other hand, had consolidated his influential position at the Department of Economics, and together with Stigler, was the most important representative of the Chicago School of economics and of the nascent Monetarism. He had also published his (1953) *Essays in Positive Economics* as well as his (1957) *Theory of the Consumption Function,* under the NBER General Series. Nevertheless, his confrontation with the representatives of the 1940s Cowles's approach continued, especially with Klein, since his 1957 "study of the consumption function […] was intended to explode the theoretical basis of Klein's models" (Epstein 1987, 135-136). Instead of attacking the whole Cowles Commission's approach, Friedman changed his strategy directing the attention of his attack to a particular, though important, element of macroeconometrics: household behavior and the consumption function. While the 1940s criticisms had focused on the general methodological approach of structural macroeconometricians (and econometricians *tout court*), his 1950s attack aimed at one sensible part of the model.

Theories of the household behavior and especially of the consumption function were highly debated issues during the 1950s.[49] At least three theories of the spending and saving behavior co-existed: (1) the absolute income hypothesis, (2) the relative income hypothesis, and (3) the permanent income hypothesis. Even if the three theories were very different, all of them presented some common features. They all sought for generality; they all had "been used on time series, as well as on cross-section data and to derive macro- as well as micro-relationships"; they all had been originally advanced "in terms of individual behavior and then generalized to aggregate behavior, sometimes with explicit recognition of the aggregation problem" and sometimes "largely ignoring it on the apparent presumption that nonlinearities or distributional effects are relatively unimportant" (Ferber 1962, 20). All these theories or hypotheses tried to "isolate the influence of income, and occasionally of wealth, on consumer spending, holding constant the effect of other […] variables" (*ibid.*). Another common feature was that all the theories seemed to receive support from some empirical studies, but not from others. "Finally, each [theory] when first presented appear[ed] deceptively simple […] but when it [came] to implementation, proponents of the same hypothesis often disagree[d] with each other on appropriate definitions and approach" (*ibid*.).

The absolute income hypothesis was stated by John Maynard Keynes (1936, 96) in the following way: "men are disposed, as a rule and on the average, to increase their consumption

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[49] See Ferber (1962) for a survey of the main empirical research on these subjects.

as their income increases, but not by as much as the increase in their income." The empirical application of this theory generally followed one of these two forms:

(1) $S = a + bY + cZ + u$

(2) $\frac{S}{Y} = a' + b'Y + c'Z + u'$

where S represents savings, Y income, Z is a conglomeration of other variables, and u is a stochastic term. "Questions about the adequacy of the absolute income hypothesis arose because of its apparent inability to reconcile budget data on saving with observed long-run trends" (Ferber 1962, 22). In fact, various studies including Simon Kuznets (1946; 1952) and Raymond Goldsmith (1955) suggested that the saving ratio had remained practically unchanged since the 1870s (*ibid.*), casting doubt on Keynes's insight.

Dorothy Brady and Rose Friedman (1947) set forth the relative income hypothesis. This hypothesis stated that "the saving rate depends not on the level of income but on the relative position of the individual on the income scale" (Ferber 1962, 23). Its form for empirical application is as follows:

(3) $\frac{s}{y} = a + b\frac{y}{\bar{y}}$

where s and y represent individual savings and income, and $\bar{y}$ individual average income. Both Franco Modigliani (1949) and James Duesenberry (1949) provided some empirical support for this hypothesis. For instance, Duesenberry showed that "people seek to maintain at least the highest standard of living attained in the past" (Ferber 1962, 23).[50]

The permanent income hypothesis "grew out of the rising concern regarding the adequacy of current income as the most appropriate determinant of consumption expenditure" (25). In particular, the work advanced by Friedman and Kuznets (1945) provided the empirical observations, suggesting that even with a substantive variation of income, consumption would exhibit great stability. "This led to the belief that people geared their expenditures to average actual and anticipated income over a number of periods rather than only to income received in the current period" (Ferber 1962, 25). Responsible for the development of this theory were Milton Friedman, on the one hand, and Franco Modigliani together with R. E. Brumberg and Albert Ando, on the other. Friedman's formulation of the hypothesis rests on three fundamental

