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MACROECONOMETRIC MODELING AND THE SSRC'S  
COMMITTEE ON ECONOMIC STABILITY, 1959-1963

BY

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CENTER FOR THE  
HISTORY OF POLITICAL ECONOMY  
AT DUKE UNIVERSITY

# Macroeconometric modeling and the SSRC's Committee on Economic Stability, 1959-1963

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## Abstract

We study the construction of the macroeconometric model of the Committee on Economic Stability (CES) of the Social Science Research Council (SSRC) in the early 1960s using the CES's archival records. Building this model was central not only to set the bases for the subsequent construction of other models such as the Brookings Model (1963-1972) and the FRB-MIT-Penn model (1966-1974) but also to consolidating large-scale macroeconometric modeling as a scientific practice at the frontier of macroeconomics in the 1960s. We provide a detailed account of the complex establishment of the Committee and argue that the organization of the CES was the response of an important group of economists concerned about the instability of the US economy and wanting to develop new methods with the “widest possible degree of acceptance” to understand concrete fluctuations of the economy and to act upon them to maintain stability. As the first model-building enterprise of this size, the project's many challenges in terms of logistics, data, and computing capacity, evidence the importance of configuring a specific institutional and material context necessary to develop this new scientific practice. In this sense, we discuss the functioning and management of this ambitious project and, in particular, the structure and organization of the team in charge of the model (or “federation of research projects”), which was structured around more than 20 researchers based in different locations. We argue that the CES was successful in bringing together academics and people from government agencies and in the very practical purpose of producing, collecting, centralizing, and managing data for the purpose of generating quantitative policy analysis.

**Keywords:** History of macroeconomics; macroeconometric modeling; Committee on Economic Stability; Social Science Research Council; large-scale macroeconometric models; Brookings Model;

**JEL Classification:** B22, B23, B4.

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# Macroeconometric modeling and the SSRC's Committee on Economic Stability, 1959-1963

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## 1. Introduction

The Committee on Economic Stability (CES) of the Social Science Research Council (SSRC) played a central role in consolidating large-scale macroeconometric modeling as a scientific practice at the frontier of macroeconomics during the 1960s.<sup>3</sup> The Committee was behind the initial work (1960-1963) in the construction of the model that set the bases for the Brookings Quarterly Econometric Model of the United States (1963-1972) on the one hand, and of the FRB-MIT-Penn model (1966-1974) on the other. In this paper, we use the Committee's records to document its work on the construction not only of a specific large-scale macroeconometric model of the United States, but also of the practice of macroeconometric modeling, which marked the way of doing macroeconomics in the subsequent decades. This project brought together top talent from academia, government agencies, and private research organizations providing bridges of communication among these institutions and a common roof for those interested in the opportunities offered by macroeconometric model-building. Furthermore, as the first model-building enterprise of this size, the project's many challenges in terms of logistics, data, and computing capacity, evidence the importance of configuring a specific institutional and material context necessary to develop this new scientific practice.

Given the novel and innovative character of this practice in the early 1960s, the team involved in this project was, in some sense, groping in the dark and trying to find and invent specific solutions to the institutional, methodological, technical, and theoretical challenges it faced. In this sense, this is a story of discovery and invention of a new and collective way of doing macroeconomics that was developed within a specific institutional context and through the effort of economists that might not have necessarily shared the same view on how to do useful economic research. Yet, the team was guided and inspired by the works developed during the 1940s and 1950s by one of its most prominent members: Lawrence R.

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<sup>3</sup>Following Thomas Stapleford (2017, 6), we understand practices as "collections of behavior that are teleological, subject to normative evaluation [and that] exhibit regularities across people in a constrained portion of time and space." Furthermore, a scientific practice contributes to the generation or sustainment of "formal knowledge that makes truth claims."

Klein.<sup>4</sup> As a member of the Cowles Commission from 1944 to 1947, Klein had built a macroeconometric model of the US economy (Klein 1950), which was not only inspired in the earlier work of Jan Tinbergen (1937; 1939) and which introduced the latest econometric techniques developed by Trygve Haavelmo (1944), but which also defined the bases of this new practice as a teamwork effort that needed the sheltering of a particular institution.<sup>5</sup> After the Commission's abandonment of the macroeconometric project in the early 1950s (Klein 1991), this early effort was taken up by Klein at his arrival at the University of Michigan in 1949, where he embarked on an ambitious project to build a new large-scale macroeconometric model, which resulted in the construction of the Klein-Goldberger model (Klein and Goldberger 1955) and of another econometric laboratory: the Research Seminar in Quantitative Economics.<sup>6</sup>

Although these projects led by Klein in the 1940s and 1950s constitute the major inspirational sources for the construction of new macroeconometric models in the 1960s, the macroeconometric model of the Committee on Economic Stability went beyond the achievements of these earlier projects. In particular, the macroeconometric model of the Committee contributed to the definition of macroeconometric modeling as a scientific practice that consisted not only in the construction of specific large-scale macroeconometric models, but also, and more importantly, in the consolidation of a new way of producing macroeconomic knowledge. This way of producing macroeconomic knowledge consisted in the combination of a novel, complex, and powerful scientific tool (a macroeconometric model) with the constitution and intervention of teams of experts, with individual responsibilities, and whose participation was configured in a specific way under a concrete institutional setting. As such, the practice of macroeconometric modeling developed through this project reflects the kind of social science that the SSRC was trying to foster since at least the 1950s: To be scientific, the social sciences in general and economics in particular, had to be useful and practical, integrating both rigorous quantitative methods and collaborative scientific activities between academic, private, and governmental institutions.<sup>7</sup>

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<sup>4</sup> Although a pioneer in the United States, Klein had not been the first economist to ever build macroeconometric models. In fact, Jan Tinbergen had built a model of the Dutch economy and another one of the US economy in the mid-1930s, serving as one of the most important inspirational sources for Klein's project. For an account of Tinbergen's work see Morgan (1990, chapter 4) and Boumans (1999).

<sup>5</sup> For a detailed history of the origins of the Cowles Commission see Grier (2005). See Christ (1956; 1994), and Hildreth (1985) and Morgan (1990) for a discussion of the influence of the Cowles Commission in the history of economics.

<sup>6</sup> See Pinzón-Fuchs (2017, chapter 2) for a detailed account of Klein's development of large-scale macroeconometric modeling at the University of Michigan.

<sup>7</sup> For a detailed account of the kind of science promoted at the SSRC during the 1950s, particularly through the participation of the Carnegie Corporation, see Hauptman (2016).

In the second section of the paper, we provide a detailed account of the establishment of the Committee, which was set in motion by the motivation of several economists to understand the instability of the United States economy. These economists were under the impression that general models and theories of the business cycles were insufficient to satisfy the necessities of the time, and that new methods had to be developed in order to provide a way to understand concrete fluctuations of the economic system and to act upon them to maintain economic stability. The organization of the Committee was a response to these common concerns, and its main project during 1959-1963 was the construction of a macroeconometric model, a tool that promised to aid in the understanding of the US economy's instability, as well as in examining remedial policies. In the third section, we provide an account of the way in which this specific macroeconometric model was built. We discuss the general setting under which the construction of the model came into being, in particular through the organization of two Dartmouth Conferences in 1961 and 1962, and through the subsequent transfer of the whole project to the Brookings Institution in 1963. Important features that marked the construction of the model were the possibility of carrying out quantitative policy analysis, the discussions on the importance of funding possibilities, and the pluralistic spirit that stimulated the project and that resulted in the idea that the model should generate the “widest possible degree of acceptance” among academic and professional economists.

In the fourth section, we discuss the functioning and management of such an ambitious project, which are necessarily related to the construction of this large-scale macroeconometric model. For starters, the structure and organization of the team that was in charge of the model—referred to as a “federation of research projects”—was structured around more than 20 researchers based in different locations who were expected to converge around the same goal. We also discuss the Committee’s unsuccessful potential for bringing together the “econometric” and the NBER’s “historical” approaches. We argue, however, that the CES was successful in bringing together academics and people from government agencies, building a mutually beneficial relationship in which the CES had more direct access to data and even became advisers on macroeconometric model-building to at least one agency. Furthermore, the people from the governmental agencies provided their expertise to bring in more detailed knowledge about different sectors of the economy, enriching the model’s specification. The project, too, served in the very practical purpose of producing, collecting, centralizing, and managing data that would later be used for estimating and simulating the model. The last section concludes and proposes some historiographical reflections on the history of postwar macroeconomics.

