

Peddling macroeconometric modeling and quantitative policy analysis: the early years of the SSRC's Committee on Economic Stability, 1959-1963

Juan Acosta, Université de Lille¹

Erich Pinzón-Fuchs, Universidad de los Andes²

[Preliminary version of November 16, 2018. Please do not quote without permission]

1. Introduction

The Committee on Economic Stability of the Social Science Research Council (SSRC) was established in 1959 and played a key role in the construction of large-scale macroeconometric models during the 1960s and early 1970s. Using archival material from the SSRC, we discuss the two projects the Committee carried out during its first three years of existence:³ (i) the construction of a macroeconometric model (1960-1963) and (ii) the organization of a conference on quantitative policy analysis (1963). In line with the central theme of this special issue, we focus on the effect of the Committee's activities on public economic discourse and argue that, while the Committee did not participate directly in the policy debate, it did purposefully contribute to the growing importance of macroeconometric models in policy analysis. Thus, with its activities, the Committee helped usher in an age of quantified and model-based economic discourse that was not, however, exclusively technical but that recognized both the importance of the political character of the policy-making process and the limits of the economists' toolkit. In this sense, this story goes well in line with Robert Evans's (1999) argument that the use of economic models for the purpose of policymaking does not remove politics from economics, but that it is precisely in the models that the political element can be found.

The Committee was created as a joint venture of economists in academia, think tanks, and government institutions that were interested in better understanding economic instability in postwar United States. Although the available evidence suggests that advising government agencies on economic policy was not a clear priority when the creation of the Committee was discussed, a much stronger interest in influencing economic policymaking is clear in the two projects that the Committee carried out during its first years and that we discuss here. The Committee's model was designed to be useful for policy analysis and not just for forecasting: it was considerably more disaggregated than previous models and it included parameters

¹ Contact: jca.acostamacia@etudiant.univ-lille1.fr

² Contact: ea.pinzon@uniandes.edu.co

³ The archival material we use are related to the activities of the Committee between 1956 and 1965, and contain such as correspondence between the organizers and members of the Committee, memos, minutes, and papers presented in the seminars and conferences.

that represented actual policy instruments to accommodate the needs of policy makers.⁴ In addition, the project successfully engaged important government agencies that provided data series and whose officials also contributed with their expertise and in the construction of individual sectors of the model. The 1963 conference deliberately reinforced the Committee's role as a promoter of quantitative methods for policy analysis in the United States by showcasing the experiences of Japan, France, and the Netherlands—which relied in these methods to a much greater degree than the US. The Committee, however, did not offer ready-made solutions but rather a modest message that quantitative policy analysis had the potential to make economic policy better and that it was worth investing resources in the development of these tools.

In this sense, the creation of the Committee and of its macroeconomic model provides a rich historical episode to explore how economists can influence policy through their achievement of “professional authority,” their building of an “institutional position,” and their dissemination of a “cognitive infrastructure” (see Hirschman and Berman 2014). Indeed, this story allows us to ask about the local institutions and dynamics, and about the economic ideas and devices that economists develop in concrete contexts and that might end up influencing policy. First, the creation of the SSRC’s model opened the possibility for macroeconomists to intervene the economy in concrete ways, which more general theories could not provide. Second, the establishment of the Committee allowed macroeconomists to permeate government agencies and to institutionalize their position (ibid., 790-794). And third, the creation of the scientific practice of macroeconomic modeling established the conditions for policymakers to adopt both economists’ *style of reasoning* and their *policy devices* (ibid., 794-800).

The activities of the Committee offer a useful contrast with some of the institutions and people considered elsewhere in this special issue. The Committee's active and purposeful engagement with government institutions and officials contrasts with Robert Lucas's intentional distancing from the world of policymaking described in Goutsmedt *et al.* (this issue). Contrary to the privileged place of Walter Heller at the Council of Economic Advisers and the direct policy advice he provided to President Kennedy (see Cherrier, this issue), the Committee's activities involved the technical staff and mid-ranking officials, and were related to the creation of tools and to the promise of providing better ways to think about economic policy instead of giving concrete policy advice. Finally, the Committee's outsider position and its efforts in peddling the potential uses of the methods it promoted are in stark

⁴ Although the CES economists were not explicit about which were the “previous models” they referred to, and talked about the model’s “predecessors” (see for instance Duesenberry and Klein 1965, 6-8), it is not hard to imagine that the CES economists were mainly referring to Klein’s models I, II, III built at the Cowles Commission during the mid-1940s (see Klein 1950) and to the Klein-Goldberger model built at the University of Michigan (see Klein and Goldberger 1955).

contrast to the authority and legitimacy that the Central Planning Bureau and its model exerted, from the start, in Dutch policy making, given that the Bureau occupied a central position from the heart of the Dutch government (see Kayzel, this issue).

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 Robert A. Gordon (Berkeley), he and other economists associated with the SSRC who were interested in creating a committee on business cycle research proposed the conference to explore this possibility further (Gordon 1975, 31; 1959, 38). As the first column of Table 1 shows, participants came from academia, government agencies, and private institutions like the Brookings Institution and the National Bureau of Economic Research (NBER). Gordon opened the discussion by briefly introducing the topics that 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.⁵ Most notably, however, he began his remarks by noting the lack of relevance of the available theoretical models, the disconnection between theoretical work and the empirical and policy literature, and the number of concrete questions that needed answering given the new context characteristic of the postwar US economy.