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[50] Duesenberry (1949) transformed equation (3) expressing it from the point of view of aggregation: $\frac{S}{Y} = a + b\frac{Y}{Y_0}$.

principles. "First, a consumer unit's measured (observed) income (y) and consumption (c) in a particular period may be segregated into 'transitory' and 'permanent' components" (26):

(5a) $y = y_p + y_t$

(5b) $c = c_p + c_t$

"Permanent income […] is the product of two factors: the wealth of the consumer unit, estimated as the discounted present value of a stream of future expected receipts, and the rate, r (or weighted average of a set of rates), at which these expected receipts are discounted" (*ibid.*). The second principle is that permanent consumption is a multiple k of permanent income.[51]

(6) $c_p = k y_p$

The third principle is that "transitory and permanent income are assumed to be uncorrelated, as are transitory and permanent consumption, and transitory consumption and transitory income:"

(7) $r_{y_t y_p} = r_{c_t c_p} = r_{y_t c_t} = 0$

where $r_{y_t y_p}$ is the correlation coefficient between y_t and y_p.

The permanent income hypothesis implies that "a consumer unit is assumed to determine its standard of living on the basis of expected returns from its resources over its lifetime." Furthermore, "these returns are expected to be constant from year to year," although actually "some fluctuation would result over time with changes in the anticipated amount of capital resources" (Ferber 1962, 27).

The debate on statistical illusions

In the February issue of the *Journal of Political Economy* of 1957 Milton Friedman, together with the young Gary S. Becker, published their paper "A Statistical Illusion in Judging Keynesian Models."[52] Their purpose was to attack Keynesian models for their inability to yield

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[51] Furthermore, k only depends on the interest rate i, on the ratio of nonhuman to total (nonhuman plus human) wealth, w, and a catchall variable, u, of which age and tastes are principal components. In other words, $k = f(i, w, u)$, but k is independent of the level of permanent income.

[52] Klein responded to the attack in 1958 in the sixth issue of the same journal. A joint "Reply" by Becker and Friedman (1958), and an individual "Supplementary comment" by Friedman (1958) followed in the same issue. There were two additional responses to the attack: one by Edwin Kuh (1958) who published "A Note on Prediction

accurate predictions of income, because of their inappropriate treatment of the consumption function. Friedman and Becker's claim was that Keynesian modelers, who generally adhered to the absolute income hypothesis, had replaced "[t]he ultimate objective of predicting income […] by the proximate objective of predicting consumption from current income and other variables" (Friedman and Becker 1957, 64). This was not a problem in itself. The problem was that in changing the objective, "[a]n unnoticed result […] ha[d] been the adoption of a criterion for judging alternative consumption functions that, however relevant for the proximate objective, can be seriously misleading for the ultimate objective [of predicting income]" (*ibid.*). This criterion to judge "alternative empirical consumption functions" consisted on evaluating "the error made on the average in estimating the consumption function from the function, albeit with some allowance for such considerations as the number of degrees of freedom used in estimating the function, the economic plausibility of the signs of the parameters, and so on" (Friedman and Becker 1957, 64). This criterion, however, entailed a problem: a relative small error made on average for estimating the consumption function was not adequate for judging whether the model would perform well in forecasting income. This is so, because "the accuracy of the estimate of income depends not only on the accuracy of the estimate of consumption *but also on the form of the consumption function* – in particular, on the fraction of consumption which it designates as 'autonomous'" (*ibid.*, my emphasis).

> Suppose that two alternative empirical functions are estimated for the same historical period and are alike with respect to the subsidiary considerations and that the standard error of estimate of consumption is 5 per cent of the mean value of consumption for one function and 10 per cent for the other. Suppose that these functions are used to estimate retrospectively income in each year of the same historical period from the known magnitude of investment in that year. Does it follow that the first function will give more accurate estimates of income than the second? Surprisingly enough, the answer is 'No'" (*ibid.*).

