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WASSILY LEONTIEF'S RESEARCH PROGRAM:  
SCIENCE, BELIEFS, INSTITUTIONS

(REVISED)

VINCENT CARRET

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CENTER FOR THE  
HISTORY OF POLITICAL ECONOMY  
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# Wassily Leontief's Research Program: Science, Beliefs, Institutions

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## Abstract

Wassily Leontief met with decades of success for the development of input-output analysis, and yet he remained a staunch critic of the economics profession throughout his life. To understand his success, its limits, and the origins of his discontent, I separate the scientific activities of input-output from the system of belief built around it, and from the institutions set up to advance this research program. This leads to considering the interaction of Leontief's research program with other research programs through these three poles: the scientific debate, the collision of belief systems about the world, and an institutional fight for funds and researchers. The end result is a picture of how Leontief managed to build a successful research program where the science led to beliefs about the world that were able to justify building institutions promoting input-output, in an environment of competition and cooperation.

Keywords: Wassily Leontief, input-output, ideas, beliefs, institutions, competition, cooperation

JEL Codes: B31, B41, L52, O21, P11

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## I. Beliefs, Institutions, and the Ivory Tower

In 1982, Wassily Leontief gave a disenchanted account of the progress of economics, bemoaning the state of his profession and what he viewed as its lack of progress: “Year after year economic theorists continue to produce scores of mathematical models and to explore in great detail their formal properties; and the econometricians fit algebraic functions of all possible shapes to essentially the same sets of data without being able to advance, in any perceptible way, a systematic understanding of the structure and the operations of a real economic system” (Leontief, 1982: 107). This quotation is typical of Wassily Leontief, in that it exhibits a dismissive tone both towards abstract theoretical models and the contemporary practice of econometricians. Examples abound as early as the 1930s of his railing against the “empty boxes of economic theory” (Leontief, 1936: 105; 1948: 390; 1951: 15; 1953: 4-5) his fulminations against aimless collections of data (Leontief, 1949a: 212; 1953: 5) and his damning of “indirect” statistical inference and its complex procedures (Leontief, 1953: 5-7; 1971: 2-3). Leontief became even more vocal in the 1970s, starting with a scathing indictment of economists during his Presidential Address in front of the American Economic Association (Leontief, 1971). A few years after this address, he translated his statements into actions by leaving Harvard after more than forty years spent in Cambridge, citing a “lack of moral support” for his work (Meislin, 1975). Why was Leontief so disappointed by economics?

An internalist history cannot answer this question, if only because it is not raised by Leontief’s work taken in isolation. Most of Leontief’s disappointment lay in the organization of “Academic Economics” itself, the title he used for his attack on the economic profession published in *Science*. He argued indeed that this situation was “likely to be maintained as long as tenured members of leading economics departments continue to exercise tight control over the

training, promotion, and research activities of their younger faculty members, and by means of peer review, of the senior members as well” (Leontief, 1982: 107). It is not clear whether Leontief suggested that economics would fare better without peer review, but his harangue was the expression that, beyond scientific debates, the institutional organization of economics was shaping his assessment of his field. To understand Leontief’s attacks against economics and economists, we need to consider the relationship between scientific inquiry and the institutional organization of social science.

How do institutions interact with the pursuit of knowledge? Universities, such as the Massachusetts Institute of Technology or Stanford University, have been instrumental in providing the conditions of development and funding for postwar economics (Weintraub, 2014; Cherrier and Saïdi, 2020). Funding is often made possible by the construction of a nexus between a university and a funder, private or public, such as the National Science Foundation (Solovey, 2020). Professional societies, such as the American Economic Association or the European History of Economic Thought also shape the direction and definition of science, through their structures and their governance (Hoover and Svorenčik, forthcoming; Desmarais-Tremblay and Svorenčik, 2021). Journals, their editors and the refereeing process, guard the status of science, enforce the boundaries of fields, and protect the claims to priority or fame (Düppe and Weintraub, 2014a; Bjerkholt, 1995).

Institutions shape the conduct of science both through the targeted funding of certain research programs, and as an enforcement mechanism keeping in check the boundaries of programs and the behavior of participants. But science and scientists also have a role in shaping their institutions. This role manifests in the construction of belief systems that shape and legitimize the construction and development of institutions. By “belief system,” I mean that in order to

defend a research program, a number of results are highlighted, marketed, and sold to potential funders. This process can take different forms, from selling predictions, as was the case of the Harvard Barometer (Friedman, 2013), to claiming a central importance of the research program for the conduct of a particular policy, as Leontief did with planning policies.

Abstracting from the daily routine of writing, teaching, researching, consulting and communicating, we see how institutions rely on belief systems generated by a research program, and how these institutions promote the scientific inquiry at the heart of this program. The borders between scientific research, beliefs, and institutions are porous and not set in stone. But economists do divide their time between different activities, in the office or the laboratory, in committees and advisory bodies, or as editors and fundraisers. The functionally different nature of these activities should be noted, together with their interdependence: Leontief was incredibly successful as a scientist, an institution-builder, and an adviser, and these activities had real consequences on the development of input-output analysis.

This description of a research program as divided into three interrelated elements is a heuristic tool to examine the rich historical record, to organize the archival evidence in a narrative making sense of the successes and failures of a research program. By separating ideas, beliefs and institutions, and focusing on the relationship between the three, we obtain a more comprehensive picture of the way in which Leontief's research program was built and evolved, and why it was both successful and contested. These three elements were the subject of debates, collaborations and conflicts of different natures, which shaped the construction of input-output from the outside. These interactions, in particular with other research programs, are analyzed with a view of understanding how they constrained the input-output research program. However, we will not spend much time on the relationship of the input-output program with the rest of society. This

has been addressed to some extent elsewhere, in the study of input-output in the Bureau of Labor Statistics (Kohli, 2001), of input-output and development economics (Akhabbar, 2019: Chapter VII) or in the diffusion of input-output as a tool in the planning process (Carret, 2022a).

In the case of Leontief, his work on input-output led him to develop a theory of economic planning through his collaborations with government employees and administration economists. This work helped him justify and legitimize the construction of institutions such as the Harvard Economic Research Project, which advanced the scientific research on input-output methods. But Leontief was also cooperating or competing with other researchers in all these fronts: his research was debated in conferences, correspondence and publications by other academic economists; his beliefs on science, econometrics or economic policy were attacked and defended; his institutions were jeopardize or secured by ideological shifts, and by the competition with other economists vying for funds and personnel. By examining this record, we answer why Leontief was so disappointed in academic economics. The strong interdependence of ideas, beliefs and institutions in his research program did not leave much room for any kind of criticisms, lest it jeopardize the survival of input-output.