## **2. The establishment of the Committee on Economic Stability**

The establishment of the Committee was the result of an SSRC Conference on Economic Instability held on June 17-19, 1959 at the University of Michigan. According to R. A. Gordon’s

account, he and other economists at the SSRC interested in the possibility of creating a committee on business cycle research proposed the conference to “get a group of economists to talk about whether such a committee seemed wise” (Gordon 1975, 31; 1959, 38). Gordon, then at the University of Berkeley, and Paul Webbink, from the SSRC and who would later oversee and handle the administrative paperwork of the Committee, were joined by 17 economists, 7 of which had positions outside academia: among the participants of the conference were people from the Brookings Institution, the Board of Governors, the Council of Economic Advisers (CEA), the National Bureau of Economic Research (NBER), the Joint Economic Committee of the Congress, and the National Planning Association.<sup>8</sup> Table 1 contains the complete list of participants (column 6) and their affiliation.

Gordon opened the discussion with a short paper in which he briefly introduced the topics which would be discussed at the conference, centered on understanding what was known about the instability of the US economy and whether there were fundamental differences between pre and postwar business cycles.<sup>9</sup> Most notably, however, he began his remarks by noting the lack of relevance of the models put forth by theoreticians, the disconnection between theoretical work and actual policy questions, and the number of concrete questions that needed answering. Geoffrey Moore (NBER), Bert Hickman (Brookings), and James Duesenberry (Harvard) also presented papers that looked in detail at the characteristics of the cycle and at the changing role of specific elements in making the economy more or less stable (e.g. fiscal policy, financial distress, and the so-called automatic stabilizers that had been put in place in the postwar).

The paper presented by Duesenberry—co-authored by Gary Fromm (Brookings) and Otto Eckstein (Joint Economic Committee)—had been specifically commissioned by the organizers of the conference and was, notably, the only paper that contained an econometric model.<sup>10</sup> As Duesenberry, Eckstein, and Fromm (DEF) put it, their purpose was to consider what sources of instability were still present in the economy and to give an indication of their quantitative importance. They presented their model as follows:

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<sup>8</sup> See the list of participants, SSRC1, box 145, folder 801. The list in the SSRC’s records shows that Paul McCracken (University of Michigan) and Arthur Burns (NBER) were originally included in the list but did not attend the conference.

<sup>9</sup> “Notes for the SSRC Conference on Economic Stability,” SSRC1, box 145, folder 801.

<sup>10</sup> “Stability and instability in the American economy,” SSRC1, box 145, folder 801. A revised version of the paper later appeared in *Econometrica* with a different name, “A simulation of the United States economy in recession” (Duesenberry et al., 1960). Duesenberry was the one initially asked to contribute the paper, and Eckstein and Fromm joined in afterwards. As Gordon puts it, the paper’s role was to provide something “to seek our teeth into at the beginning” (1975, 31). Note the Harvard connection between the authors of the paper: Fromm and Eckstein had obtained their PhDs at Harvard and Duesenberry was a professor there.

In order to test some of the stability properties of the American economy in recession, we have constructed a model which seeks to reflect the effectiveness of the much-vaunted automatic stabilizers, as well as the feedbacks of a downward spiral through consumption and through the reaction of inventories. We have endeavored to keep the model simple, yet provide it with sufficient detail that it can give a *fair reflection of the reality of recession*; the resultant model is considerably more complicated than the traditional multiplier model, but the concepts which it employs are strictly macro-economic and, on the whole, the same as the concepts used in piece-meal short-run analysis of business conditions. The model is also constructed in such a way that it can be employed as a policy model; tax rates, unemployment benefit rates, and autonomous expenditure levels are explicit parameters.<sup>11</sup>

It was a model purposefully limited to analyzing the behavior of the US economy in a recession and that, by including explicit policy parameters, allowed the authors to carry out fiscal policy experiments. Their conclusion, after carrying out several of these policy experiments, was that fiscal policy and the automatic stabilizers (e.g., unemployment insurance and automatic rate changes in income taxes) made it “possible to reduce the instability of the system considerably, and at relatively little financial cost to governments.” However, the automatic stabilizers did not guarantee that the economy would “work itself completely out of a recession,” and it was clear for DEF that “actions on the part of the government” were necessary for the economy to return to full employment.<sup>12</sup>

Prices were held constant in the model and DEF explicitly recognized that they had no theory of price change. DEF also left out monetary policy almost completely—mentioning it only in passing and very briefly—and focused on the behavior of inventory investment, treating non-inventory/fixed investment in a narrative way in another section of the paper, and limiting themselves to indicating “some of the major connections between private investment and the other variables in the system.”<sup>13</sup> Clearly, as DEF acknowledged, the model had important limitations, but their 100-page paper did a great job in showcasing the type of questions that could be investigated with a macroeconometric model and the opportunities for quantitative analysis of the cycle that such a model created.<sup>14</sup> At the same time, it allowed for a comparison between this approach and that of the NBER.

Geoffrey Moore’s paper<sup>15</sup> used a very different methodology to discuss the characteristics of the 1957-1958 recession relative to previous ones. He proceeded by presenting the evolution of various series of data, shown in several tables and graphs, that allowed him to characterize each cycle and make comparisons among them. He concluded that many

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<sup>11</sup> “Stability and instability in the American economy,” 24. Our emphasis.

<sup>12</sup> *Op. cit.*, 43, 98.

<sup>13</sup> *Op. cit.*, 90

<sup>14</sup> As Klein put it later, DEF’s model played an important role in “the whetting of the appetites” for a large-scale macroeconometric model (Klein 1975, 13).

<sup>15</sup> “Some reflections on the 1957-58 recession and recovery,” SSRC1, box 145, folder 801.



features were the same, including the duration, the severity, the scope, the shifts in the composition of output, the early decline in profit prospects and investment commitments. However, some characteristics of the 1957-58 recession were different: it had shown less financial distress, more stability of personal income and prices of commodities, and a more unstable rate of interest. Moore pointed out that although these were important differences, what was known about business cycles was still relevant. It was, however, important to “guard against over-simplification in the use of historical-perspective.” He listed three aspects in particular: 1) “Don’t confine comparisons to the immediately preceding recession, or even the last two;” 2) “Don’t confine comparisons to an average of preceding cycles; and 3) “Be aware that current developments can fall outside the range of previous experience, but use that range as a guide to help avoid the biases we are all heir to.”<sup>16</sup>

Hickman,<sup>17</sup> for his part, presented an informal discussion of the determinants of private investment in postwar United States, considering different sectors as well as the role of financial elements and monetary policy. Hickman’s was, in fact, the paper that conceded most space to monetary policy considerations, concluding that while financial causes of instability had been greatly reduced—thanks mostly to deposit insurance and to the fact that the Fed had done a decent job during the postwar—the problem of the timing of monetary policy and the challenge of handling both employment and price stability simultaneously were not going to go away anytime soon.

The summary of the discussion<sup>18</sup> shows that there was an active debate around each of the papers presented, not only on the specific elements that were considered to contribute to the stability or instability of the postwar economy of the US but also on the methodological and organizational aspects of carrying out research on this subject. The latter was a key reason for the organization of the conference and so participants had been asked to

give serious thought in advance of the conference to [their] views about the present state of research on economic stability and instability, to questions such as the adequacy of the statistical data on which analysis must perforce be based, and to suggestions regarding the improvements in this range of research which you believe could and should be attempted.<sup>19</sup>

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<sup>16</sup> *Op. cit.*, 35

<sup>17</sup> “The Determinants of Private Investment in the Postwar Economy, SSRC1,” Box 145, folder 801.

<sup>18</sup> Discussion summary, SSRC1, box 145, folder 801. Individual opinions reported in the summary should be taken with a grain of salt, however. Interoffice correspondence indicates that while the summary would be kept for the record, it would not be sent out to the conference participants or others because it contained inaccuracies which would take a lot of work to correct and the gain from doing so may not justify the effort. SSRC inter-office correspondence, Sept 22, 1959, SSRC2, box 151, folder 1721.

<sup>19</sup> Webbink to the participants in the conference, May 11, 1959, SSRC1, box 145, folder 801.