Indeed, the postwar US economy recorded a behavior of relative stability with the following general characteristics (Gordon 1957, 115): (1) the absence of major economic depressions; (2) the maintenance of a level of full employment (or something close to it); (3) a "gratifying" rate of real growth (despite its decline during the two "mild post-war recessions" of 1948-49 and 1953-54); And (4) a wider distribution of "the fruits of prosperity" which had been more important "than ever before" (ibid.). Only "on the side of prices" and on the persistence of "minor cycles," however, was the "stabilization record weakest." Indeed, while the postwar US economy presented "consumers' price index that [stood in 1957] some 50 per cent above [its 1946] level," it continued to present recurrent minor fluctuations. Gordon considered that "it [was] now becoming a cliché to say that the economy is inherently more stable than it used to be," mainly due to the existence of "the built-in stabilizers, long-term capital budgeting by industry, deposit insurance, amortized mortgages, downward price and wage inflexibility, etc."⁶ Yet, according to Gordon (1957), little factual information existed that confirmed the stabilizing effects of any of these factors, and so little was known about the conditions that "are necessary to insure that a 'minor' recession remains minor."⁷ In this sense, the work of the Committee should provide both a better understanding of the

⁵ "Notes for the SSRC Conference on Economic Stability," SSRC1, box 145, folder 801.

⁶ "Proposal for a Committee on Economic Stability of the Social Science Research Council," box 150, folder 1717.

⁷ "Notes...," SSRC1, box 145, folder 801.

mechanisms of these minor cycles as well as a concrete and useful tool to mitigate these cycles.

Table 1: Participants in Committee activities⁸

	Michigan (1959)	Dartmouth 1 (1961)	Dartmouth 2 (1962)	1965 Volume	Conf. QPA (1963)
Number of participants	19	29	30	25	32
Participants with PhDs	16	21	20	21	-
Participants from academia	11	18	21	21	11
Participants from gov. agencies ⁹	5	8	7	4	13
Participants from private institutions	3	3	3	1	9

Source: Records of the SSRC.

Geoffrey Moore (NBER), Bert Hickman (Brookings Institution), and James Duesenberry (Harvard) also presented papers that looked in detail at the characteristics of the business 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 Institution) and Otto Eckstein (Joint Economic Committee)—had been specifically commissioned by the organizers of the conference and was the only paper that contained an econometric model.¹⁰ The core of the paper consisted on the various simulations, "policy experiments," that Duesenberry, Eckstein, and Fromm carried out to consider the effects of different fiscal policies and of the automatic stabilizers on economic instability. The model had several limitations that the authors willingly acknowledged—it only considered an economy in a recession and it did not consider monetary policy or price changes—but it showcased effectively the type of questions that could be investigated with such a model. As Lawrence R. Klein (Pennsylvania) later put it, the model played an important role in "the whetting of the appetites" for a large-scale macroeconomic model (Klein 1975, 13).

⁸ A couple of the participants had double affiliations and were thus counted twice. The information about the education of participants is not complete, so the number of PhDs presented is a lower bound. Given the amount of international participants for which we do not have adequate information, we do not report the number of PhDs for the 1963 conference.

⁹ This includes multilateral agencies and foreign government agencies.

¹⁰ "Stability and instability in the American economy," SSRC1, box 145, folder 801. A revised version of the paper appeared later in *Econometrica* with a different title: "A simulation of the United States economy in recession" (Duesenberry et al., 1960).

The summary of the discussion 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.¹¹ 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 (i) "facilitate the coordination of research," (ii) "help integrate current research methodologies,"¹² (iii) "facilitate the collection and publication of needed data, particularly by the Federal Government," and (iv) "serve as a channel of communication and a facilitating agency in the field of research on problems of economic instability." The first function was specifically geared towards helping researchers working on econometric models come together. Gordon highlighted the need to take stock of the research available in order to avoid duplication of work and to channel efforts into disaggregation. He hoped 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" (*ibid.*). Judging from the summary of the discussion at the conference it would seem that the idea to build a larger, more disaggregated model was explicitly considered, but this is not completely clear. Gordon (1959) is equally unclear in this respect.

It should also be noted that a fifth function of the committee, not reported in Gordon (1959) but discussed at the Michigan conference, was that of "providing information to policy-making agencies of the government." Specifically, the conference's discussion 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."¹³ However, and this might explain why this function did not appear in Gordon (1959), the discussion 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."¹⁴ Unfortunately, there is no further record of the specific points that were advanced against this function during the conference.

¹¹ Discussion summary, SSRC1, box 145, folder 801.

¹² While econometric modeling clearly occupied a central role, the "historical" approach of the NBER was seen as potentially useful complement. The discussion summary of the conference explicitly shows these approaches were seen as complementary rather than substitutes. As we mention in Acosta and Pinzón-Fuchs (2018), however, the NBER's approach ended up playing a minor role in the activities of the Committee.

¹³ Discussion summary, SSRC1, box 145, folder 801.

¹⁴ *Ibid.* Our emphasis.

The proposal for the establishment of the Committee on Economic Stability was accepted in September of 1959 and the initial members of the Committee were recruited in the following months (Gordon 1959, 39).¹⁵ Table 2 lists the Committee’s members during the early 1960s—the exclamation sign (!) denotes the chairman. As can be seen from their affiliations, most of the members of the Committee were academics. However, as Table 1 shows, the Committee’s activities had a broader reach and engaged more government officials in an effort to develop macroeconomic models and promote the use of quantitative policy analysis.