## II. Leontief's Research Program: Ideas, Beliefs and Institutions

Leontief arrived at Harvard University in 1932. The Committee on Research in the Social Sciences gave him the financial means to pursue his ideas, collect data and build the input-output approach. His interaction with government administrations in the early 1940s led him to a theory of economic policy based on the input-output approach, which he refined in subsequent decades. In turn, the system of belief that he developed around input-output legitimized the construction and funding of new institutions promoting the development of input-output approaches.

## A. The Science of Input-Output

Leontief's project started from the general interdependence between different sectors of the economy. While this interdependence had been the subject of many works from François Quesnay to Léon Walras, Leontief deplored that "when it comes to the practical application of this theoretical tool, modern economists must rely exactly as Quesnay did upon fictitious numerical examples" (Leontief, 1936: 105). The project which he set onto in the early 1930s was to give an empirical content to the abstract relationships expressed in general functions of production and consumption (Leontief, 1936: 116). Leontief described three steps to give an empirical content to general equilibrium: the formulation of a theoretical framework for an orderly collection of statistical data, the gathering and arranging of data itself, and finally the application of the theoretical framework to analyze the data collected (Leontief, 1937: 109).

The collection of data was organized around a double-entry accounting scheme ensuring that quantities produced somewhere were used as input somewhere else. The data was organized in a square matrix where the rows represent the output of each industry to other sectors of the economy, and the columns the inputs entering each industry. Dividing each column by the total output (the sum of a row) corresponding to the same industry allowed him to obtain a matrix of technical coefficients, interpreted as the amount of each input needed by an industry per unit of its output. The technical coefficients could then be used to examine how changes in one sector would affect the demand in all the other sectors, independently from the level of production.

The key assumptions made by Leontief were that the economy was in equilibrium, and that coefficients of production were constant. The assumption of equilibrium meant that the product of an industry was used either as input by other industries, or for the consumption of the household sector. In his early models, households were modeled as a sector producing work and



receiving different goods as inputs consumed (Leontief, 1941: 41). The idea that the economy was in equilibrium also came up when Leontief considered prices, as he introduced a set of equations relating the total value of the output of an industry with the costs of its inputs (Leontief, 1941: 36).

Constant coefficients meant using linear production functions and a very basic treatment of the factors of production, which amounted to a rejection of marginal productivity theory. Leontief gave two arguments justifying this approach: first, from a practical point of view, the important restrictions on the shapes of his production functions came from the limits imposed by the available statistical information (Leontief, 1937: 111). Second, he justified the absence of factor substitution and the fixity of technical coefficients by arguing that substitutability was an inherently aggregative concept: “the empirical importance of variable coefficients of production will become the smaller, the less we use aggregative concepts, the finer we elaborate our industrial classification” (Leontief, 1937: 113).

The focus on the productive structure of the economy, the interdependence of the sectors making up this structure, and the disaggregation of production in different goods and sectors characterized the input-output approach and guided Leontief’s early collection of data on the economy. A crucial step in the evolution of the input-output approach was the move from a model in closed circuit where nothing entered or exited, to an open circuit model with a final demand made of exogenous household and government consumption and investment. Opening the model transformed input-output from an academic tool describing the economy into a tool oriented toward informing economic policies. This open model continued to evolve in Leontief’s research center, where dynamic concerns, regional analysis and international trade, among other extensions, became the subject of new research in subsequent decades. Opening the model also

led to the possibility of normative statements on the best organization of the economy, the way in which governments could act upon it, and the place of the economist in this system. It created an opportunity to develop a belief system around what constituted good policies and good science, that were used to legitimize the construction and development of the input-output program.

## B. The Political Economy of Input-Output

The framework of general interdependencies at the basis of input-output guided the collection of data. It also laid the foundation for a theory of economic policy considering its effects through the whole industrial structure, rather than through aggregates or on one sector. Throughout his professional life, Leontief wrote about and developed his theory of economic policy, and how input-output could be used in this process.<sup>2</sup>

In a book chapter on “Economic Statistics and Postwar Policies” written during the war, Leontief explained how input-output could inform a coherent approach to economic policies and “supply a real foundation for a detailed mapping of concrete recommendations or specific actions” (Leontief, 1943: 161). For Leontief, the collection of data and the formulation of policies should not be viewed as separate endeavors, and it was precisely “the fundamental lack of coherence in traditional economic policies” that had led to an incoherent approach to data collection (Leontief, 1943: 159-160).

The first ingredient for input-output to become a useful guide to a coherent economic policy was to consider economics as a machine and the economist as its engineer. During his 1953 Gibbs lecture given at the annual meeting of the American Mathematical Society, Leontief spoke of “the quasi-mechanical nature of the economic system as a whole” (Leontief, 1954: 223). He held to this vision in the following decades, arguing that this machine was not infallible, so that

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<sup>2</sup> Reyes (2016) gives some examples of policy writings by Leontief but downplays their importance in Leontief’s work. I argue in this paper that the development of input-output was linked to his vision of economic planning.

economic policy and planning were justified by its failures: “When a machine does not perform as expected, one naturally is tempted to interfere. ... Any kind of active economic policy or economic planning represents a purposeful interference with the operation of the competitive machine” (Leontief, 1966: 239). This vision of the economy as a machine was echoed in media outlets after the war, as governments were turning to new kinds of interventions in their economies (Figure 1).