The existence of different, but not necessarily exclusive methodologies for analyzing business cycles was clear for the participants: there was the NBER's or "historic" method on the one hand and the econometric or "Keynesian" method on the other. Gardner Ackley (U. Michigan) is reported to have summarized them in the following way:

He contrasted the method of Mitchell and the National Bureau of Economic Research, where the emphasis was on the timescale, with the Keynesian method, where the emphasis was on discovering functional relationships. The Keynesian method had been effectively refined during recent years: much data had been collected on consumption, and in the area of investment several experiments had been made with acceleration models and capital-accumulation models.<sup>20</sup>

Ackley also suggested combining both approaches by comparing the actual cyclical behavior of selected variables with the behavior produced by the estimated functional relationships and then trying to account for the difference. On a similar vein, Duesenberry is reported to have admitted the inability of the econometric method to account for structural changes and to have pointed out that

it would be useful to integrate the econometric with the historical approach. Although the former might explain most of the observed variance, the latter could throw light on those characteristics of the cycle which combined to produce an unexpected turn of events.<sup>21</sup>

Also important were the data needs. In fact, the participants made remarks on the need for better and new series of data on specific variables, and for setting up procedures for making data more easily and widely available. The conference concluded with a vote in favor of the establishment of a committee at the SSRC that would fulfill several functions. As reported in Gordon's summary of the conference (1959, 39) for ITEMS, the SSRC's magazine, these functions were to:

1. Facilitate the coordination of research.
2. Help integrate current research methodologies.
3. Facilitate the collection and publication of needed data, particularly by the Federal Government.
4. Serve as a channel of communication and a facilitating agency in the field of research on problems of economic instability.

Three remarks must be made regarding these functions. The first function was specifically geared towards helping researchers working on econometric models come together. It highlighted the need for taking stock of the research available in order to avoid duplication

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<sup>20</sup> Discussion summary, p.13, SSRC1, box 145, folder 801. Note that Ackley did not draw the distinction between the NBER and the Cowles Commission.

<sup>21</sup> *Op. cit.*, 3, 7.

of work and to channel efforts into disaggregation (*Ibid.*). Furthermore, Gordon pointed out that

[i]n this way econometric business-cycle research could have much more of a cumulative effect than has been true in the past, when each investigator has started largely from scratch. It might also be possible to secure agreement on the main features which need to be built into these econometric models. (*Ibid.*)<sup>22</sup>

Second, it should also be noted that a fifth function, not reported in Gordon (1959) but included in the summary of the conference discussions, was that of providing information to policy-making agencies of the government.<sup>23</sup> Specifically, the summary reports that Henry Wallich (CEA) emphasized “the value that the model-building project could have in providing government agencies with policy recommendations” and that Duesenberry “said that simulation experiments with a model could easily be made to provide policy implications.” However, and this might explain why this function did not appear in Gordon (1959), the summary also reports that “[t]here was some debate on the question of whether the task of providing recommendations for current policy would conflict with the basic research objectives of the project.”<sup>24</sup> Unfortunately, there is no further record of the specific points that were advanced against this function during the conference.

Finally, another key difference between the discussion summary and Gordon (1959) is that the idea of building a more disaggregated model seems to have been in the air at the conference, although it is not explicitly mentioned in Gordon (1959). In the discussion summary, Duesenberry is reported as talking about the necessity to make an effort to synthesize the work being done at the time, which “would permit the construction of a model with a considerable degree of disaggregation.” Wallich’s reference to a “model-building project” further suggests that the construction of a larger model was explicitly in the minds of the attendants. Gordon (1959, 39), however, reports only that a working conference would be a good first measure to bring together the people working on this type of research.

In any case, the proposal for the establishment of the Committee on Economic Stability was accepted in September of 1959 (Gordon 1959, 39),<sup>25</sup> and the initial members of the

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<sup>22</sup> In the discussion summary of the conference Duesenberry is reported as emphasising this particular point.

<sup>23</sup> The other four functions that the committee would fulfill were also reported in the summary of the conference discussion. The main differences are that the wording in Gordon (1959) is different and no individual points of view are communicated.

<sup>24</sup> Discussion summary, 15-16, SSRC1, box 145, folder 801.

<sup>25</sup> See the “Proposal for committee on economic instability,” Sept 12, 1959, SSRC2 Box 151, folder 1721. In the end, though, the last word of the committee’s name was replaced by “Stability,” SSRC inter-office correspondence, Sept 22, 1959, SSRC2, box 151, folder 1721.

Committee were recruited in the following months. Table 2 shows the Committee's members during the early 1960s. It is noteworthy that, although Klein was one of the founding members of the Committee, he did not attend the 1959 conference and it would seem that his participation in the project was not initially guaranteed. In fact, the records show that it was considered that he might not be interested in joining the project, and the names of Irwin Friend and Robert Eisner were put as alternatives in case Klein declined the invitation.<sup>26</sup>

### 3. The macroeconometric model of the CES

The chief value of a Committee like yours, which can presumably tap both talent and money in quantity, provided it knows how to use them, should not be to encourage small jobs of the horse-and-buggy type. Rather it should try to think of those very large enterprises which individuals and small groups have rejected in the past, or perhaps never even considered, because they seemed too formidable.<sup>27</sup>

Moses Abramovitz's comments on the potential of the Committee, though directed towards a different type of project—a big survey of private firms—captured well the possibilities that the existence of the Committee opened up and that would materialize with the construction of the macroeconometric model. Indeed, the Committee's activities started taking shape right after its official establishment, and at its first meeting it was decided that Klein and Duesenberry would carry out a summer institute on econometric models.<sup>28</sup> A group led by Duesenberry and Klein met in New York in February of 1960 to start planning the summer institute and discuss the type of model they would like to eventually produce. The group concluded that

we want to produce a system that will be jointly useful in forecasting and policy formation. At first we should concentrate on a model of the ordinary business cycle of 8-10 years' duration. Disaggregation ought to be carried to the point where needs of policy makers are served. In government work, housing,

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<sup>26</sup> SSRC inter-office correspondence, Sept 22, 1959, SSRC2, box 151, folder 1721. Although we have not been able to locate sources to clarify Klein's potential negative decision, it would make sense that his participation was not guaranteed. Indeed, Klein had had a promising but fleeting trajectory as Lecturer and director of the Quantitative Economics Research Seminar of the University of Michigan from 1949 to 1954. However, he decided to leave Michigan in 1954 and to join Oxford University only to return to the US in 1959 as a Professor of the University of Pennsylvania. His departure from Michigan was prompted by the pressure exerted by McCarthyism, the House of Unamerican Activities, and the accusations Klein received from some members of the University of Michigan such as accounting Professor William A. Paton for his short membership to the Communist Party in the mid-1940s. We ignore if Klein was invited to the conference, but it is very likely given his importance in the field. However, it wouldn't be a surprise if he had decided to decline the invitation given the presence of people like Paton at Michigan. For a detailed account of this episode see Pinzón-Fuchs (2017, chapter 2).

<sup>27</sup> Abramovitz comments, Nov. 24, 1959, box 151, folder 1721.

<sup>28</sup> Minutes Washington conference, December 28, 1959, SSRC1, box 147, folder 810.

the motor industry, agriculture, foreign trade, and finance must be treated separately as areas of policy action. Business would be interested in the maximum possible degree of disaggregation. They would be especially interested in material on inventories. We do not plan to go into regional work now, but we might have someone take up this question at our summer seminar.<sup>29</sup>

Thus, despite the apparent debate that took place around the idea of building a model for policy analysis at the Michigan conference, this clearly entered into their considerations of the type of model they wanted to produce. Furthermore, they were also interested in building a model that businesses could potentially find useful, thus leaving the door open to offering services like the University of Michigan's Annual Economic Outlook Conference had been doing since the mid 1950s. The concern for the business public is not mentioned explicitly in subsequent minutes or correspondence of the Committee, however.<sup>30</sup> In any case, the concern for both policy and business usefulness evidences the importance of extra-academic interests in the Committee's macroeconomic project.

At the February meeting, it was also decided that the summer institute would last six weeks and that it would take place "at a quiet retreat" where specialists would discuss the construction of the model. These plans were subsequently altered and it was agreed that two seminars would be held instead, at Dartmouth College, during the summers of 1961 and 1962.<sup>31</sup> The idea behind this summer institutes was to get all the people involved in the same place, thinking about the common project, and to give them enough time to actually work. The first summer institute would serve as a stepping stone of the project and it was expected that each of the specialists selected to contribute to the model would "come to the seminar with an historical summary of work done in [his] sector and [with] suggestions for new formulations."<sup>32</sup> Furthermore,

[p]ersons selected to contribute papers and work at the seminar would be instructed to approach their problem with as few preconceptions as possible and to be ready to include as many variables as

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<sup>29</sup> Meeting minutes, Feb 24, 1960, SSRC1, box 147, folder 810. A subsequent letter by Klein makes a couple of corrections based on comments by Moore: the typical duration of the cycle according to the NBER is 4-5 years, and the Adelmans' simulations of the Klein-Goldberg model favored this length; the model should aim at forecasting or identifying turning points. See Klein's letter of March 21, SSRC1, box 147, folder 810.