Table 2: Member of the Committee on Economic Stability, 1959-1964

Members CES	Affiliation	59-60	60-61	61-62	62-63	63-64
Klein, Lawrence R.	U. Pennsylvania	X	X	X	X	X
Duesenberry, James	Harvard University	X	X	X	X	X
Hickman, Bert	Brookings Institution	X	X	X	X!	X!
Gordon, R. A.	UC Berkeley	X!	X!	X!	X	X
Moore, Geoffrey	NBER	X	X	X	X	X
Lusher, David	CEA	X	X	X	X	X
Abramovitz, Moses	Stanford		X	X	X	X
Bronfenbrenner, Martin	U. Minnesota			X	X	X
Modigliani, Franco	MIT				X	X
Fox, Karl	Iowa State University				X	X

Source: Records of the SSRC.

3. The macroeconomic model of the CES

Planning for the construction of a large-scale macroeconomic model started in early 1960. The team of experts that would be in charge of each of the individual sectors of the model was almost complete by October of 1960 and funding was sought from the National Science Foundation (NSF).¹⁶ Although the NSF’s grant seems to have been awarded in June of 1961,¹⁷ preliminary work on the model began in February of that year. From 1961 to 1963 a team of more than 20 researchers led by Klein and Duesenberry worked on the model.¹⁸ Researchers

¹⁵ 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.

¹⁶ Herring to Riecken, October 4, 1960. SSRC2, box 151, folder 1721.

¹⁷ Fouraker to Klein, June 16, 1961. SSRC2, box 151, folder 1721.

¹⁸ It is noteworthy to point out that Klein did not attend the 1959 Michigan conference but he was invited to participate in the Committee soon afterwards. We ignore the reasons for his not having attended the conference given his stature as one of the main proponents of macroeconomic

worked mostly individually, meeting only a couple of times a year, most notably during two multi-week conferences at Dartmouth during the summers of 1961 and 1962 (Klein 1961; 1962). The model was handed over to the Brookings Institution for management and further development in September of 1963, thus becoming the Brookings model, and several volumes appeared afterwards describing its structure and simulation results—starting by Duesenberry *et al.* (1965), which reports mostly the work carried out during the Committee phase of the model. The model was a milestone in the practice of large-scale macroeconomic modeling due to its size, its technical innovations, and the way it was built. These aspects have been discussed elsewhere, but two important elements stand out in connection with the relationship that the model helped build between academic economists, private institutions, and government agencies.¹⁹ First, the model was conceived from the beginning to be useful for economic policy analysis, and second, the model drew extensively on both data and expertise from government agencies. Although it took a few more years of work on the model beyond its Committee phase to obtain the type of quantitative policy analysis that the project promised (Fromm and Taubman 1968), the project was successful in gaining the attention of government agencies and in building important connections between academia and government that paid out in the following years.

As we mentioned in the past section, there was some discussion around the potential role that the Committee could have in advising government agencies. And although we have not located the proposal presented to the NSF, the cover letter sent by the SSRC's president did not mention anything related to economic policy either.²⁰ It is possible that emphasizing the scientific aspect of the project was a strategic choice to appeal to the NSF, but it is in any case clear that building a model that could be useful for policy analysis was a central concern for the team working on the project from the start. The intention to "produce a system that [would] be jointly useful in forecasting and policy formation" was explicitly stated in the meeting where the construction of the model was decided,²¹ and it was confirmed a year later in the first preliminary meeting between the team of researchers in charge of the individual sectors of the model.²² In terms of the structure of the model, this concern for policy usefulness meant an overall higher degree of disaggregation and the explicit inclusion of parameters representing actual policy instruments. These parameters would allow the

modeling, but we conjecture it might be related to his previous experience at the University of Michigan, where he had been accused of sympathising with communism and driven out into exile. Pinzón-Fuchs (2017, ch. 2) describes this episode in detail.

¹⁹ See in particular Bodkin *et al.* (1990). Acosta and Pinzón-Fuchs (2018) describes in detail the approach followed in the construction of the model and its importance for the consolidation of the practice of macroeconomic modeling during the 1960s.

²⁰ Herring to Riecken, October 4, 1960. SSRC2, box 151, folder 1721.

²¹ Meeting minutes, Feb 24, 1960, SSRC1, box 147, folder 810.

²² Meeting minutes, Feb 3, 1961, SSRC1, box 147, folder 810.

model to produce quantitative estimates of the effectiveness of any specific policy instrument considered.²³

The relationship with government agencies was crucial due to the amount and variety of data series that were used to build the model, some of which were specially put together for the project. Officials from the Department of Commerce's Office of Business Economics (OBE) seem to have been particularly helpful in getting the needed data series.²⁴ The detailed specification of each of the individual sectors was left to experts whose models would then be combined into a full model of the whole economy. As shown in columns 2-4 of table 1, some of the experts involved in the discussion and construction of the model also came from government agencies. Columns 2 and 3 refer to the participants of the two Dartmouth conferences where the team of researchers involved in the project, as well as occasional guests, met to discuss their progress and worked on turning the parts of the model into a consistent whole; column 4 refers to the researchers that authored the individual sector models included in Duesenberry *et al.* (1965). Although our data still has some gaps, we found that at least four (out of eight) of the government-affiliated participants at Dartmouth 1, at least three (out of seven) of the participants at Dartmouth 2, and three (out of four) of the government-affiliated contributors to Duesenberry *et al.* (1965) held Ph.D. degrees. This information is not a perfect indicator, but it does suggest that the connections established during the model project involved the technically oriented government officials, that is, staff members who could understand the technical discussions or that at the very least were interested in hearing about them.