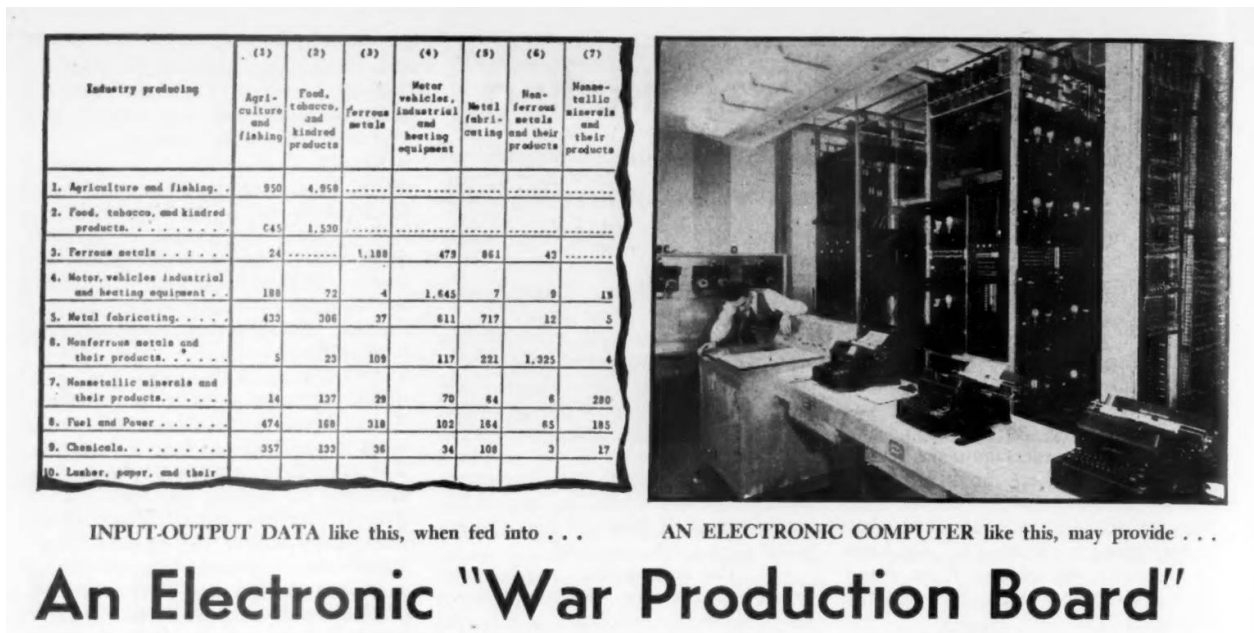


Figure 1: From *Business Week* of October 9, 1948.

The second ingredient of input-output as a policy tool was to draw up alternative plans or scenarios which a political authority could then choose from. Leontief already suggested this in the late 1940s (Leontief, 1949a: 214), and put this clearly in the 1950s, in the context of the development of a dynamic model meant to help governments draw investment policies: “In dynamic as in the static input-output analysis, consideration of the national economy as an open system offers an analytical tool particularly well suited to the making of appraisals of the material implications of alternative policy decisions” (Leontief, 1953: 65). In the following

years, he spread these ideas in publications and lectures aimed at a public outside of academia proper, in *Scientific American* (Leontief, 1951; 1952), in the *Naval War College Review* (Leontief, 1955a), and in *Foreign Affairs* (Leontief, 1960). In the 1970s, in a context of deepening crisis, a number of people, including Nixon's chairman of the Council of Economic Advisers, called for the creation of an "economic planning agency" (Golden, 1973). Leontief himself advocated for a "National Economic Planning Board" (Leontief, 1974), and reiterated the place that input-output should occupy in a planned economy. Dismissing in *Challenge* the "conventional monetary and fiscal policies, relying on a rather sketchy aggregative description and analysis of the economic system" (Leontief, 1976: 7), he argued instead that planning should be based on a "democratic choice among alternative scenarios" that could be drawn with the tools of input-output (Leontief, 1976: 6).

This was the recipe for the political economy of input-output: based on the knowledge of the industrial structure, policies targeting a particular sector or increasing the demand for goods in another sector could see their effect unfold through the machinery of economic relationship. The goal was to coordinate the consumption and investment of many different but interdependent sectors, and this implied that the decisions were centralized as well: if decisions in many different sectors were done on different bases, they would jeopardize the coordination through input-output. To solve the problems of imbalances and "miscalculations" of Soviet planning or the War Production Board, Leontief viewed the role of the planner as that of an expert in the centralization and analysis of economic knowledge, a "planning technician [that] must be able to compute not only one balanced plan, but many," before suggesting them to a political authority that could take its pick among them (Leontief, 1960: 263-265). Politicians or dictators could not be expected to carry these tasks by themselves, but they could "decide that the solution of this

problem is worth the cost involved in solving it; ... set the experts to work on it and give them all possible support” (Leontief, 1960: 264). Leontief saw the biggest hurdle in the adoption of input-output and other “scientific” methods of planning in Soviet Russia in their “bourgeois” origins (Leontief, 1960: 264-265); but he clearly saw a role to be played by input-output to balance multiple sectors and objectives, and noted that he met “scores of young economists” being taught in the USSR who had a “good acquaintance, both theoretical and practical, with input-output research” (Leontief, 1960: 268-270). The model led the economist to view the economy as a machine; to repair or steer this machine implied a detailed knowledge of its inner workings, and justified the financial support of institutions conducting input-output research.

### C. The Institutions of Input-Output

Leontief’s early work in Harvard was made possible by the existence of the Harvard Committee on Research in the Social Sciences. The Committee was a rebranded version of the Committee on Economic Research that had been created in 1917 to promote business cycle research at Harvard (Friedman, 2009; 2013; Carret and Assous, forthcoming). One of the first moves of the Committee was to recruit Warren Persons, who developed the Harvard Barometer and its forecasting methodology during the 1920s, before the economic collapse led to a quiet change in the Committee’s name. One of the staff members of the Committee was Elizabeth Gilboy, who read and reviewed Leontief’s work and was herself involved in early econometric debates (Gilboy, 1931; Morgan, 1990: 136-161; Bjerkholt, 2016: 46, 50). Gilboy had established contact with Leontief in the early 1930s, and she helped him settle at Harvard through the funding of the Committee on Research in the Social Sciences (Bjerkholt, 2016: 87ff.). The Committee received a major grant from the Rockefeller Foundation, and Leontief was one among ten of Harvard faculty members to benefit from it (Anonymous, 1936; see also the acknowledgment of his early

papers, Leontief, 1936; 1937). Leontief argued later that nobody believed in his project to the extent that the Committee asked him to report his “research failure so they could close the book on it” (Leontief, 1985: 28).

In 1941, Leontief published *The Structure of American Economy, 1919-1929*, which gathered his work of the past decade on the development of input-output and two input-output tables for the American economy in 1919 and 1929. The book itself was poorly received by reviewers, who acknowledged the valiant effort of Leontief and his assistants but criticized the assumptions of the closed model (Neisser, 1941; Boulding, 1942; Rothbarth, 1943). This publication coincided with the beginning of Leontief’s relationship with the Bureau of Labor Statistics, which participated in the construction of a table for 1939 and more importantly led the development of the open version of the input-output model (Kohli, 2001). This work was used in the postwar employment forecasts of the War Production Board, at a time when economists were scrambling to predict the consequences of the end of wartime spending.