<sup>30</sup> Many of the people involved in its work would later participate in the business of commercial econometric models. Klein, for example, would go on to lead the Wharton Economic Forecasting Associates, while Fromm and Eckstein would later found Data Resources Incorporated.

<sup>31</sup> Meeting minutes, April 30, 1960, SSRC1, box 147, folder 810. It's unclear why they chose Dartmouth College as the location for the summer conferences. It should be noted, though, that Dartmouth had an active group of faculty and undergraduate students working on making computers easier to use, whose work led to the development of BASIC and the Dartmouth Time-Sharing System.

<sup>32</sup> Meeting minutes, Feb 24, 1960, SSRC1, box 147 F810.

possible in the first instance. The overall coordinator would have the main task of trimming the parts to sizes that would fit in a workable scheme.<sup>33</sup>

A preliminary list of personnel—presenters and discussants for eleven sectors as well as an intervention on the statistical method and another on a historical summary of cycles—was agreed upon at an April meeting.<sup>34</sup> The steering committee of the SSRC, the Problems and Policy Committee, approved the econometrics model proposal in May<sup>35</sup> and in July Klein addressed his fellow Committee members with news about the project: A subcommittee composed by Duesenberry, Klein, Moore, Avram Kisselgoff (Allied Chemical Co.), and David Lusher (CEA) had been appointed to “deal with the problem of constructing an effective new econometric model of the USA.”<sup>36</sup> Klein emphasized again that they wanted their model to have the “widest possible degree of acceptance” and that they were thus “approaching the problem with no fixed ideas on the design or scope of the model.”<sup>37</sup> He also elaborated on the path that lay ahead for the project:

Specialists will be expected to work during the academic year preceding the first summer session, summarizing as much as possible of the known econometric material for his designated area. This material, together with positive suggestions by each author, will be discussed at the first session. During the following year, specialists will be expected to work on data for their sectors to be presented in a more final form at the second summer session. In this stage, data will be made mutually consistent and the forms of relationships studied will be chosen so as to fit with other contributions.<sup>38</sup>

Naturally, this type of project would require ample funding. The National Science Foundation (NSF) provided a \$105,000 two-year grant, plus a \$20,000 extension in 1963. The cover letter of the original proposal, sent by the SSRC’s president, Pendleton Herring, mentioned the same elements highlighted by Gordon (1959) and that had motivated the creation of the Committee: the team would “evaluate critically” the econometric work done on specific sectors of the economy and “establish a basis for a *generally acceptable* model of the economy.”<sup>39</sup> The Board of Governors of the Federal Reserve was also contacted, but their reception of the project was lukewarm. They were happy to let Daniel Brill<sup>40</sup> collaborate with the project, but Governor A. L. Mills felt it was not appropriate for the Board to

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<sup>33</sup> *Op. Cit.*

<sup>34</sup> Meeting minutes, April 30, 1960, SSRC1, box 147, folder 810.

<sup>35</sup> Webbink’s letter to Gordon, May 27, 1960, box 151, folder 1721.

<sup>36</sup> July 13, 1960, SSRC1, box 147, folder 810.

<sup>37</sup> *Op. Cit.*

<sup>38</sup> *Op. Cit.*

<sup>39</sup> Herring to Riecken, Oct 04, 1960, SSRC2, box 151, folder 1721. Our emphasis.

<sup>40</sup> At the time Brill was an Associate Adviser at the Division of Research and statistics, and had been chosen as the specialist in charge of the monetary sector. Brill would become the director of the Division of Research and Statistics a few years afterwards, playing a key role in the FMP model project. See Acosta and Cherrier (forthcoming).

finance an outside organization in a project of this kind. If the project had the promise that seemed to be expected from it, it would in a sense be similar to the Talle Subcommittee studies that the Board undertook several years ago pursuant to Congressional request, and if that were the approach the study perhaps should be focused entirely in the Board.<sup>41</sup>

While the Board did not commit any funds at the time, it did authorize the staff to discuss the matter further with the Committee. Webbink reported that

[f]urther discussion with Jack Noyes [Director of the Board's Division of Research and Statistics] has made it clear that getting financing from the Federal Reserve would require a more specific statement of plans and anticipated results. It would probably be better to err on the modest side of this rather than on the expansive side, but it might also be necessary to make some contention that what will be accomplished is something that the Federal Reserve otherwise, sooner or later, would have to do, or at least ought to do, with its own staff.<sup>42</sup>

The idea of obtaining funds from the Board was eventually dropped on the ground that it was very uncertain and that convincing its members would take too much work.<sup>43</sup> In addition, both Webbink and the members of the Committee were quite confident that they would obtain the funding from the NSF, as it effectively happened.<sup>44</sup> It is interesting to note, however, that there would seem to be at least some agreement between both parties—the CES and the Board—regarding the potential usefulness of the project. Furthermore, Webbink's proposed strategy of emphasizing the inevitability of the project is an example of the push for the use of quantitative tools for policy analysis that was part of the Committee's ethos.

A planning meeting took place in February of 1961 and participants in the summer institute got to interact with each other and plan their contributions for the summer. The minutes of the meeting report that "[t]he main point taken up [...] was the division of the economy into sectors, and an attempt to reach some preliminary agreement within the group on the work to be done by each sector specialist in preparation for the first summer seminar."<sup>45</sup> A

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<sup>41</sup> Minutes of the Board meeting of September 23, 1960, 4ff. The minutes of the Board meetings are available at <https://fraser.stlouisfed.org/title/821>. The only explicit objection recorded in the Board minutes is by Governor A. L. Mills Jr.. Since no contrary points of view were raised, we can assume that there were no strong opinions in favor of the project other than the favorable words mentioned regarding the prestige of the SSRC and of the economists associated with the project.

<sup>42</sup> Webbink to Gordon, Oct 05, 1960, SSRC2, box 151, folder 1721.

<sup>43</sup> *Op. Cit.* See also Gordon to Webbink, Oct 10, 1960, SSRC2, box 151, folder 1721.

<sup>44</sup> On the approval of the NSF grant see Fouraker to Klein, June 16, 1961, SSRC2, box 151, folder 1721. There is no further mention of the project in the Board's minutes before the December 12, 1960 meeting (pp. 4-5). By then the project had already been funded and the matter was closed at the Board.

<sup>45</sup> Meeting minutes, Feb 3, 1961, SSRC1, box 147, folder 810.

discussion initiated by Edwin Kuh led to the reaffirmation that their objective was to build a model that was useful for solving policy questions and not a pure forecasting model. This was followed by a discussion about the type of variables that would be needed to build a model that shows the flexibility with which the model as a whole was being considered:

There was a general discussion of the approach to policy uses. Jim Duesenberry suggested that we make up a list of policy variables and be sure that each sector specialist includes some work on these if relevant to his sector. Franklin Fisher noted that our list should include a number of policy variables suggested by intuition and theory. Karl Fox raised the question of the influence of the end use of the system on the degree of disaggregation, and the choice of *targets* or *instruments*. We agreed generally that the system should, at first, be left open so that any variables of potential importance could be included.<sup>46</sup>

The discussions of the specialists about their individual sectors also show how their work constrained and was constrained by the rest of the model. For example:

Dan Brill, who will study the money market, raised important questions as to the coordination of this work with that in other sectors on saving (business saving and personal saving including residential construction) and with the financial variables that appear in the various behavior equations of the system. From the other sectors, we listed the following possible financial variables: consumer credit terms, mortgage rates and terms, long term rates, short term rates, share prices, gold stock, foreign liquid balances, and corporate balances. This brought Dan's work into focus. At the same time we asked of him a precise statement and listing of the control variables in the monetary sector. He plans to study these under the broad headings of treasury debt management, open market operations, reserve requirements, and government corporations. We asked him to show how specific control variables under these general headings are (structurally) related to the monetary variables appearing in the equations of the other sectors of the economy.<sup>47</sup>

### **The Dartmouth conferences**

The first of the two summer institutes took place at Dartmouth College during August 7-25, 1961. The meeting brought together the team of researchers directly involved in the model project, as well as some guests and research assistants, to discuss the reports that had been written since the February planning meeting and to start structuring the model. Table 1 (columns 7 and 8) lists the participants in the two Dartmouth conferences. As Klein noted in his summary of the conference, it was a format that enabled the functioning of the new model-building strategy that the Committee was inaugurating:

The subcommittee recommended a new approach to model building. The limited scope of most other efforts in this field can be attributed to the fact that they have basically been "one-man" jobs. At best a small, closely knit research team with not more than one or two or three principal investigators have

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<sup>46</sup> *Op. cit.*, 2-3. The terms "targets" and "instruments" were emphasized in the original. Note that they correspond to Tinbergen's usage.