Even if the connections between the model building project and government agencies were not built directly with people high up in the decision-making ladder, they did have a long lasting effect by helping some of these agencies establish macroeconomic modeling research agendas. This is particularly clear in the case of the Board of Governors. The Committee initially contacted the Board looking for funding but the Board's response was lukewarm. They would let Daniel Brill—who was initially in charge of building the model of the financial sector—and other members of the staff participate in the project but were hesitant to fund it.²⁵ Paul Webbink, who handled the administrative affairs of the Committee at the SSRC, reported that

²³ For comparison, a model focused exclusively on forecasting the GDP or the level of prices need not worry with a disaggregated, detailed specification of the model as long as the model estimated produced good estimates.

²⁴ "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).

²⁵ 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>.

[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.*²⁶

The request for Board funds was eventually dropped. The Committee was confident they would get the funding from the NSF and considered that following up on the request for funds from the Board would take up too much valuable time from Klein.²⁷ Even so, the proposed strategy is noteworthy and proved to be true. The idea that the model project was in the Board's best interest and, even more so, unavoidable, fits in well with the attention given to the policy usefulness of the model and with the agenda pursued with the 1963 conference. And the Board did in fact develop a macroeconomic model afterwards, in collaboration with the Committee's Subcommittee on Monetary Research. The Board's model was another joint project led by Frank de Leeuw (DRS), Franco Modigliani (MIT), and Albert Ando (Pennsylvania). Brill, who became director of the DRS in 1963, established a modeling project led by De Leeuw—who had replaced him as the final responsible for the financial sector in the Committee's project (De Leeuw 1965). This project merged in 1966 with a project at MIT led by Modigliani and Ando, and that became the Federal Reserve Board-MIT-Pennsylvania model project (1966-1970). The project was fully funded by the Board via the Committee's Subcommittee on Monetary Research, an initiative that had brought the DRS staff and academic monetary economists together since 1964.²⁸

A less well documented, though equally interesting relationship, emerged between the Committee and the Department of Commerce's Office of Business Economics. The Department was a major source of data as it produced the national accounts, but a group of their officials was also interested in obtaining help from the Committee in kick-starting its own econometric research group at the OBE.²⁹ The OBE had taken up and updated Klein's quarterly model (Klein 1964), and they had the intention of doing further work on econometric policy analysis. Researchers at the Department wanted the Committee to help

²⁶ Webbink to Gordon, Oct 05, 1960, SSRC2, box 151, folder 1721. Our emphasis.

²⁷ *Op. Cit.* See also Gordon to Webbink, Oct 10, 1960, SSRC2, box 151, folder 1721.

²⁸ For details on this model see Acosta and Rubin (2018) and Backhouse and Cherrier (2018) For a discussion of what the model meant for the Board's relationship with economists see Acosta and Cherrier (2018).

²⁹ Gordon to Webbink, August 28, 1961; Gordon to Webbink, October 16, 1961, SSRC2, box 151, folder 1721.

them guide their research agenda and find adequate personnel.³⁰ There is some evidence in the SSRC records that shows that the Committee advised the OBE at least until 1965, but the details are unclear.³¹ In any case, the first version of the OBE's model was ready by 1966, which later became the BEA model, when the OBE changed its name to the Bureau of Economic Analysis (Bodkin *et al.* 1991, 120).

The CEA also showed some interest in the model project and got involved in its construction. 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.³² The Committee approached James Tobin and Walter W. Heller early on with a rather open invitation to discuss and see if the Council of Economic Advisers would be interested in the Committee's work, getting an enthusiastic response from both of them.³³ It would seem that a meeting took place on May 17 of 1961 but we have not found any further evidence of collaboration. Lusher, a member of the Committee as well, dropped out of the model project when he got sick and his work was taken up by Albert Ando, Cary Brown, and Earl Adams.

It would be impossible to say for sure whether the Board or the OBE would have carried out their model projects if the Committee's own project had not existed. But the Committee's project certainly helped move things along, particularly because it showed officials from these and other agencies how a large-scale model could be built and what was needed to do so. The Committee's model project helped diffuse macroeconomic modeling by example; the conference on quantitative policy analysis organized by the Committee in 1963 reinforced the message, but it did so in a much more direct and open way.

4. Promoting quantitative policy analysis in the United States

The Committee organized an international conference in August of 1963 that supplemented the interest that the macroeconomic model project had provoked in some government agencies. It was explicitly aimed at giving quantitative policy analysis more visibility among US economists, including government officials. The planning of the conference was in charge

³⁰ See the minutes of the meeting between the OBE team and the Committee, November 5, 1963, SSRC1, box 147, folder 811.

³¹ See for example Klein to CES members, March 29, 1965. SSRC1, box 147, folder 812.

³² Lusher worked with Louis Weiner on this sector and they participated in the two Dartmouth conferences. However, the chapter for Duesenberry *et al.* (1965) on this sector 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.

³³ 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.

of Bert Hickman (Brookings Institution), Charles Holt (Wisconsin), Karl Fox (Iowa State), and Erik Thorbecke (Iowa State). Concrete plans started taking shape in late 1962 and Hickman suggested that it might be a good idea to contact the organizers of an NBER conference on the same topic, i.e. planning.³⁴ It is unclear whether the organizers actually contacted the NBER but the evidence suggests that, if they did, nothing came out of this. The NBER did carry out a conference on "National Economic Planning" in November of 1964 (Milikan 1967), but none of the Committee members seem to have participated in it and, similarly, nobody from the NBER participated in the Committee's conference.