This is the "statistical illusion" Friedman and Becker denounce in their title. The relatively small errors made by the functions that the Keynesian modelers would prefer are not a sufficient condition to consider that the consumption function is accurate enough for predicting income (see table 2). In this case, statistical adequacy does not immediately mean that the

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model will perform better. In the Keynesian model "under consideration, a given error in predicting consumption is magnified by the multiplier process into a much larger error in predicting income" (66).

Table 2: Errors in the Prediction of Consumption and Income from Different Consumption Functions.

ERRORS IN THE PREDICTION OF CONSUMPTION AND INCOME FROM
DIFFERENT CONSUMPTION FUNCTIONS

| | RELATIVE ERROR IN PREDICTING | | | |
CONSUMPTION FUNCTION	Consumption from a Known Level of Income (1)	Income from a Known Level of Investment (2)	Income from an Estimated Level of Investment* (Consumption Known with Certainty) (3)	Income from Estimated Consumption and In-vestment* (4)
(12) $C(t) = 70.2 + .780Y(t) + u_1$	.049	.196†	.100	.242‡
(13) $C(t) = .890Y(t) + u_2$	.057	.462†	.200	.551‡
(14) $C(t) = (.879)(.4)\int_{-\infty}^{t} e^{.38(T-t)} Y(T)dT + u_3$ . . .	.040	.051	.029	.059
(15) $C(t) = 162.0 + 237.0e^{.02t} + u_4$	.066	.059	.020	.062
(16) $C(t) = C(t-1) + u_5$	.068	.061	.020	.065
(17) $C(t) = e^{.02}C(t-1) + u_6$	.064	.057	.020	.061

* The relative error in predicting investment is assumed to be .20.
† Instead of .196 and .462, these figures would be .147 and .420, respectively, if more efficient estimates of the parameters of these consumption functions had been used.
‡ Instead of .242 and .551, these figures would be .178 and .465, respectively, if more efficient estimates of the parameters of these consumption functions had been used.

Source: Becker and Friedman (1957, 67).

Friedman and Becker's claim here is about *the form of the consumption function* and about its implication on statistical grounds, hence the problem is one of economic theory. But it is also one of specification, which cannot be regarded as a matter of economic theory alone. Rather, specification has to be regarded as a matter of economic theory combined with its consistency to structural econometric modeling. It is at this point where my claim about the reconfiguration of the relationship between economic theory, application and the policy sphere becomes evident in more concrete terms.

Furthermore, Friedman and Becker would propose another kind of test to be put into practice if the model were to be used for prediction. Naïve models should be used to test the forecasting performance of any macroeconometric model (equations (16) and (17) in table 2). These models would "provide a standard for judging [the size of the] relative errors [of the consumption functions]" (68). The models proposed by Friedman and Becker were:

(8) $C(t) = C_{(t-1)} + u_5$

(9) $C(t) = e^a C_{(t-1)} + u_6$

In fact, some Keynesian models (like the Klein-Goldberger (1955) model) did not make use of this kind of naïve models that had been proposed by Friedman since the end of the 1940s (see section I). Klein thought that the naïve models presented some important deficiencies in particular cases.

Klein responded to the attack in the *Journal of Political Economy* in 1958 with a paper entitled: "The Friedman-Becker Illusion." According to Klein, "Messrs. Friedman and Becker" were "in essence" right when they suggested "that statistical calculations of the Keynesian consumption function should be judged not on the conventional goodness-of-fit criteria for the consumption function itself but on these same criteria for the derived multiplier equation. Had they simply made this observation without implying that other students had been laboring under an illusion," he continued, "one could have said that they were essentially correct in repeating what econometricians had long ago recognized and made abundantly clear. They proceed, however, as though they [had] made a new discovery" (Klein 1958, 539). According to Klein, by thinking that they had discovered something new, it was Friedman and Becker who were under an illusion. The problem evoked in Friedman and Becker's critique was an old and well-known problem for econometricians and many efforts had been undertaken to overcome it. In fact, according to Klein, Friedman and Becker had discovered nothing new.