<sup>47</sup> *Op. cit.*, 3.



undertaken the task of constructing an economy wide model. The subcommittee suggested that a large research group be assembled for periodic meetings with private research being conducted by individuals between meetings [...] Instead of the small, closely knit research team, we decided upon a *federation of major research projects* united at periodic conferences and held together by two coordinators.<sup>48</sup>

Such a format allowed “investigators to meet in a common discussion where each separate research effort could be adjusted towards fitting in a systematic whole,” a necessary counterbalance to the main disadvantage that such a team effort implied:

[T]he possibility of heterogeneity and lack of complete research discipline and coordination [are the main disadvantages]. Each separate investigator may be inclined to attach too much importance to many small points within his sector. These small points may not be significant when considered from the point of view of an over-all model of the economy.<sup>49</sup>

Klein also highlighted the broad knowledge of the participants:

[W]e had an unusual array of talent. Each person knew economic theory, statistical theory, and realistic description of behavior associated with his own sector. Many of the people knew other sectors well, and criticism was highly constructive. New ideas about model construction came out of the discussions.<sup>50</sup>

The discussions were productive and actual work—even if still exploratory in nature—was carried out during the conference. The reports prepared by the researchers, many of which contained exploratory calculations, were complemented with additional calculations made on the computers of the Dartmouth College and of the Board of Governors. Data, however, was clearly a pressing constraint on the project, and Klein’s summary evidences the importance of being in contact with people from the agencies that could alleviate such constraints:

Data problems arose frequently, and we discussed practical means for obtaining assistance in getting necessary data from government agencies. The visit of George [J]aszi to our conference in connection with other meetings of the Committee on Economic Stability was fortunate. We were able to discuss with him the obtaining of special series for our purposes from the National Income Division of the Department of Commerce.<sup>51</sup>

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<sup>48</sup> “The Dartmouth Conference on an Econometric Model of the United States,” August 7-25, 1961, SSRC1, box 147, folder 810. Our emphasis. A slightly reduced version of this summary appeared in ITEMS as Klein (1961).

<sup>49</sup> *Op. cit.*, 4.

<sup>50</sup> *Op. cit.*, 3.

<sup>51</sup> *Op. cit.*

The issue of the level of disaggregation was particularly important. It was one of the main objectives of the project, but dividing sectors into subsectors could easily get out of hand, and any explanatory variable included in a particular sector would need to be endogenized in the model as a whole. In the end, it was agreed that they would work on three increasingly disaggregated models, setting a 30-sector model as the arrival point, although they considered it was unlikely that they would get there by 1962.<sup>52</sup> Following the conference, researchers were expected to work on preliminary versions of their sector models: it was “expected that participants [would] appear next year at the research conference with a tentative set of equations for [their] sector[s] and series of prepared data.”<sup>53</sup> Duesenberry and Klein, in their role of coordinators, would be in charge of writing the proposed models of the whole system for each of the three levels of disaggregation.

A preliminary outline of the aggregative model was ready by November 1961,<sup>54</sup> and an interim meeting took place on February 22-23, 1962 at the Brookings Institution.<sup>55</sup> Reports on individual sectors were presented and the initial aggregative model was modified to incorporate the researchers’ new work. Researchers were asked to send Charles Holt and Franklin Fisher—who were in charge of studying the properties of the model and the details of its estimation, respectively—information on the mean and variance of their series, preliminary OLS estimations if they had them, or their “best guesses” if they did not.<sup>56</sup>

The second Dartmouth conference took place during August 6-17, 1962 (Klein 1962). As for the previous year’s meeting, Klein’s summary emphasized the importance of the criticism offered by the team of researchers on each individual sector. And, again, actual work got done, but this time there was much more to work with in terms of preliminary results:

For the 1962 conference we had nearly complete presentations of single-equation least-squares estimates of the relations that will be taken into account in the aggregative model. Some of these were known in crude form at the 1961 conference; some were available at our interim meeting in February; but most were put before the group for discussion for the first time this past summer. (Klein 1962, 39)

As a result, a clearer picture of the structure of the model as a whole emerged. While there were still “some loose ends in the system,” and some equations and identities had not yet been decided upon or adequately specified, the team was able to put together a “nearly

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<sup>52</sup> This discussion on the level of disaggregation of the model led to the development of a way to combine input-output matrices with more traditional econometric modeling, a distinguishing feature of the Brookings model (Bodkin et al. 1991, 99).

<sup>53</sup> *Op. cit.*

<sup>54</sup> Klein to Webbink, Oct 19, 1961, SSRC2, box 151, folder 1721. See also Klein’s letter of November 1, 1961, SSRC1, box 147, folder 810

<sup>55</sup> Minutes, Washington meeting, Feb 23-24 1962, SSRC1, box 147, folder 810.

<sup>56</sup> *Op. cit.*

complete” flow diagram of the aggregative model (Klein 1962, 39; see figure 1).<sup>57</sup> The exact number of equations would depend on how these issues were dealt with, but the aggregative model was around 100 equations while the next stage in disaggregation would take it to around 300 equations, the main difference being the number of production sectors included in each version. Although modifying the model, improving or augmenting it in any sense, was a complicated job potentially involving work on several sectors of the model, Klein “hope[d] to have a *living* model that will be kept up to date, continuously improved, and explored for the possibility of incorporating further sector detail” (Klein 1962, 38). This, however, was the work to be undertaken in the next stage of the model project:

At the conclusion of the second summer conference we held an organizational meeting, at which it became clear that the participants in our project had indeed found a fruitful basis of research cooperation that we want to continue indefinitely into the future. With the two models being planned, we still have much to do and can readily conceive of specialized or more refined studies along the same lines continuing as far ahead as we care to look. We therefore agreed that the committee should seek means of perpetuating its project and continuing our joint research effort. We do not plan to meet in the summer of 1963 but will reconvene as a group when a model has been fully estimated and applied.

By 1964 or 1965, we should be ready for this stage. We agreed that a permanent research base should be sought for the model, where it could be maintained and extended by a small permanent staff. In that case special projects could be undertaken by members of the larger research team, and periodically the group as a whole could consider the entire model. The group would include the present collaborators, but additions or retirements would be possible. (Klein 1962, 40)

### **The Brookings model**

The next home of the model would be the Brookings Institution. Although it is unclear from the available records whether any alternative site was seriously considered, Klein provided an explicit argument in favor of choosing the Brookings Institution over a university to host the project:

We selected the Brookings Institution as a highly desirable site because it removes the model from any particular school of thought in economics. The diversity of views among the members of our team and the wide acceptance throughout the profession that we are seeking for this model suggests that it should not be in any particular university where it may eventually become dominated by a small group of economists who tend to think along similar lines. We want it near data sources, and we want it in an establishment with known research facilities. To us, the Brookings Institution seemed to be an ideal locale, and we were pleased to have an enthusiastic reception for this idea from the Brookings staff.<sup>58</sup>

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<sup>57</sup> The diagram was also presented at the December meetings of the Econometric Society, unfortunately it wasn't present in the SSRC records. The version of the diagram included as a figure comes from the August, 1964 progress report of the Brookings model, BIA. This same diagram was also included in Duesenberry *et al.* (1965). These flow diagrams likely played a role helping the team make sense of the model and as an explanatory device for outsiders.

<sup>58</sup> See the NSF grant proposal “An econometric model of the United States economy,” January 30, 1963, 7, BIA. The grant proposal notes explicitly that Klein wrote the section from which this quote

In any case, many members involved in the Committee's work would continue to be involved in this new stage at the Brookings Institution. Most notably, Klein would be the principal investigator of the project, Fromm would be the project's staff director, and Duesenberry would chair an advisory committee that would help steer the project.<sup>59</sup> At first sight, this transition to the Brookings Institution seems to have been an easy one, but judgment should be reserved until more research has been done on this new stage of the model at Brookings.