The conference had a clear goal from the start: to promote quantitative policy analysis in the United States by showcasing the experience of other countries. Indeed, early in 1963 Hickman emphasized this objective:

I believe that we should be careful not to lose sight of the educational function of the conference, both for the participants and profession at large. There should be heavy emphasis on milking the experience of the foreign economists who have been working with the tools and the associated political and administrative problems. A volume of collected papers on country experience[s] with "Quantitative Planning of Economic Policy" on the national level, rounded out with a general report on the proceedings of the conference and possibly supplemented by other papers, should attract wide attention among US economists.³⁵

The initial version of the conference proposal, sent later to the Ford Foundation, emphasized this educational purpose. The proposal noted the experience accumulated in other countries "concerning the quantitative formulation and planning of economic policy on a national and regional level" and explicitly stated that the purpose was to "acquaint American economists with this body of foreign experience and to stimulate research on the application of quantitative tools to policy problems in the United States." Papers would be commissioned from "persons actively at work in the field and [would] not be burdensome to prepare" so that the conference volume could be published soon afterwards.³⁶

The conference would last five days and bring together up to 40 economists to discuss about the techniques of quantitative policy analysis and the experience of countries that had led the path in their use. In the proposed agenda mornings would be occupied with technical papers dealing with the theory of economic policy, and estimation and specification issues. Friday also included two papers about "the political-economic process, dealing with problems of communications between economic advisers and policy makers, interpretation of results by economists to policy makers, administration and implementation of the policy

³⁴ 19621204 Hickman to Fox. SSRC2, box 151, folder 1722.

³⁵ 19630102 Hickman to Fox. SSRC2, box 151, folder 1722.

³⁶ "Proposal for a conference on 'Quantitative Planning of Economic Policy' under the sponsorship of the SSRC Committee on Economic Stability." January 18, 1963. SSRC1, box 147, folder 811.

process, etc." Technical discussions would be supplemented with afternoon discussions based on the relevant parts of papers that presented the experiences of the Netherlands, Norway, France, Italy, and Japan with "quantitative planning and [the] implementation of economic policy." The structure of these papers would follow the proposed agenda of the conference so as to facilitate comparisons and would be "solicited from the chiefs of the relevant government bureaus or close associates. These papers would not be delivered at the conference but would be prepared as background papers and circulated in advance of the conference."³⁷

The proposal was also explicit on the pedagogical objective in regards to the selection of American participants who were "selected in conformity with the basic purpose of fostering interest in research on quantitative analysis of economic problems." The organizers not only invited young and senior scholars, but they also sought to have "a wide coverage of institutions." In addition, "[e]conomists with basic policy interest but comparatively little econometric training [were] asked to participate, and conversely, econometricians who had not previously done research on policy matters [were] also [...] invited."³⁸

The Ford Foundation's initial response to the grant application was negative. Despite the apparently balanced agenda of the conference, which included both technical and real-world implementation discussions, the Foundation considered that there was too much emphasis on the technical side. As Hickman reported to the rest of the planning committee:

The Ford Foundation is cool to the conference as we planned it—cool to the point of refusing to finance it. Their principal objection is that too much emphasis is planned on techniques and too little on the actual contribution of quantitative methods to economic policy. Does the advice of the technicians get accepted? Is the advice straight from the models or does the judgment of the planning chief and his staff enter heavily into the final recommendations? What role do political factors play in setting constraints on admissible goals? On crucial variables like the money wages? What means are used to implement the policies?³⁹

The proposal was modified to increase the emphasis on the pragmatic problems and was finally approved by the Ford Foundation.⁴⁰ The new proposal had similar language, insisting on the fact that "American economists and policymakers are largely uninformed about these important developments" made in France, the Netherlands, Norway, and Japan, and that "[a] critical appraisal of the contribution of quantitative techniques to the planning and implementation of economic policy in these countries could significantly affect the future

³⁷ Ibid.

³⁸ Ibid.

³⁹ 19630304 Hickman - Fox. Holt. Thorbecke. SSRC2, box 151, folder 1722.

⁴⁰ 19630412 Hickman - Fox. Holt. Thorbecke. SSRC2, box 151, folder 1722.

direction of economic research and economic policy in the United States."⁴¹ The new proposal also explicitly emphasised the effort made to communicate these techniques to a wider public, insisting that technical papers should be "expository in nature and [confine] any difficult mathematical material to appendixes."⁴² In addition, the new agenda explicitly incorporated the questions suggested by the Ford Foundation, which would be treated in the papers prepared on the experiences of the above mentioned countries. Policy papers now appeared explicitly in the agenda and occupied the last three full days of the conference, with the technical papers confined to the first two days. The country experiences to be discussed were cut to three: France, the Netherlands, and Norway instead of Japan. A couple of months later, however, this last choice was reversed and Japan was included again.⁴³

It is unclear how the final choice of the authors of the policy papers was made, but Hickman does point out that he got some suggestions from Jan Tinbergen—who could not personally attend the conference but was reportedly very enthusiastic about it.⁴⁴ In any case, the educational purpose of the conference was also made explicit to the authors of the policy papers,⁴⁵ and the questions included in the conference's instructions clearly echoed those proposed by the Ford Foundation:

How is the economic policy problem identified and defined? How or to what extent is the policy decision problem formulated in quantitative terms? How are the specific objectives or targets of economic policy determined? How are the relationships between proposed policy actions and desired economic outcomes estimated and how successful are the models in forecasting economic activity and the influence of policy actions on economic activity? If a formal quantitative model is used, how is the mathematical solution of the decision problem obtained? How do the results of the quantitative economic analysis contribute to the political decision process?⁴⁶

The conference took place in August 19-24, 1963 at the Brookings Institution. As shown in Table 1, in the end there were 32 participants and those associated with government agencies outnumbered academic economists: 13 participants were affiliated to government agencies, 11 to academic institutions, and 9 to private organizations; 10 out of 23 US

⁴¹ "Agenda for a Conference on 'Quantitative planning of economic policy' under the sponsorship of the SSRC Committee on Economic Stability." March 15, 1963. SSRC2, box 151, folder 1722.

⁴² Ibid.

⁴³ See the May 1, 1963 version of the conference agenda. SSRC1, box 147, folder 811.

⁴⁴ 19630129 Hickman - Fox. Holt. Thorbecke. SSRC2, box 151, folder 1722.

⁴⁵ See for example Hickman to Bauchet, June 18, 1963. SSRC2, box 151, folder 1722.

⁴⁶ 19630501 "Note to authors of cuntry papers." SSRC1, box 147, folder 811. These questions seem to follow Charles Holt's steps for quantitative policy formulation. As reported by Hickman these are: "identification of the problem; discovering the relevant relationships; specifying the objectives; quantitative formulation; mathematical solution; interpretation of the results for policy makers; and administration and control of either the research process or the policy process or both — this was not clear to me." See Hickman to Fox, January 2, 1963, SSRC2, box 151, folder 1722.

participants were associated with government agencies. In particular, there were officials from the Board of Governors of the Federal Reserve, the Council of Economic Advisers (CEA), the Department of Commerce's Office of Business Economics (OBE), the Department of Labor's Bureau of Labor Statistics, the Treasury's Office of Financial Analysis, and the Agency for International Development. With the exception of the Joint Economic Committee and the Bureau of the Budget, the organizers were successful in getting officials from the agencies they originally wanted, although they did not get some of the high-profile officials they had considered, such as Gardner Ackley (CEA) and George Jaszi (OBE). It is also noteworthy that none of the Harvard and MIT economists associated with the Committee such as Franco Modigliani, Edwin Kuh, Duesenberry, and Eckstein attended the meeting. Other major names like James Tobin, Arthur Okun, and Kenneth Arrow were considered initially but did not participate in the conference.⁴⁷

The presentations of the conference and the published volume (Hickman 1965a) provided a broad introduction to the technical aspects of quantitative policy analysis as well as a presentation of the experiences of the Netherlands, France, and Japan. The conference showed even more clearly why proponents of the use of quantitative tools in economic policy considered these tools valuable. A short note published as a Brookings Research Report summarized this well:

The techniques of policy planning provide a *rigorous and systematic* method of exploring the impact on the economy of specific governmental actions. Their purpose is to supply the policy maker with a more *scientific* basis for choosing among alternative economic policies than is given by the rough estimates or intuition frequently underlying policy decisions.⁴⁸

The volume conveyed a general support for the use of quantitative tools and also showed that there were different approaches available. The chosen countries illustrated this well: Japan and France focused on long-term planning and the Netherlands focused on short-term planning. The Committee's macroeconomic model was closest in type to the work done in the Netherlands, but there too, it was possible to choose between using previously specified values for the target variables (Tinbergen's approach) or deriving the optimal values from a maximization program using a decision-maker's preference function (Theil's approach) (Hickman 1965b; Theil 1965). It was clear that in order to apply either of these approaches in a rigorous way, "a complete econometric model must be built in which all relevant target and instrument variables are included and all coefficients are numerically estimated"

⁴⁷ See the initial list of potential candidates: Hickman to Fox et al., April 12, 1963. SSRC2, box 151, folder 1722.

⁴⁸ "The uses of quantitative economic planning." SSRC2, box 151, folder 1722. Our emphasis. The note was not signed, but it could well have been authored by Hickman—who wrote the introduction to the conference volume (Hickman 1965b)—or any of the other members of the planning team.

(Hickman 1965b, 6). The kind of econometric model used in the Dutch case was studied in van den Beld's (1965) and Fox and Thorbecke's (1965) papers in the volume. And yet, an important message that the book wanted to convey was that, even if it was still under construction and preliminary, a project already existed in the United States that tried to build such an econometric model with the purpose of doing quantitative policy analysis in a rigorous way. This project was, of course, the Committee's macroeconomic model that had now been passed on to the Brookings Institution.

Another important point emphasised both during the conference and in the book, was the political character of quantitative economic policy analysis. More specifically, Hickman (1965b, 9) reminded the readers that the "determination of desired values of targets and instruments [...] and the weights attached to them" was conditioned to higher order political aims. Indeed, Etienne S. Kirschen and Lucien Morissens (1965) described how higher order political aims such as full employment, price stability, improvement in the balance of payments, expansion of production, or improvement in the allocation of factors of production, had affected the formulation of different targets in nine Western countries in the postwar era. Kirschen and Morissens (1965, 133) insisted that the choice of these political aims depended on the preferences of the political parties, administrators, and interest groups. As Hickman (1965b, 9) put it, "these last findings serve[d] as a healthy reminder to the economist" that quantitative policy analysis was not only a technical endeavour, but that it was "essentially a political problem."