Klein's (1958, 539) purpose was to prove the superiority of his approach, by demonstrating that: "(1) There has been no statistical illusion in the judging of Keynesian models. (2) The improved consumption function put forward by Friedman has long been in existence and used to good advantage in Keynesian models […] (3) It is an easy matter to construct an alternative consumption function that performs at least as well and possibly better on the Friedman-Becker criteria […] (4) The Friedman-Becker criteria are not adequate for judging Keynesian models. (5) There are logical deficiencies in the naive models used for this particular case and, in fact, for a wider class of problems." I will treat the first and the fourth points in a somewhat thoroughly way, since it is these points which will allow me to show how the differences in the US-Walrasian and US-Marshallian approaches look like in a concrete case. For the other three points I will just mention some important aspects in passing.

Estimation of the consumption function when investment is an 'autonomous' or exogenous variable

According to Alvin H. Hansen, "the statistical data […] tend to support the thesis that the active dynamic factor in the cycle is investment, with consumption assuming a passive, lagging role […] For the most part spontaneous expenditures […] are likely to be made on investment goods upon durable consumers' goods, but not upon other forms of consumption. […] It does not follow, however, that all investment is spontaneous. Much of it is, in fact, induced. It is however quite impossible to determine statistically what part is spontaneous and what part is induced" (Hansen 1941, 50, quoted by Haavelmo 1947, 75-76). Paul A. Samuelson would understand the behavior of investment in a similar way: "In behavior [investment] is sporadic, volatile, and capricious. Its effective determinants are almost completely independent of current statistical factors (level of income, etc.)" (75-76).

According to these statements, it was clear for Haavelmo (1947, 76), as well as for other econometricians and in particular for Keynesian econometricians like Klein, that in order to estimate the marginal propensity to consume, they "should take […] the regression of income on investment to obtain the multiplier, and from this estimate of the multiplier […] derive the marginal propensity to consume." This is the approach Klein defended from the attack by Friedman and Becker.

Klein recognized that "if investment is treated as an exogenous or autonomous variable," then "the multiplier equation would be used for prediction or analysis" and this would yield a magnified error, which will very likely "give a much poorer showing in goodness-of-fit measures" (Klein 1958, 539). Following this reasoning, Klein (1958) defined C_t as consumption of period t, I_t as investment of period t, Y_t as income of period t and u_t as a random disturbance, and wrote the simple Keynesian model in this form, with

(10) $C_t = \beta + \alpha Y_t + u_t,$

the consumption function,

(11) $Y_t = C_t + I_t,$

the definition of income;

(12) $C_t = \frac{\beta}{1-\alpha} + \frac{\alpha}{1-\alpha} I_t + \frac{u_t}{1-\alpha},$

and,

(13) $Y_t = \frac{\beta}{1-\alpha} + \frac{1}{1-\alpha} + I_t \frac{u_t}{1-\alpha},$

the multiplier equations. Equations (10) and (11) are "structural equations," one is a behavior equation and the other is an identity. Equations (12) and (13) are reduced form equations. Note that the parameters of these reduced form equations are derived from the structural equations.

According to Klein, "when Friedman and Becker show that the variance of $u_t/(1-\alpha)$ in the multiplier equation is much larger than the variance of u_t in the consumption equation […] they are not proving the general inadequacy of Keynesian models or even of the consumption function; they are simply giving a laborious demonstration of the fact, already well known, that the simple multiplier model is not suitable for more than pedagogical use in the classroom" (*ibid.*). However, Keynesian modelers were not using these very simple forms to attempt forecasting. In fact, the structural macroeconometrics approach followed both a Keynesian framework in terms of theory, and a Walrasian framework in terms of methodology. This means that Keynesians like Klein would combine the theoretical insights provided by Keynesian theorists (like Hansen and Samuelson), with the Walrasian approach of considering the economy as a system of simultaneous mathematical equations.