Leontief’s relationship with the Bureau of Labor Statistics spread to other agencies, to the extent that in November 1948 he told Gerhard Colm, a member of the Council of Economic Advisers, that he could not continue to consult for all those government agencies without financial compensation.<sup>3</sup> In addition to the support of public agencies, Leontief succeeded in securing funds for his own research center on input-output at Harvard. The Harvard Economic Research Project was created in 1948 with the help of a grant from the Rockefeller Foundation, with Gilboy acting as associate director. In a “General Program Outline” for the Project, Leontief underlined that existing research on the American economy was either descriptive or concerned with short run movements, both insufficient approaches to understand and analyze the “structural

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<sup>3</sup> Letter from Leontief to Colm (November 24, 1948), Box 4, Folder ‘Council of Economic Advisers,’ Wassily Leontief Papers, Harvard University (hereafter WLP).

relationships in our economy over the long run.” Leontief argued that funding for the Project was warranted by the importance of its goals to answer “the long run questions of utilization and allocation of national economic resources” and “long-run development,” which were the major concern of postwar policies.<sup>4</sup>

In 1952, the Ford Foundation became a new funder of the Harvard Economic Research Project, who already had a staff of twenty regular members and ran a course in the Economics Department. In a document sent in 1954 to the Rockefeller Foundation, Leontief connected the growth of input-output and the collaboration with the Bureau of Labor Statistics, with the support given by the Rockefeller Foundation for the creation of the Harvard Economic Research Project and its development since 1948. He asked the Foundation to make its support more permanent with a one million dollar grant, ensuring the stability of the Project’s budget for the next decade. The penetration of input-output into government agencies, including the newly created Council of Economic Advisers, had legitimized the whole project and facilitated its support by the two foundations. The possibility of growth opened by this support in turn led to a dissemination of input-output to students, government agencies and businesses, helped by the possibility of welcoming visiting economists at the project for short-term stays.<sup>5</sup>

The diffusion of input-output abroad was summarized by Hollis B. Chenery, a former member of the Harvard Economic Research Project, who was working in 1950 for the Economic Cooperation Administration (the United States government agency in charge of enacting the Marshall Plan). Writing in November 1950 to Leontief about the results of his conversations with Dutch economists, Chenery was optimistic on the prospects of input-output for the organization of the European reconstruction, and for its adoption by the Organisation for European Economic

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<sup>4</sup> Leontief, ‘General Program Outline of the Harvard Economic Research Project’ (March 14, 1948), Box 3, Folder ‘Drafts, Research Project Papers,’ WLP.

<sup>5</sup> Leontief, ‘The Harvard Economic Research Project’ (circa 1954), Box 13, Folder ‘HERP,’ WLP.

Co-operation, the European organization created to distribute the aid from the Marshall Plan. This led to the publication a few years later of the first input-output table at the scale of the European economy (Kirschen, 1958).<sup>6</sup>

Leontief had met with Chenery a few months earlier during the first international conference on input-output in September 1950, a conference organized in the Netherlands where input-output was also being developed by government agencies, alongside national accounting techniques. The rapidity of the diffusion of input-output and the way in which Leontief's scientific discourse was able to convince technocratic elites, does not tell us anything about the way in which it was received and used, especially at the level of national planning where its application proved impossible (Carret, 2022a). Instead of taking up this problem, in order to understand Leontief's view of academic economics, we now turn to his debates, conflicts, and cooperations with other research programs.

### III. Competition and Cooperation with Other Research Programs

In the 1930s, Leontief was already positioning his own approach in opposition to the budding macrodynamic models developed by other early econometricians. The late 1940s were a time ripe for scientific debates, as different research groups which had been working in relative isolation began to meet again in annual conferences. Conflicting visions of the role of aggregation in economic policy, on the place of individual choices and rational behaviors, and on the role of the economist in society, paved the way for a collision of belief systems and policy recommendations. Some controversies spilled over into a conflict between institutions, as Leontief was trying to build his own research center and secure its funding.

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<sup>6</sup> Letter from Chenery to Leontief (November 14, 1950), Box 4, Folder 'C,' WLP.



## A. Scientific Debates Between Competing Research Programs

Even though his language remained mostly non-confrontational in the 1930s, it is striking how much Leontief's descriptions of his project were directed against macroeconomic approaches of the economy. As noted by Akhabbar (2019: Chapter VII), Leontief's approach was antithetical to the aggregated, macrodynamic research program that had been built by Ragnar Frisch, Michal Kalecki and Jan Tinbergen in the early 1930s (Carret, 2022b). The relationship between Leontief and Frisch in the 1930s was at an all-time low, as the "pitfalls" debate on the estimation of supply and demand curve was still smoldering (Bjerkholt, 2016: 92-98; Hagemann, 2021).

In the following years, Leontief remained staunchly opposed to macrodynamics and to the macroeconometric analysis that came after it, with Lawrence Klein as his main target. During the war, Klein had proposed his own interpretation of Keynes' General Theory in his PhD dissertation, supervised by Paul Samuelson at the Massachusetts Institute of Technology (Klein, 1944). He joined the Cowles Commission in 1944, and became the main builder of a macroeconometric model of the United States, relying on the tools of statistical inference developed by his colleagues of the Cowles Commission (Klein, 1950; Koopmans, 1950).<sup>7</sup>

With the war over, economists began to reconvene in conferences. In January 1946, the American Economic Association, the Econometric Society and a few other societies decided to hold their annual meeting together in Cleveland, Ohio. Beginning in 1947, the Econometric Society also held a meeting in late summer, jointly with the American Mathematical Society. Tjalling Koopmans, who was vice-president of the Econometric Society in 1949, pointed out to Leontief this division of labor, with the Christmas meeting focusing on the exposition of broad

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<sup>7</sup> See Akhabbar (2021) and Boumans (2009: 21-27, 2016: 419-420) on the conflicting econometric methodologies of Leontief and Klein; Akhabbar (2021) draws clearly the distinction between "indirect" statistical inference, rejected by Leontief, and his own approach of "direct induction." I thank a referee for bringing my attention to this point.

problems, and the late summer meeting on more technical problems, setting the stage for postwar debates.<sup>8</sup>

At the 1946 Cleveland meeting, W. Duane Evans, Jerome Cornfield and Marvin Hoffenberg, three economists from the Bureau of Labor Statistics, presented their model of postwar employment, based in part on the input-output study that they had led in collaboration with Leontief. During the same session, Klein presented his macroeconomic model of the United States economy (Econometric Society, 1946: 159-163). The conference focused on postwar demand and reconstruction, and in a letter to Leontief written shortly afterward, Evans expressed his disappointment at Leontief's absence from this discussion.<sup>9</sup>

The following year, at the September 1947 Washington D.C. meeting of the Econometric Society, Leontief was present to reject Keynesian theory and "aggregative analysis" as guides for economic planning, in favor of his disaggregated strategy (Leontief, 1949b: 273-274). Leontief's protestations against aggregative analysis did not diminish the interest of other researchers in the subject. During the 1947 meeting in Chicago, problems of aggregation featured in the discussions of several sessions, including one on the "Relationships Derived from Aggregate Data" chaired by Samuelson (Econometric Society, 1948b: 202-204). The importance of individual behavior in these models and the relationship of this problem with aggregation questions was addressed at the same time by Klein in several papers (Klein, 1946a,b), which offered another contrast with the purely technical relationships developed by Leontief in his input-output analysis.