In this sense, the message that the Committee was trying to pass on was not that quantitative tools were the ultimate and infallible way to make policy analysis. Rather, they conceived these tools as a way to help policymakers make decisions, but they understood pretty well that the decision-making process could not be mechanistic or technical, and that the political dimension was, in the end, the most important dimension in this process. In addition, the conference participants recognized that the contributions of quantitative policy analysis, while promising, were still modest and should be further developed.⁴⁹ In particular, Holt (1965) called attention to the important difference between simply using quantitative tools as a way to make policy decisions and the quantitative decision analysis approach. The use of quantitative methods for policy analysis, on the one hand, helped policy makers achieve a "coherent and timely set of economic policies" at all the levels of the decision process using economists' "unconditional and conditional forecasts [...] of the outcomes of alternative courses of action" (Holt 1965, 254). Yet, however important economists' contributions were, these were scattered within a complex process that was sometimes "reduced to the art of finding legislation that stands a chance of passage in Congress" (253) and that was dispersed among the political power of "various agencies, committees, and chairmen, as well as the

⁴⁹ "The uses of quantitative economic planning." SSRC2, box 151, folder 1722.

Senate, the House, and the President” (254). The quantitative decision analysis approach, on the other hand, consisted in posing the decision making problem in terms of the maximization of the “accomplishment of a welfare function subject to the constraint of economic relationships” (255). This provided the economists with a “framework for thinking about the decision process that is less simplified,” and that “implies, not a change in the process itself, but a different way of relating the work of economists to it” (254).

Yet Holt was careful in his formulation of the advantages of this approach as well as of the “limited knowledge [of economists] and the genuine differences between objectives of various groups, the conclusions of the formal analysis will be, not a single ‘best’ action alternative, but rather several ‘good’ alternatives depending upon the assumptions that are made,” which clearly left the door open for the importance of the political aspects in the decision making process (Holt 1965, 255). Holt also made clear that they were not “visualizing a benevolent dictatorial technocracy run by professional economists” but a way to make economists “better able to offer sound advice on a professional level to politically responsible decision-makers” (255). The Brookings Research Report quoted above summarized well not only Holt’s idea on the limits of an exclusively technical approach, but also reinforced the SSRC’s pluridisciplinary approach to the process of policy decision making:

Of equal or perhaps greater importance is the less technical problem of relating the professional advice of the economist to the political decision process so that quantitative analysis can be of maximum effectiveness and use to the responsible decision makers. Fundamental to this aim is a greater understanding of how economic policy decisions are actually made. These are problems which cannot be solved with the economist’s toolkit alone. The skills, techniques, and theories of other social science disciplines must be utilized more fully before the promise implicit in quantitative decision analysis can be realized.⁵⁰

To be sure, the conference did not propose a program for making policy decisions that was blindly based on quantitative methods. Rather, the organizers of the conference were willing to recognize that this process was conditioned by higher order political aims, that it was complex, and that the economists’ toolkit alone was not sufficient to provide sound evaluation of alternative policies.

5. Conclusions

⁵⁰ “The uses of quantitative economic planning.” SSRC2, box 151, folder 1722.

The two activities we have discussed, the construction of a large-scale macroeconomic model and the organization of the 1963 conference on quantitative policy analysis, were certainly important in furthering the Committee's guiding objective of channeling efforts into the understanding of instability in the United States. The model project, in particular, was at the forefront of macroeconomics and played an important role in the consolidation of macroeconomic modeling. But besides the scientific contributions of the Committee in these years, its activities helped promote quantitative policy analysis in the United States. The model did so by direct collaboration with government officials, who provided much needed data and expertise. The type of highly disaggregated model that was conceived and the concern for its policy usefulness made collaboration with government officials unavoidable, but the project also built important communication bridges between the Committee and government institutions that helped other model projects come into being at the Board of Governors and the OBE.

The 1963 conference further emphasized the usefulness of quantitative policy analysis. Having a macroeconomic model was the first step—and the Committee's model was certainly going to be bigger and “better” than anything available until then—but it was also necessary to rethink the approach to economic policy. The conference sought to show economists in the United States how a deeper involvement of quantitative analysis could help make economic policy better, more rigorous. Although countries that had taken the lead in the use of quantitative policy analysis had obtained only modest results so far, the conference showcased these various experiences and approaches to try to convince US economists that this was a path worth following. It was also clear that the politics of economic policy was not going to disappear and that politics was ultimately the defining element of each policy. However, once a policy was formulated, quantitative analysis could help policymakers carry it out more effectively.

References

Archival material:

- SSRC1 Rockefeller Archive Center, Records of the SSRC, Record Group 1, Accession 1, Series 1.
- SSRC2 Rockefeller Archive Center, Records of the SSRC, Record Group 2, Accession 2, Series 1.

Other references:

Acosta, Juan and Beatrice Cherrier. 2018. Economists and the transformation of economic analysis at the Federal Reserve, 1961-1975. Presented at the 2018 HISRECO conference, Cergy, France, October 12-13.

Acosta, Juan and Erich Pinzón-Fuchs. 2018. Macroeconometric modeling and the SSRC's Committee on Economic Stability, 1959-1963. Working Paper No. 2018-08, Center for the History of Political Economy, Duke University.