This structural (Walrasian) approach would use "much more complicated systems in which the reduced-form equation for consumption is vastly different from and, I [Klein] might claim, superior to [Friedman and Becker's]" (Klein 1958, 540). Haavelmo (1943; 1944) had demonstrated that the appropriate way of estimating the structural parameters of equations in a complete system is to regard the whole set of equations simultaneously from a statistical point of view.

According to Friedman and Becker (1957), one of the equations that would present a better performance would be the following equation:

(14) $C(t) = k\beta \int_{-\infty}^{t} e^{(\beta-\alpha)} Y(T) dT + u_t$

A more superior form of Friedman and Becker's (1957) equation (14) in table 2 had, according to Klein, already been used by Keynesian modelers such as Brown (1952), Klein and Goldberger (1955), and Stone and Rowe (1956). These "Keynesian macroeconometricians" had through the use of structural econometrics, found superior estimators compared to those obtained by Friedman and Becker through the least squares method. Klein (1958, 541) explained that "a direct least-squares estimate of […] equation [(14)] will, in general, be biased in large samples […] Least-squares estimates of the preceding reduced form for C_t were not the type used by Friedman and Becker. They incurred bias elsewhere, that pointed out by Haavelmo, by making single-equation least-squares estimates

of the structural consumption function." Klein further explained that "the fact that Haavelmo [1947] derived his measures of reliability of the consumption function (confidence interval for the estimated marginal propensity to consume) from the variance of residuals and inverse moment matrix of the predetermined variables in the reduced form shows unequivocally that methods without 'statistical illusion' ha[d] been well established in the literature of the subject for many years."

Criteria for judging models

One of the common features between Klein and Friedman is their emphasis on the performance of model prediction as one of the most important criterion for judging models. This is the reason why it was "a bit strange" for Klein (1958) that Friedman and Becker (1957) had put so much emphasis on the "goodness-of-fit measures" to judge the Keynesian models.

To Klein, rather than the goodness-of-fit measures, it is the "estimates of forecast error [which] provide more suitable criteria" for judging the predictive performance of econometric models. Klein insisted that in "large, realistic systems, with many identifying restrictions suggested by theory, the use of this type of criterion is more important" (Klein, 1958, p 543).

The criterion of predictive accuracy, by itself, might be a necessary but not a sufficient condition for choosing a model, since many different models can fulfill this criterion at the same time. As Caldwell puts it to make a general methodological case, in "most cases, a number of hypotheses will meet the criterion of predictive adequacy. In such cases, additional criteria of theory choice (which are 'to some extent […] arbitrary') must be invoked to choose among them" (Caldwell 1984, 228). In the case of Friedman, these "arbitrary criteria" would "include simplicity and fruitfulness, and to a lesser degree, logical completeness and consistency" (228). In the case of Klein (1958, 543-544), however,

> […] the best test is the more painful one of experience. We must build up a record of
> concrete forecasting results of Keynesian models against those of alternative systems,
> naive or otherwise. At this point we may note the logical weakness of naive models in
> picking out the all-important turning points. The success of Keynesian models in the turn
> of 1953-54 is an important score, putting this approach well in advance of naive
> persistence. Of course, there are an infinite number of naive models, and perhaps the
> authors can concoct one that will be brought into play ex-post for this turning point.

Also based in some kind of "painful experience" was the Adelmans' (1959) test of the Klein-Goldberger model evoked in section II. Adelman and Adelman's conclusion was that "it

is not unreasonable to suggest that the gross characteristics of the interactions among the real variables described in the Klein-Goldberger equations may represent good approximations to the behavioral relationships in a practical economy" (620). Furthermore, the Adelmans stated that

> when random shocks of a realistic order of magnitude are superimposed upon the original form of the Klein-Goldberger equations, *the cyclical fluctuations which result are remarkably similar to those described by the NBER as characterizing the United States economy*. The average duration of a cycle, the mean length of the expansion and contraction phases, and the degree of clustering of individual peaks and troughs around reference dates all agree with the corresponding data for the United States economy. Furthermore, the lead-lag relationships of the endogenous variables included in the model and the indices of conformity of the specific series to the overall business cycle also resemble closely the analogous features of our society. All in all, it would appear that the shocked Klein-Goldberger model approximates the behavior of the United States economy rather well (my emphasis).[53]