By 1947-1948, in addition to the work on "indirect" statistical inference which Leontief was rejecting, it also became clear that the economists of the Cowles Commission were developing

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<sup>8</sup> Letter from Koopmans to Leontief (December 15, 1949), Box 4, Folder 'Cowles Commission,' WLP.

<sup>9</sup> Letter from Evans to Leontief (March 14, 1946), Box 2, Folder 'General Correspondence 1946,' WLP.

an alternative approach to the theory of production. Koopmans, who became director of the Commission in 1948, saw the links between input-output techniques and the approach that was developed at the same time by the Cowles Commission and the RAND Corporation. Leontief was invited to visit the Cowles Commission at the beginning of 1948, and Koopmans explained in a letter that his own work was “developed here rather independently of your input-output studies, but involving mathematical problems quite similar to those met by you.”<sup>10</sup>

Koopmans continued to develop activity analysis during the following months, and at the September 1948 meeting of the Econometric society in Madison, he presented a paper on “A Mathematical Model of Production” (Econometric Society, 1949a: 74-75). Koopmans’ paper was meant to provide a basis for the “concept of a general transformation equation for the economy as a whole ... used in welfare economics” (Econometric Society, 1949a: 74). At the same meeting, George Dantzig, the mathematician at the origin of linear programming, presented a paper on “Programming in a Linear Structure,” referring to models by Leontief, Schlesinger, Wald, von Neumann and Koopmans (Econometric Society, 1949a: 73-74). Leontief was absent from this meeting, but he was certainly aware of this work, as he was elected to the Council of the Econometric Society during this meeting.<sup>11</sup>

A few months later, Leontief, Klein and Koopmans all attended the December 1948 meeting of the American Economic Association in Cleveland (Econometric Society, 1949b). Leontief presented his approach as a way to say something in the “pragmatic language of active policy making” (Leontief, 1949a: 211). He dismissed both atheoretical empirical analysis (Leontief, 1949a: 214) and highly aggregated analysis (Leontief, 1949a: 218). Leontief’s main criticism during this meeting remained the empirical methodology of macroeconomic models, and the

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<sup>10</sup> Letters from Domar to Leontief (January 28, 1948), from Koopmans to Leontief (January 30, 1948) and Koopmans, ‘Visit of Professor Wassily W. Leontief’ (undated) Box 4, Folder ‘Cowles Commission,’ WLP.

<sup>11</sup> Letter from Cowles to Leontief (September 16, 1948), Box 4, Folder ‘Econometric Society,’ WLP.

reliance on aggregation and “indirect” statistical inference, which he exhorted economists to abandon. In Leontief’s perspective, aggregation blurred structural relationships in the economy to the extent that researchers relied “to ‘blind flying’ by the complicated but hardly fool-proof instruments of indirect statistical inference” instead of directly collecting data on industrial production (Leontief, 1949a: 218). During the discussion following Leontief’s presentation, Koopmans defended the tools of statistical inference at the core of the Cowles Commission macroeconometric program, arguing that Leontief’s dynamic model would reintroduce the same difficulties (Fabricant et al., 1949: 234). He also took issue with the idea that input-output analysis could be conceived as “general equilibrium analysis,” arguing that this analysis entailed the “exercise of optimizing choice” by individuals or firms to determine prices and quantities (Fabricant et al., 1949: 234).

Most criticisms during the discussion were focused on Leontief’s model, especially on the assumption of fixed coefficients of production. Members and affiliates of the Cowles Commission and other institutions working on linear programming and its application to production theory came to regard input-output models as a special case of their own approach. For instance, Herbert Simon, a regular attendant of Cowles Commission seminars, presented a model of production mentioning that “Leontief’s input-output model is a special case with one production process for each commodity,” viewing this as “a serious deficiency of Leontief’s model” because only one scarce factor of production was possible (Econometric Society, 1949b: 173). During another meeting, Charles Hitch, the head of RAND, casually talked about a “promising production function technique ... ‘linear programming’ or its variant, the Leontief input-output matrix” (Hitch, 1950: 198). Leontief certainly did not see his model as a variant of linear programming: he viewed the simplicity of the model as necessary to make it possible to

collect interindustrial data, and to obtain a solution from these data. It was already a computationally difficult task to invert a large matrix, without having to take into consideration more difficult shapes of production functions.

After this meeting, Koopmans launched the organization of a conference on activity analysis in which Leontief was involved. Leontief did not end up going to the conference, but sent one of his recruits at the Harvard Economic Research Project, Nicholas Georgescu-Roegen. During the conference, organized in June 1949, input-output analysis was frequently cited by the participants, but, again, it was referred to as a special case of linear programming and activity analysis.<sup>12</sup> Four papers presented during the conference focused on the non-substitution theorem, by Georgescu-Roegen, Samuelson, Koopmans and Arrow. This theorem was important for Leontief and it is no accident that it was proposed by Georgescu-Roegen, a member of Leontief's Harvard Economic Research Project, as it offered a justification for the assumption in Leontief's framework that there is one production process per commodity: "As Samuelson expresses it, although alternative processes may exist, only one is actually used and, therefore, only one can be statistically observed" (Georgescu-Roegen, 1950: 216). And observing the values of the actual processes was the whole point of Leontief's input-output approach.

The methodological debate between Klein and Leontief continued, and came to a head during a conference on Business Cycles organized by the National Bureau of Economic Research in November 1949. Leontief was the discussant of a paper by Klein on "Investment in econometric models." Papers were sent well in advance and their authors only had ten minutes to present their major points, followed by a half-hour discussion by the discussants and a general discussion from the floor, setting the stage for a debate on the opposing methodologies.<sup>13</sup>

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<sup>12</sup> On the conference, see for instance Backhouse (2012: 31-35) and D ppe and Weintraub (2014b: 457-459). See also Akhbar (2005; 2014) on the debates about the constancy of coefficients and the non-substitution theorem.

<sup>13</sup> Letter from Geoffrey Moore to Leontief (July 1, 1949), Box 6, Folder 'NBER Conference,' WLP.

Klein's presentation was an in-depth analysis of studies in investment, with applications to the railroad industry and the electric light and power industries in the United States (Klein, 1951). Klein tested the relationship between investment and operating income, the stock of fixed capital, and the interest rate on new bonds of a particular industry. In his comments, Leontief focused on the role of output changes on investment with reference to the work conducted at Harvard by Chenery (Leontief in Klein, 1951: 311). Leontief presented this as his substantive point, but he also offered a methodological criticism that set the tone for the rest of the discussion by arguing that the "neglect" of important factors was due to the lack of an investigation in "concrete quantitative terms" of the type he was advocating (Leontief in Klein, 1951: 310).