After the second Dartmouth conference, the team worked hard to "get the model into shape" before handing it over to the Brookings Institution.<sup>60</sup> Thus, by September of 1963, the team had centralized the source data, transferred individual series to punch cards, and had estimates of the most of the equations in individual sectors. Estimates of the small model as a whole, however, were expected to be available only by December of the same year. Many of the individual sectors had been presented at academic conferences and a volume describing the work carried out from 1961-1963 was being put together.<sup>61</sup> This is the volume that would later be published as "The Brookings Quarterly Model of the United States" (Duesenberry *et al.* 1965). Despite the name, this volume encapsulated mainly the work undertaken by the Committee's project members between 1961-1963. In particular, the volume assembled the papers written by the sector specialists—discussed and reworked during and since the Dartmouth conferences—and offered a version of the aggregative model of around 150 equations.

As Duesenberry and Klein pointed out in the introduction to the 1965 volume, each paper could "stand on its own merits as a piece of independent research," but, taken as a whole, the work presented in the volume represented a "complete model which 'explains' the variations in *GNP* and its major components, as well as major price movements, employment, and wage rates" (Duesenberry and Klein 1965, 3). Furthermore, the "general outline of the model reflect[ed] a consensus [...] on the best set of working hypotheses about the nature of the economy" (*ibid.*). To be sure, the work on the model had not ended, and an important message conveyed in the volume's introduction was that it presented "only the first stage of a continuing effort" (*ibid.*). The new stage of the model at the Brookings institution showcased an important element of macroeconometric modeling as a practice, which involved the

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was taken. Klein's argument is also noteworthy because both the Brookings model and a version of the FRB-MIT-Penn model would end up being housed by Pennsylvania's WEFA in the 1970s.

<sup>59</sup> *Op. cit.*, 10ff.

<sup>60</sup> Klein to Webbink, January 23, 1963, SSRC2, box 151, folder 1722. Since the original NSF grant ended on July of 1963 the Committee had to ask for, and effectively got, a new grant to support the work during the summer. See Klein to Webbink, May 29, 1963, SSRC2, box 151, folder 1722.

<sup>61</sup> See the "Report on the econometric model project of the Committee on Economic Stability, Social Science Research Council," SSRC1, box 147, folder 811.

continuous, collective, and institutionalized working and reworking of the model as a whole and of its sectors, of the specification and re-specification of its equations, and of the estimation and re-estimation of the parameters, taking into account new information and research about the economy as it became available. In this sense, the objective of the project was not to “produce once and for all a fixed model of the American economy,” but to “bring together in a continually [and collectively] revised model all tested research results in the field of aggregate economics” (Duesenberry *et al.* 1965, vii). The model project provided a flexible, yet systematic “place to put things”—all the “knowledge about fine-grained sectors” produced by economists—thus contributing as well to making econometric work a cumulative process (Duesenberry and Klein 1965, 9).

Further analysis of the development of the model’s new stage at the Brookings Institution is beyond the scope of this paper. Suffice it to say that years of intense work laid ahead for the team and were documented in several Brookings Institution volumes describing the subsequent work done on the model. The journey would not be easy nor would it go as expected. As Griliches’s (1968) highly skeptical review of the 1965 volume showed, there was much work left to do to convince economists that such a large-scale project could produce useful results. The Brookings stage of the project, therefore, should be the subject of further study.

#### **4. Towards the consolidation of a practice**

In the last two sections we have documented the establishment of the Committee and its work on the macroeconometric model project during 1959-1963. With this general overview as a basis, we can now comment further on some of the project’s characteristics that should be taken into account to understand the consolidation of macroeconometric modeling as a practice during the 1960s.

##### **Managing the Federation of research projects**

As we briefly mentioned in the introduction, several other macroeconometric models had been built before the Committee’s project began, but the latter was far more ambitious in terms of size, complexity, and scope than any other earlier project of this sort. The Committee’s project involved more than 20 researchers, located in different types of institutions and in different geographical places, whose work had to be steered towards a common goal. The Committee’s project was not just a technically difficult enterprise, but its logistic challenges were significant as well. And yet, there is practically no evidence in the Committee’s records of any major personal, logistic, or administrative problem regarding the

model-building project nor any of the other activities of the Committee.<sup>62</sup> To be sure, the paper trace of discussions on these matters can never be perfect and it is certainly possible that critical matters might have been dealt with in person or by telephone. Furthermore, our sources have so far been limited to the Committee's archives and it is possible that the archives of individual participants might allow for an alternative interpretation. In any case, the lack of evidence of any major problems in the internal correspondence of the Committee is noteworthy given the scale and complexity of their project.

There are a couple of factors that might explain why the Committee and the macroeconometric model project ran with relative smoothness. The first factor has to do with the important institutional infrastructure that supported the Committee, reflected in the Social Science Research Council's support, embodied in Paul Webbink's figure who oversaw the Committee's activities. The SSRC managed the grant funds of the Committee's projects, reducing the administrative burden on researchers. Furthermore, by providing information and at times demanding information from researchers, sending out reminders, and occasionally offering logistic advice, Webbink and the SSRC provided important administrative expertise that certainly contributed to the smooth working of the Committee and thus to the success of its work. At the same time, working as an SSRC's Committee possibly helped the team obtain the funds needed to carry out their work, although more research is needed on the National Science Foundation's funding of economics projects during the 1950s and 1960s to throw light on this issue.

A second important characteristic was the summer institutes format that the Committee adopted. The two summer institutes at Dartmouth College, during 1961 and 1962, were essential for the progress of the project and allowed researchers to come together under the same roof for a period of time long enough to allow for actual and communal work to be done. It is difficult to say whether the "federation of research projects" approach would have worked without such opportunities for the whole group of researchers to come together and work on the model. While the Committee did meet at least a couple times a year—sometimes after meetings of the American Economic Association, sometimes at the Brookings Institution in Washington, and sometimes at the SSRC's offices in New York—it would have been impossible to schedule (and afford) regular meetings for more than 20 people. It is also unclear, however, that such a different format would have actually improved the Committee's functioning, since the effectiveness of the summer institutes consisted in the provision of enough time to individual researchers to carry out work on their own and then to discuss it with the rest of the team for a couple of weeks. The origin of the summer institute

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<sup>62</sup> While the model-building project was by far the main activity of the Committee during the early 1960s, other possible projects were continually being discussed and a conference on quantitative policy analysis was organized on August of 1963.

idea is, unfortunately, not clear. We cannot tell yet with certainty whether this idea was an original idea, or if it was adopted from previous experiences in economics or other disciplines, or from previous experiences in other SSRC committees.

Yet, some characteristic elements of the functioning of the Committee probably found their inspiration in the Research Seminar in Quantitative Economics (RSQE) of the University of Michigan, and in the Conference on the Economic Outlook organized yearly since 1953. Klein, of course, played a paramount role in setting both the RSQE and the Economic Outlook Conference at Michigan (Pinzón-Fuchs 2017, chapter 2). The weekly RSQE was “really based around this project team research effort” (Klein and Goldberger 1955, 1), and was understood as an ongoing project where Klein and Arthur Goldberger built what came to be known later as the Klein-Goldberger model. The Economic Outlook Conference was the yearly event attended by economists from different companies where the use of earlier versions of the model was unveiled, providing informative forecasts about the behavior of the economy for the following year. Daniel Suits describes the way in which the seminar worked in the early 1950s:

The seminar would tool up in September when the students arrived and the assignment was to take the model apart and see where it had functioned poorly last year and what should be done about it to improve it, with the notion that come the second or third week of November [...] somebody had to stand up in front of that Conference on the Economic Outlook and produce a forecast from this model.<sup>63</sup>

Finally, the personal qualities of the people involved must also be taken into account. Besides Webbink’s outstanding role, the two chairmen of the Committee during our period of interest, Robert Gordon and Bert Hickman, seem to have done things right in terms of supporting ongoing research and keeping communication fluid with Webbink and the rest of the Committee members. As the coordinator of the model project, however, it is Klein that deserves the most praise in this respect. Duesenberry’s involvement in the project seems to have been intermittent at certain times,<sup>64</sup> and the fact that almost all of the correspondence related to the model in the Committee’s archives is from Klein shows that he was the main overseer of the project.<sup>65</sup>

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<sup>63</sup> Suits, quoted in Brazer, “The Economics Department of the University of Michigan: a centennial retrospective,” TUMA, box 5, p. 143.