Not only had Klein and Goldberger built a model system allowing for the understanding of the economy, but this model system was also able to describe the US economy with such accurateness, as the very precise descriptions of the NBER. This kind of test could also be understood as part of what Klein called "painful experience." Yet Klein understood that large-scale macroeconometric models were not a "once-and-for-all-job" (Klein and Goldberger 1955, 1), but a constant building practice, involving many people, and much tinkering.

> Contrary to Friedman's tastes, other researchers have sought improvement in the Keynesian consumption function through the introduction of new variables. There are great limits to the extent to which one can come upon radically improved results by juggling about the same old variables in a different form. Instead of adhering to the 'rule of parsimony,' we should accept as a sound principle of scientific inquiry the trite belief that consumer economics, like most branches of our subject, deals with complicated phenomena that are not likely to be given a simple explanation […] The addition of extra predetermined variables (not lagged incomes) that are not correlated with income or that do increase the multiplier are likely to improve the fit of the multiplier equation at the same time that they are improving the fit of the consumption equation. I venture to predict that

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[53] Note here that the works of the NBER are also taken as the standard of test to assessing whether a model provides an accurate description of the US economy or not. In this case both the NBER's descriptions and the naïve models would perform as the "zero hypotheses" to test the forecasting performance of models.

much good work will be done in the years to come on adding new variables to the consumption function and that it will not be illusory (Klein 1958, 545).

Concluding Remarks

Friedman's longstanding relationship with the members of the Cowles Commission was not only conflictive, but it was also fruitful in terms of methodological discussions. Both factions involved in this enduring relationship developed two empirical approaches that marked postwar economics. From the point of view of macroeconometrics, Klein and Friedman embodied a reinterpretation of the ancient Walras-Marshall divide that ended up providing two ways of conceiving model systems characterized by their differences in their purposes, size and set up, as well as by their procedures and routinized practices. This debate also yielded the important "naïve models," which became of popular and common usage in econometric practice. The most important point, however, is that because of the "tooled" nature of twentieth century economics, a reconfiguration occurred in the relationship between pure theory, applied theory, data and policy issues. These spheres lost their hierarchical character and were necessarily "fused" into the macroeconometric model that could no longer account for a clear distinction between them. The macroeconometric tool changed the nature of economic theory and the way economic knowledge was produced.

Friedman's criticism of US-Walrasianism pointed to the idea that this approach would be empty mathematization of economics. Klein's Walrasian approach, however, shows that a mathematized framework of general equilibrium, combined with statistical theory and with a great amount of empirical work, could be useful to provide a tool of reasoning for understanding and intervening the economy. It is important to underline that Walras's influence in twentieth century US-Economics is weaker than it is suggested by the label. In fact, the careful study of the practices and visions of economists, and especially of econometricians in the United States during the 1940s and 1950s, suggests the development of a particular and new approach that carries the word "Walras" only as a label. This new approach is built around an "epistemological credo" of general equilibrium (rather than around Walras's original project), which provided a solid framework from which to think about the relations between the economic variables.

Structural macroeconometricians were particularly bound to this credo. The macroeconometric "tool" itself allowed for the conception of the economy only as a set of simultaneous equations, always expressing the relationships between the variables in a general

equilibrium framework. Econometrics did not establish only a method from which to produce a particular model system allowing for understanding and intervening the economy, but as Vining (1947) put it, econometrics also put a "strait jacket" to the possibilities of explaining phenomena. If the researcher sticks to a particular way of producing a model system in order to produce knowledge from a reasoned and logical procedure, then she also restricts herself to a number of possible explanations she can find only in that particular system. Other explanations that are not conceivable within the general equilibrium framework will then just be excluded, or they will not even be possibly conceived within the rules of this particular model system.

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