In his answer, Klein recognized that both he and Leontief were interested in detailed structural informations, but noted that they were at "opposite poles" on their empirical approach, and he disagreed with Leontief on the importance of information directly gathered from experts (Klein, 1951: 314-315). These comments during the conference led to an exchange of letters between the two economists a few days later.<sup>14</sup> The debate quickly shifted from the discussion of income versus output as appropriate variables explaining investment, to a debate around the methodology of statistical inference. Leontief argued that the selection of profits was a symptom of statistical formalism, and ended his letter by asserting, somewhat ironically, that he had so far restrained himself from being too controversial in his "impassionate plea for realistic analysis." Klein did not appreciate hearing again about Leontief's contempt for inferential statistics, and commented that he was repeating the same things he had to say at the Cleveland Winter meeting in 1948. In the same vein as Koopmans, Klein argued that the problems of identification of

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<sup>14</sup> Letters from Klein to Leontief (November 30 and December 13, 1949) and from Leontief to Klein (December 8, 1949), Box 6, Folder 'NBER Conference,' WLP.

equations that Leontief was raising could be turned against him: “Charges similar to those you raise against modern econometric work can be applied with equal force to your own analysis, and I feel that you overlook this on public platforms.” This debate continued in the following years, for instance when Klein reviewed the book published by the Harvard Economic Research Project (Leontief, 1953); In his review Klein noted that Leontief had not changed his position against macroeconomics and statistical inference, deploring that:

Leontief has strong ideas about the appropriate course of quantitative research in economics and a low level of tolerance for alternative approaches. ... He shuns macroeconomics and much of the technique of modern econometrics. ... It is hard to see why he felt the necessity for carrying the battle on every page. (Klein, 1953: 260)

Contrary to Leontief, Klein called for a complementary use of input-output analysis and macroeconometrics in his letters, arguing that he always “admired and supported” Leontief’s work, without viewing it as an “alternative to econometric model-building”; he argued rather that “the two should be complementary, each one handling problems that are outside the scope of the other.”<sup>15</sup> Years later, in his 1977 American Economic Association Presidential address, Klein was still promoting a reconciliation of Keynes and Leontief (Klein, 1978a,b). Despite Klein’s plea for complementarity, upon receiving his answer Leontief put an abrupt end to their discussion, arguing that “this might be as well since the note of personal acrimony which seems to be creeping into our discussion will hardly contribute to a solution of the controversial scientific problem at hand.”<sup>16</sup> Leontief wanted to limit their debate to science but his unwillingness to imagine a complementarity between the approaches was symptomatic of the belief system around input-output: using input-output in economic policy meant centralizing decisions and this could not accommodate itself with another basis for decision-making. Input-output was meant to suggest different scenarios (see subsection II.B) from which a decision-making authority could

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<sup>15</sup> Letter from Klein to Leontief (December 13, 1949), Box 6, Folder ‘NBER Conference,’ WLP.

<sup>16</sup> Letter from Leontief to Klein (December 20, 1949), Box 6, Folder ‘NBER Conference,’ WLP.

choose; but for these scenarios to keep their consistency, it was necessary that they be adopted as a whole, making it harder to integrate the policy tools developed by competing research programs. Thus the dispute between Leontief, Klein and Koopmans was not simply a debate over methodology, but a conflict between different approaches to the conduct of economic policy, relying on conflicting assumptions and rationale.

## B. Belief Systems Colliding

Leontief had developed a political economy of input-output hinging on the belief that the economy was a machine that could be used to predict the effect of alternative economic policies, and that it could be repaired when it started to break down. His view of the economy as a machine was not far from that developed during the socialist calculation debate by Barone, Lange and others. Leontief compared the working of the price system under perfect competition as that of an “impersonal automatic computer” (Leontief, 1966: 238), around the same time that Lange suggested that recent technical progress had solved the computational problem of finding a general economic equilibrium (Lange, 1967). As noted by Akhabbar (2010: 54), Koopmans and Klein also proposed competing visions of planning policies and of the economic system that led to different systems of beliefs, often viewed by Leontief as competing with his own approach.

Koopmans’ work, presented during the same postwar meetings where Leontief introduced input-output, was in the tradition of optimal allocation that harked back to the socialist calculation debate and welfare economics. It was first applied to transportation problems and then generalized to production and the utilization of resources (Econometric Society, 1948a: 66). This program, merged with the research on linear programming, culminated in the June 1949 Activity Analysis conference organized by the Cowles Commission. In his introduction to the



conference, Koopmans linked the theory of activity analysis with early general equilibrium models, Leontief's work, the development of welfare economics and the socialist calculation debate. But he argued that it was possible to abstract from any specific institutional arrangement, and this was one of the reasons to choose the term "allocation" of resources rather than "programming", which was too much reminiscent of a central direction of the economy (Koopmans, 1949: 6-7).

At the heart of his approach was the idea of a conscious action, represented by the concept of maximization that was the organizing principle of his 1957 book (Koopmans, 1957). In it, he started from Samuelson's recognition of the pervasiveness of optimization in economics, and presented economic debates on policy as recommending "that the policy makers seek to maximize some target function of variables regarded as social objectives, subject to given restraints of technology, of resource limitations, or of international intercourse" (Koopmans, 1957: 5). Koopmans' approach was intrinsically linked to the idea of maximization, and to welfare considerations, once it evolved into a theory relating competitive equilibrium and Pareto optima.

In contrast to Koopmans, Leontief's focus remained on the productive structure of the economy, and the individual barely appeared in his models. This was noticed by many critics, including Leonid Hurwicz who underlined that Leontief seemed to "regard it as unsound to have the behavior patterns brought into the picture" (Hurwicz, 1955: 632). Hurwicz, who had been a member of the Cowles Commission during the 1940s and who turned to the theory of resource allocation in the 1950s, expressed his doubts on the possibility of omitting altogether "the principles guiding human behavior in connection with the decisions affecting production, construction of capital equipment, and the size of inventories" (Hurwicz, 1955: 634).