<sup>64</sup> This is the only noteworthy difficulty that appears in the Committee’s records, but even this does not seem to have been a major cause of concern, most likely because Klein provided a sturdy backbone for the project. Duesenberry’s apparent lack of commitment to the project emerged most notably in the discussion to choose a replacement for Gordon when he announced he had to cut back on his responsibilities due to an illness.

<sup>65</sup> Webbink made his appreciation of Klein’s work explicit: “[L]et me say [...] that the economics community owes you a very large quantity of gratitude for the skill and devotion with which you have directed the project, and that I am personally grateful for the experience of dealing with someone so

## **The role of the NBER**

As already mentioned in section two, George Moore of the NBER participated in the 1959 Michigan conference,<sup>66</sup> presenting a paper that showcased the NBER's approach to the analysis of business cycles. In particular, Moore's paper served as a useful way to contrast Dusenberry, Eckstein, and Fromm's contribution, above all since the conference participants seem to have considered that complementarity of both approaches, the "historical" and the "econometric," was desirable. Although the establishment of the Committee seemed like a good opportunity for the collaboration between the NBER and the econometricians to flourish, this collaboration does not seem to have really taken up, at least in any direct way. In fact, although Moore was a member of both the Committee and the macroeconometric model Subcommittee,<sup>67</sup> he did not play a major role in shaping the agenda of the Committee and did not contribute to the model project beyond the first Dartmouth conference.

The only project that Moore presented for consideration by the Committee was the co-sponsoring of a "time series encyclopedia" that the NBER wanted to prepare and publish.<sup>68</sup> Moore considered that the Committee's involvement would make it easier for the NBER to find the funding necessary for the project, and the Committee agreed it was a valuable initiative. The NBER did, however, organize a conference on economic planning in 1964, a year after the Committee organized a conference on quantitative policy analysis that, incidentally, did not include anyone from the NBER.<sup>69</sup> If there was ever a potential for complementarity between both institutions it was not being developed through these projects.

Regarding the macroeconometric model, at the February 24, 1960 meeting it was decided that it would be useful to have a contribution on the "historical summary of the main features of individual cycles" for the next year's summer institute.<sup>70</sup> It was initially considered that Abramovitz would work on this, but by the time of the planning meeting of February 3, 1961 that task was most likely going to be taken up by Moore. According to the minutes of the

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thoroughly responsible and systematic. I hope that your graduate students are absorbing these qualities from you." Webbink to Klein, August 5, 1963, SSRC2, box 151, folder 1722.

<sup>66</sup> It is interesting to note again that the list of participants in the Committee's records points out that Arthur Burns was also invited but did not attend. See SSRC1, box 145, folder 801.

<sup>67</sup> The records show that he attended the meetings of the Committee and the Subcommittee regularly for 1959-1961, that he did not attend the two Subcommittee meetings of 1962, but that he did attend again the 1963 Committee meeting.

<sup>68</sup> See the minutes of the meetings of December 28, 1959, and December 28, 1960, SSRC1, box 147, folder 810.

<sup>69</sup> The proceedings of the NBER conference were published as (Millikan 1967); the proceedings of the Committee's conference were published as Hickman (1965).

<sup>70</sup> Meeting minutes, SSRC1, box 147, folder 810.



meeting, Moore's paper "would indicate the type of features that ought to appear, explicitly or implicitly, in our final model if it is to give a faithful representation of American business cycles, as measured by the National Bureau."<sup>71</sup> Moore effectively participated in the first Dartmouth conference, but there was no contribution on this topic at the second Dartmouth conference. Similarly, no chapter dealing with a historical analysis of the US cycles was included in Duesenberry *et al.* (1965), although the possibility of validating the model by comparing its output against the NBER's characterization of business cycles was very briefly mentioned by Holt (1965, 640) in his chapter on the simulation work done on the model thus far. It is possible that this was due to the fact that initial simulations of the complete model were not available before the project was handed over to the Brookings Institution, but no historical chapter was included in the Fromm and Taubman (1968) volume or in any of the subsequent volumes describing the work on the Brookings model. It is also important to note that while the initial thought was that the NBER's approach could *complement* the results of the econometric model, the relationship quickly passed to seeing the NBER's work as a way to *validate* and not extend or complement the econometric model. This should be seen as a continuation of the debates held between Klein and Friedman in the late 1940s and early 1950s on the role that NBER methods, such as "naive-models" should play as standards to measure the performance of large-scale macroeconometric models, notably in the context of Carl F. Christ's (1951) work at the Cowles Commission.<sup>72</sup> It could also have been a reaction to the work done by Irma Adelman and Frank L. Adelman (1959) on the Klein-Goldberger model.<sup>73</sup>

### **The connection with government agencies**

The Committee was successful in bringing together academics and people from government agencies. This was important on at least three fronts: the expert knowledge that these people brought to the project, the direct access to the data from their agencies, and the potential use that these agencies could make of the results of the model. All of these elements contributed to carrying out the project in line with the goals laid out by the Committee, and also with its ultimate aim of increasing the understanding of the *actual* US economy. Since we lack sources related to the planning of the 1959 Michigan conference, we do not know how the contact with these government agencies and researchers was initially made, but looking at the academic background of the participants (see table 1) we can see that all but one<sup>74</sup> of the five government-affiliated participants held a PhD. Similarly, and although our data still has some gaps, we can see that at least four (out of seven) of the government-affiliated participants at Dartmouth 1, at least three (out of seven) of the participants at Dartmouth 2, and four (out of five) of the government-affiliated contributors to Duesenberry *et al.* (1965)

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<sup>71</sup> SSRC1, box 147, folder 810.

<sup>72</sup> See Pinzón-Fuchs (2017, chapter 4) for a discussion on the debate between Klein and Friedman.

<sup>73</sup> Holt (1965) cites their work.

<sup>74</sup> It is not clear whether Louis Weiner effectively graduated from his Ph.D. at Harvard.

held PhD. degrees. This suggests that the Committee established a connection with the people who could understand the technical discussions involved in the model project, or that at the very least were interested in hearing about them. Thus, it would seem, the actual connections between the model building project and government agencies were built through these technically oriented people and not directly with people high up the decision-making ladder. This was certainly the case with the Board of Governors, whose representatives in the project were the future director and staff members from the Division of Research and Statistics. Sherman Maisel, who was the expert in charge of non-business construction and became a Board Governor in 1965, was a Professor at UC Berkeley during the period he was involved in the Committee's model project.<sup>75</sup>

The participation of some members of the Council of Economic Advisers in the Committee's activities is particularly interesting given the Council's high standing and influence. Both Henry Wallich and David Lusher attended the 1959 Michigan conference, and Lusher became the expert in charge of the Government revenues and expenditures sector.<sup>76</sup> The Committee approached James Tobin and Walter W. Heller early on with a rather open invitation to discuss and see if the Council would be interested in the Committee's work, getting an enthusiastic response from both of them.<sup>77</sup> It would seem that a meeting took place on May 17, 1961 but unfortunately there is no further evidence about any other contacts with the CEA before 1964.

The Committee and the model project also established an interesting relationship with the Department of Commerce. Not only was the Department—as the producer of the national accounts—a major source of data, but a group of their officials was interested in obtaining help from the Committee in kick-starting its own econometric research group at the Department's Office of Business Economics (OBE).<sup>78</sup> The OBE had taken up and updated Klein's quarterly model (Klein 1964), and had the intention of doing further work on

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<sup>75</sup> The effect that someone like Maisel might have played in the establishment of macroeconometric modeling at the Board is an important question that should be looked into. His own account of his years at the Board strongly suggests that he played a central role in bringing forecasts into the FOMC's policymaking process (Maisel 1973). See also Acosta and Cherrier (forthcoming).

<sup>76</sup> Lusher worked with Louis Weiner on this sector and they participated in the two Dartmouth conferences. The chapter for Duesenberry *et al.* (1965) on this sector, however, was written by Albert Ando, Cary Brown, and Earl Adams, Jr. The Treasury helped Lusher in his work and Klein was glad they were showing interest in their work. Klein to Webbink, July 1962, SSRC2, box 151, folder 1721.