Acosta, Juan and Goulven Rubin. 2018. Bank agency in early large-scale macroeconometric models of the 1960s. Forthcoming in a special issue of *History of Political Economy* on the history of macroeconometric modeling.

Backhouse, Roger and Beatrice Cherrier. 2018. The ordinary business of macroeconometric modelling: working on the Fed-MIT-Penn model (1964-1974). Forthcoming in a special issue of *History of Political Economy* on the history of macroeconometric modeling.

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

Duesenberry, James S., Otto Eckstein, and Gary Fromm. 1960. A Simulation of the United States Economy in Recession. *Econometrica*, 28(4): 749-809.

Duesenberry, James S., Gary Fromm, Lawrence R. Klein, and Edwin Kuh (eds.). 1965. *The Brookings Quarterly Econometric Model of the United States*. Chicago: Rand McNally & Company.

Duesenberry, James S. & Lawrence R. Klein. 1965. Introduction: The research strategy and its application. In James S. Duesenberry *et al.* (eds.). *The Brookings Quarterly Econometric Model of the United States*. Chicago: Rand McNally & Company, pp. 3-32.

Evans, Robert. 1999. *Macroeconomic Forecasting: A Sociological Appraisal*. London: Routledge.

Fisher, Franklin. 1965. Dynamic structure and estimation in economy-wide econometric models. In James S. Duesenberry *et al.* (eds.). *The Brookings Quarterly Econometric Model of the United States*. Chicago: Rand McNally & Company, pp. 589--635.

Fox, Karl A. and Erick Thorbecke. 1965. Specification of Structures and Data Requirements in Economic Policy Models. In *Quantitative Planning of Economic Policy* by Bert G. Hickmann (ed.), 18-42. Washington D.C.: The Brookings Institution.

Fromm, Gary & Lawrence R. Klein. 1965. The complete model: A first approximation. In James S. Duesenberry *et al.* (eds.). *The Brookings Quarterly Econometric Model of the United States*. Chicago: Rand McNally & Company, pp. 681-738.

Fromm, Gary and Paul Taubman. 1968. *Policy simulations with an econometric model*. Washington DC: Brookings Institution.

Gordon, Robert A. 1957. Stabilization Policy and the Study of Business Cycles. *The American Economic Review*, 47 (2): 115-126.

Gordon, Robert A. 1975. Discussion. In *The Brookings Model: Perspective and Recent Developments*, North-Holland Publishing Company, Amsterdam, edited by Gary Fromm and Lawrence R. Klein, 31-34. North-Holland.

Hickman, Bert G. (ed.). 1965a. *Quantitative Planning of Economic Policy*. Washington D.C.: The Brookings Institution.

Hickman, Bert G. 1965b. Introduction. In *Quantitative Planning of Economic Policy* by Bert G. Hickmann (ed.), 1-17. Washington D.C.: The Brookings Institution.

Hirschman, Daniel and Elizabeth Popp Berman. 2014. Do economists make policies? On the political effect of economics. *Socio-Economic Review*, 12: 779-811.

Holt, Charles C. 1965. Validation and Application of Macroeconomic Models Using Computer Simulation. In *The Brookings Quarterly Econometric Model of the United States*, edited by James Duesenberry, Gary Fromm, Lawrence Klein, and Edwin Kuh, 637-650. Chicago, Ill: Rand-McNally.

Kirschen, Etienne S. and Lucien Morissens. 1965. The Objectives and Instruments of Economic Policy. In *Quantitative Planning of Economic Policy* by Bert G. Hickmann (ed.), 111-133. Washington D.C.: The Brookings Institution.

Klein, Lawrence R. 1950. *Economic Fluctuations in the United States, 1921-1941*. New York: John Wiley & Sons, Inc.

Klein, Lawrence R. and Arthur S. Goldberger. 1955. *An Econometric Model of the United States, 1929-1952*. Amsterdam: North-Holland.

Klein, Lawrence R. 1961. The Dartmouth conference on an econometric model of the United States. *ITEMS* 15 (3): 34–36.

Klein, Lawrence R. 1962. The Second Summer Conference on an Econometric Model of the United States: Summary Report. *ITEMS* 16 (4): 37–40.

Klein, Lawrence R. 1964. A Postwar Quarterly Model: Description and Applications. In *Models of Income Determination*, edited by Conference on Research in Income and Wealth, 11–57. Princeton University Press.

Klein, Lawrence R. 1975. Research contributions of the SSRC-Brookings econometric model project—A decade in review. In *The Brookings Model: Perspective and Recent Developments*, North-Holland Publishing Company, Amsterdam, edited by Gary Fromm and Lawrence Klein, 13–29. North-Holland.

Milikan, Max (ed.). 1967. *National Economic Planning*. New York: National Bureau of Economic Research.

Pinzón-Fuchs, Erich. 2017. *Economics as a 'tooled' discipline: Lawrence R. Klein and the making of macroeconomic modeling, 1939-1959*. Ph.D. dissertation, Université Paris 1 Panthéon-Sorbonne.

Theil, Henri. 1965. Linear Decision Rules for Macrodynamics Policy. In *Quantitative Planning of Economic Policy* by Bert G. Hickmann (ed.), 18-42. Washington D.C.: The Brookings Institution.

Van den Beld, C. A. 1965. Short-Term Planning Experience in The Netherlands. In *Quantitative Planning of Economic Policy* by Bert G. Hickmann (ed.), 134-162. Washington D.C.: The Brookings Institution.