It is clear that Leontief's was not a theory of the individual; maximization considerations were subsidiary to his scheme because the point of input-output techniques was to observe what values could be given to production functions, and not to infer whether or not they were the results of a rational behavior. The non-substitution theorem also showed that the question of rational behavior had little operational importance for his description of the economy. Leontief saw general equilibrium differently from Koopmans, insisting on the idea of interdependence rather than the problem of coordination of interests. The automatic, machine-like character of the productive system was the main object that interested Leontief, to observe and predict the effects of certain policies. In a 1950 letter to John Hicks, Leontief describe the opposition between the two research programs:

They [the Cowles Commission] concentrate on pragmatic problems of rational behavior, while we at Harvard are concerned mainly with positive explanation of the actual working of the economic system. The difference (sic) between the two are, incidentally, greater than they seem to appear to an outside observer. Starting with our common interest in theory, Cowles Commission develops it in the direction of technical refinement and mathematical elegance, while I move in the opposite direction of empirical analysis.<sup>17</sup>

Leontief emphasized this position against welfare economics during a 1951 Symposium on Linear Inequalities and Programming organized by the U.S. Air Force in Washington D.C. (USAF, 1952). In his presentation (not published in the proceedings), he argued that the problem of choice should not be posed as an alternative between different combinations of production processes, or as the maximization of a welfare function, but as a choice among different coherent scenarios that could be produced by an input-output model. Leontief underlined that a model basing decisions on a maximized function would have a real problem in translating “the choice

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<sup>17</sup> Letter from Leontief to Hicks (March 21, 1952), Box 8, Folder ‘H,’ WLP. See also Akhabbar (2005: 13) who mentions a letter from Leontief to Hoffenberg, where he complains that the Cowles Commission is distorting his positions.

function into a manageable form,” mentioning the contradictions that could arise from such functions.<sup>18</sup>

Leontief became increasingly vocal against a theory of economic policy based on objective functions, denouncing it during his Gibbs lecture where he argued that this analysis was unable to say anything about the distribution of goods in the economy (Leontief, 1954: 219);<sup>19</sup> in a review of the translation of Walras’ *Elements of Pure Economics*, he berated the “intrepid band of modern welfare theorists perched ... high above the ground of factual experience” (Leontief, 1955b: 250); and decades later, he still mocked the aimless task of a (probably imaginary) welfare economist, invited to advise a government and asking its members to describe their country’s social welfare function, before attempting to draw up an optimal plan to maximize this function: “Needless to say, both the government and the planner were completely frustrated. The planning approach cannot be implemented in such a naive way” (Leontief, 1980: 822). But naiveness was argued back against Leontief’s “exaggerated claims” (Koopmans, 1957: 189-191), and against the “numerous and varied claims concerning the usefulness of Leontief matrices in both government and business” (Ryan, 1953: 481).

Leontief’s Gibbs lecture is perhaps his most eloquent presentation of his qualms towards the rest of economic theorizing; building up the modern theoretical edifice from maximizing behavior and general interdependence, he lets his audience get a glimpse at the first cracks, when he compares “the modern welfare theorist” to his “counterpart,” the “eighteenth century believer in the Invisible Hand” (Leontief, 1954: 222). Halfway through his lecture, after lingering on dynamics, he detonates his charges and collapses the building: “Seldom, in modern positive

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<sup>18</sup> His presentation on “Choice and Non Choice Models” was not published in the proceedings of the conference but the stenographic transcription of his presentation is in his archives; see Leontief, “Choice and Non Choice Models” (June 1951), Box 10, Folder ‘Rand,’ WLP.

<sup>19</sup> Fittingly, his Gibbs lecture, which was republished in a collected volume of his essays in 1966, was used as a starting point by Sen (1976) in his critical examination of modern welfare theory.

science, has so elaborate a theoretical structure been erected on so narrow and shallow a factual foundation” (Leontief, 1954: 224). Alongside his contempt of the abstractions contained in the idea of a social welfare function, Leontief denounced in the same breath the use of “indirect” statistical inference to estimate aggregated models. He argued that inferential econometricians were in the position of someone asked to reproduce the blueprint of a motor on the basis of the information conveyed by the dashboard’s dials and the noise coming from under the hood, and suggested that the task of economists should be to look directly under the hood (Leontief, 1954: 228).

The “blind-flying” econometrician that received the brunt of Leontief’s attack was Klein, who had used macroeconomic models to lay the basis of his own theory of economic policy during the postwar meetings. During the 1947 Atlantic City meeting of the American Economic Association, Klein presented in the “Use of Econometric Models as a Guide to Economic Policy,” different aggregated models to test policies relating to investment, and underlined that his model included only factors on the demand side (Klein, 1947: 136). This was in stark contrast to Leontief’s model which hinged upon production functions and considerations of the supply side of the economy, another point of clear divergence between the two economists. In addition to definitions (accounting identities) Klein’s models were also based on Keynesian behavioral equations relating for instance consumption with income.

During the Washington meeting of the Econometric Society in September 1947, Leontief rejected “Keynesian theory” as a basis for economic policy, arguing that aggregation was useless for policy making and economic planning, “because in this type of question we have to deal with concrete, separate industries, with individual prices, or at least outputs and prices of small commodity groups” (Leontief, 1949b: 274). Leontief defended his approach as a more concrete

way to plan the economy, capable of giving precise answers to the questions asked by businesses and governments.

In their correspondence of 1949, both Klein and Leontief also fought over the boundaries of econometrics, by tracing them around their respective research programs. Klein contrasted in his answers “econometric model-building” and input-output analysis, showing that he clearly identified his methods to econometrics and that input-output techniques were outside of this scope. Leontief, who had taken part in some of the most important econometric debates of the 1930s, still exhorted his colleagues to follow him by addressing them collectively as “we as econometricians” during the 1947 Washington meeting. But his position in econometrics clearly shifted to that of an outsider, as was made clear by Howard Ellis when he asked him to write the chapter on econometrics of his upcoming *Survey of Contemporary Economics*, and described him as someone “who masters the mystery of Econometrics, but who is not identified by most economists as one of the ‘professionals’ in this field”.<sup>20</sup>

In 1952, the National Bureau of Economic Research’s Conference on Income and Wealth was organized in New York to appraise the results obtained by input-output studies, after recognizing both the large amount of funding that it received and the fact that it remained controversial among economists (Goldsmith, 1955: 3). The stated aim was to improve the communication between the practitioners who had implemented those studies and the economists who had dismissed its theoretical basis. Milton Friedman, in his discussion of the review of input-output by Carl Christ (a member of the Cowles Commission), summed up the general feeling of economists towards the approach, arguing that what would remain from input-output would be

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<sup>20</sup> Letter from Ellis to Leontief (April 2, 1947), Box 2, Folder ‘Books and Articles 1947,’ WLP. The survey can be found in Leontief (1948).

the “by-products” of the detailed exploration of particular industries, rather than the “grandiose dreams” of predicting major changes in the economy (Friedman in Christ, 1955: 174).

Along the same lines, one of the sharpest critics of the claims of input-output was F.A. Hayek. In the 1940s, Hayek emphasized the impossibility of concentrating the economic knowledge necessary to apply the ideas of central planners (Hayek, 1945). This did not prevent economists working in this vein to continue advocating for the creation of a “central coordinating agency” (Chenery, 1953: 95), that would be able to apply the criteria of input-output to plan and direct the economy. In the 1970s, as the same solutions were proposed in the wake of a new crisis, Hayek derided in *The Morgan Guarantee Survey* the “new confusion about planning” (Hayek, 1976: 4-5). This was a reaction in particular to a *New York Times* column by Leontief, where he had argued in favor of a “national economic planning board” (Leontief, 1974). Hayek concluded that “the present revival of the planning idea in the United States is inspired by the input-output representations developed by Professor Leontief, and rests entirely, I am sorry to say, on a colossal overestimation by its author of what this technique can achieve” (Hayek, 1976: 10). Leontief was rather displeased by this criticism and sent a letter to the editor disparaging Hayek’s credentials (Caldwell, 2016: 10).