<sup>77</sup> See Gordon's memos of April 7 and April 19, 1961, as well as the minutes of the Committee's meeting of December 28, 1960, SSRC1, box 147, folder 810. Tobin had been initially considered as a candidate to take over the work on consumption for the model. It would seem that he was officially invited, and declined, but there is no further evidence on this in the Committee's records. See Klein's letter of invitation to collaborate on the model project, July 13, 1960, SSRC1, box 147, folder 810.

<sup>78</sup> Gordon to Webbink, August 28, 1961; Gordon to Webbink, October 16, 1961, SSRC2, box 151, folder 1721.

econometric policy analysis. Researchers at the Department wanted the Committee to help them guide their research agenda and find adequate personnel.<sup>79</sup> This is another clear example of the type of technically oriented people from government agencies that were attracted to the Committee's activities.

### **Data, estimation, and simulation**

Econometric models need data, and one as large, complex, and disaggregated as the Committee's needed a lot of data. The importance of data for this particular project had been foreseen since the 1959 Michigan conference and the government agencies that collaborated with the Committee's project played a key role in supplying it. Yet, not all the series that the model team needed existed at the time and so an important part of the project's work went into producing new data series, in particular for their preliminary work on the disaggregate, 30+ sector model. Thus, series of employment, wages, capital stock, GNP, and price deflators for each of these sectors were produced (Klein 1962, 40). Even if the existence and availability of data had not been an issue, the massive amount of data alone presented some important challenges for the team. In particular, to be able to estimate the complete model it would be necessary to have all the data series available in one place. This involved getting source data from all individual researchers and transferring it to punch cards or magnetic tape. An initial process of centralization of both data and preliminary estimates of individual equations took place at the University of Pennsylvania during 1962-1963. Similarly, once the project was handed over to the Brookings Institution, a similar central repository of data was created.<sup>80</sup>

The model, of course, had to be estimated as well. This represented an important challenge and Franklin Fisher played a key role in this aspect of the project. Every individual researcher could provide ordinary or even two-stage least squares estimates for their sectors, but this was a preliminary result since estimates of the parameters were likely to change once the model as a whole was estimated. The estimation of the model as a whole, however, was difficult given the high degree of interdependence and the extremely low amount of observations relative to the number of variables and lags.<sup>81</sup> Fisher worked on evaluating the most adequate estimation methods for such a system and in the implementation of a recursive-block strategy that allowed portions of the model to be estimated independently without sacrificing consistency. Finally, for the model to be useful for policy analysis—a key goal of the whole project—a computer program (SIMULATE) was

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<sup>79</sup> See the minutes of the meeting between the OBE team and the Committee, November 5, 1963, SSRC1, box 147, folder 811.

<sup>80</sup> See Klein (1962, 39) as well as the "Report on the econometric model project of the CES, SSRC," September 26, 1963, SSRC 1, box 147, folder 810.

<sup>81</sup> Duesenberry and Klein (1965) and Fisher (1965) explain in detail the challenges involved in the estimation of the model.

devised at the University of Wisconsin that could solve and simulate the model (Holt 1965). The program was built in parallel to the model, and thus Holt used previous, smaller models to test and improve the program during the 1961-1963 stage of the project.

## **5. Concluding remarks: Towards a history of empirical/applied macroeconomics**

The history of macroeconomics is usually told in terms of both schools of thought, key authors, texts, theories, ideas, and methodological dicta, and of the policy conclusions derived from these theories and ideas (e.g., Snowdon and Vane, 2005; De Vroey, 2016). This view of the history of macroeconomics, although enlightening and pedagogically useful, has traditionally downplayed the importance of applied work done by practicing macroeconomists, as well as the institutional context in which this work was carried out and the tools needed to do so. An interesting and somewhat paradoxical characteristic of this approach to the history of macroeconomics is that, while New Classical Macroeconomics is shown to emerge out of a criticism of large scale macroeconometric models, it is the IS-LM model that is put at the center of the narrative regarding the Keynesian developments in the 1940s and afterwards. Left unexplained, the lack of discussion of the type of work involved in the construction of large-scale macroeconometric models—to which Robert E. Lucas's (1976) criticism was pointed at—could be interpreted as implying that it was a straightforward extension of the theoretical, IS-LM-type models produced to interpret Keynes's message.<sup>82</sup>

A large-scale macroeconometric model, however, is a different type of object. It takes a different type of work to build and use, and it is meant to be used to answer much more specific quantitative questions. This type of models, together with the macroeconometric modeling practice that coevolved around it, brought about a new way of doing macroeconomics, and hence a new way of producing macroeconomic knowledge. In this paper, we have looked first at how the particular macroeconometric model of the Committee on Economic Stability was built, and second at how the practice of macroeconometric modeling emerged and evolved out of this project, spreading modestly, at first, among the community of academic economists and among a few government economists. The paper is a contribution to the understanding of what it took to build not only a large-scale macroeconometric model in the early 1960s and the motivations behind such an enterprise

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<sup>82</sup> As historians of econometrics have shown, however, the relationship between theoretical results and estimation was far from simple and involved debates over the uses of econometrics that shaped the development of the economics discipline. Neglecting the role played by large scale macroeconometric models in the decades following the postwar misrepresents the actual practices of macroeconomists at the time, which also contributes to overestimating the relative importance of theory in the evolution of macroeconomics. The history of econometrics and the history of macroeconomics have been usually written without making much emphasis on their interconnections and concomitant evolution. For accounts on the history of econometrics, see for example Morgan (1990), Epstein (1987), and Louçã (2007).

but also of the concrete and step-by-step process necessary for inventing a new way to do macroeconomics along the way.

We have shown that the construction of a large-scale macroeconometric model like the CES's is a difficult and complex process. It is difficult, because it must, on a daily basis, face and somehow find a way to overcome fundamental problems that are related not only to economic methodology, theory, and practice, but also to coordinative, administrative, funding, and institutional issues. This process is also complex, because of the very nature of the kind of objects and practices that are being built—a large-scale macroeconometric model and a practice like macroeconometric modeling—which require the creation of an institutional setting that coordinates and oversees the whole project. This institutional setting also provides the breeding grounds for a new scientific practice like macroeconometric modeling to be further developed and disseminated not among universities and classrooms, but among institutions that were involved in the formulation, recommendation, and, sometimes, execution of economic policies.

The development of this new way of producing macroeconomic knowledge through these novel tools and practices was marked by a change in the questions asked by the macroeconomists who were less interested in providing general, ideal, and theoretical answers to the economic problems of the postwar world than in providing concrete and useful solutions to problems related to the instability of the economic system. Although macroeconomists did certainly appreciate what had been learned with and after John Maynard Keynes's works, they were critical of the static nature of the Keynesian models and of the lack of relevance of contemporary theoretical business cycle models because these did not talk about actual business cycles or about the movement of relevant variables within a concrete and contemporary economic context. In fact, macroeconomists believed that quantitative work was necessary not only to understand the specificities of the situation of the US economy in the early 1960s but also that it would allow them to act upon the economic system and to provide concrete and well-informed policy recommendations based on rigorous bases. The idea that quantitative work would be more rigorous is related as well to the wider transformations that were occurring within economics and other social sciences about the necessity of adapting and constructing a powerful technical apparatus for the postwar sciences (see Morgan and Rutherford 1998; Isaac 2010). In particular, the project developed at the CES must be understood within a specific context of changes in which both the SSRC and other institutions such as the Carnegie Corporation, played a paramount role in promoting an economic and social science that was practical and useful (Hauptman 2016).

This specific episode in the history of macroeconomics—the construction of the large-scale CES macroeconometric model—leaves more open questions than it provides specific answers. In particular, further studies are necessary on the subsequent development of the

macroeconometric modeling project within the Brookings Institution, which would clarify the actual role played by this model in informing concrete policy decisions. Judging by the subsequent publications, it seems that the Brookings project did not become the infallible tool used to make policy recommendations, but that it became rather a sort of “laboratory” where economists would learn the practice of macroeconometric modeling and how to concretely build a large-scale macroeconometric model (Klein 1975). At the Brookings Institution, economists would have first-hand access to the teamwork and institutional dynamics of such an ambitious enterprise, to the methodological, theoretical, and practical difficulties of putting together a 30+ sector macroeconometric model, and of building in as much detail as was needed. More research on the individual trajectory of the participants in the Committee’s activities is necessary, for it might allow us to have a better understanding of the way in which networks of economists were built across different institutions, allowing for the dissemination and continuous adaptation and evolution of macroeconometric modeling. The study of these individual figures might also help us understand specific difficulties that do not appear in the archival material of the SSRC.

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