The transformation of input-output from a descriptive tool to a framework for the coordination of national economic policies led to a belief system on economic policies. At the heart of it was the centralization of information into input-output tables, to suggest different scenarios to a decision-making authority; this vision of economic policy was different from that arising from the objective functions of welfare economics, from the macroeconomic policies advocated by Klein, and from the prescriptions of market coordination by Austrian economists such as Hayek. Leontief proved very resistant to other approaches of economic policies; this can be understood

when the logic of input-output is kept in mind: because the analysis is based on an interdependent system, if there were multiple decision-makers and one of them did not buy into a scenario and strayed away from it, the scenario would become inconsistent. Thus whenever Leontief and his followers described the institutional organization of input-output economic policy, they referred to a powerful authority variously called “central coordination agency” or “national economic planning board.” It was a “grandiose dream” which faced competing belief systems, stemming from different research programs. These conflicts traced and shifted the boundaries of econometrics and spilled over in the competition for funds and researchers between the institutions sustaining those research programs.

### C. Institutions and the Fight for Funds and Researchers

Leontief seemed to have remained convinced throughout his life that the failure of input-output to make any real headway in the planning process was political and practical rather than epistemic. That is, he viewed it as a consequence of the inability of governments to set up the necessary institutions to collect and centralize economic data, rather than the result of the impossibility to centralize economic knowledge and to forecast in an uncertain world. In the early 1960s, describing his dynamic model which required the collection of an entire new set of coefficients on the productive capacities of the economy, Leontief argued that “the principal obstacles to rapid adoption of such advanced methods lie not so much in the complexity of analytical design, but in the continuing inability of governmental and private statistical organizations to provide the large amount of detailed factual information that their practical application requires” (Leontief, 1966: 244).

At the same time, Leontief remained hopeful that the institutions necessary to centralize information and decision making would be built. As we have noted, such centralized institutions

were implied by the nature of input-output analysis: the technician could present several scenarios, but the scenario adopted needed to be implemented as a whole to remain consistent. Thus in the writings of the proponents of input-output policymaking, we consistently find references to a “central coordination agency” (Chenery, 1953: 95), a “national economic planning board” (Leontief, 1974) or a “central planning agency” (Dapprich and Cockshott, 2023: 421). Leontief remained optimistic even after government funding dried up in the United States during the Eisenhower administration, when the “unconcealed alarm” of “certain business circles” was on display against the application of input-output “to the traditional problems of the economic system as a whole” (Leontief, 1960: 268). In the 1950s, Leontief went abroad to convince governments in Europe and in developing countries to adopt input-output planning, and even went to Soviet Russia where he witnessed the development of input-output analysis in this country (Leontief, 1960: 268-271). Back in the United States, administrative funding for input-output was resumed in the early 1960s, when the Kennedy administration came into power (Kohli, 2001: 191 and 207-208). At the same time that he was criticizing the inability of governments to provide the needed factual information, Leontief thus also held the hope that “the steady advance in collection and systematic organization of basic statistical information will soon permit a practical application of more effective methods” (Leontief, 1966: 247).

Funding for input-output and the construction of tables was a major concern, as the gathering of data was a time consuming process but a necessary condition for the results of input-output analysis to make any sense. What made securing funds more difficult, or perhaps more urgent from the point of view of Leontief, was the competition for these funds between different research programs. In 1955, as he wrote to the Rockefeller Foundation to secure a million dollar grant financing the needs of his project for the next ten years, Leontief argued that “[the National



Bureau of Economic Research has received, for example, in the last two years, over three million dollars (!) from the Ford and the Rockefeller Foundations alone” (emphasis by Leontief).<sup>21</sup> Twenty years later, Leontief’s departure from Harvard was also motivated by institutional disagreements, at least according to Kenneth Arrow who argued that “Professor Leontief’s strictures on the Department represent a less than thoroughly candid presentation of his real motives. ... The Department refused to make an appointment he wishes to have to continue his work on input-output analysis.” Arrow argued that Leontief’s decision was made easier by the fact that he was offered funding by New York University to pursue his research, and that Leontief’s condemnation of Harvard’s economic department over the non-appointment of radical economists was at best misleading.<sup>22</sup>

In his letter to the Rockefeller Foundation, Leontief connected the necessity of securing funds with the importance of being able to retain researchers: “The very success of the Projects’ work threatens fatally its future progress: practically all members of the senior research staff constantly receive excellent employment offers from other universities and research organizations.” Beyond the competition for funds, it was indeed a competition for researchers that drove the antagonism between research programs led at the Cowles Commission and the Harvard Economic Research Project. After the September 1948 Madison meeting of the Econometric Society, Koopmans contacted Leontief, hinting at a possible new visit to Harvard in the fall and a cooperation between the Cowles Commission and Leontief’s group, to “avoid duplication of effort.” Koopmans ended his letter by asking Leontief’s opinion of Georgescu-Roegen’s mathematical ability. Leontief shared a very good opinion of Georgescu-Roegen, adding that he expected to keep him working on his project as long as he was willing to stay; he also announced to

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<sup>21</sup> Letter from Leontief to Weaver (June 6, 1955), Box 14, Folder ‘Rockefeller,’ WLP.

<sup>22</sup> Arrow to Bok (April 1, 1975), Box 68, Folder ‘Department of Economics, Harvard University,’ Kenneth Arrow Papers, Duke University. I thank Béatrice Cherrier for calling my attention to this archive.

Koopmans that he was going to receive some new funding, which led to the creation of the Harvard Economic Research Project.<sup>23</sup>

By then, it was apparent that the work of at least three different groups was overlapping: the input-output approach of Leontief at Harvard, the activity analysis of the Cowles Commission, and the construction of linear programming tools, developed by the Air Force and RAND. That there was overlap was obvious to Koopmans, a fact which prompted him to adopt both a cooperative and a confrontational stance: he launched in the following weeks the organization of the conference on activity analysis to bring the groups together in June 1949, but he also actively tried to poach some of the economists working with Leontief. In particular, it seems that Leontief did not take well the offer made by the Cowles Commission to Georgescu-Roegen. In late January 1949, a few weeks after the Cleveland meeting, after Koopmans enquired again about the status of Georgescu-Roegen at Harvard, Leontief told him bluntly that:

In my efforts to build up our team and to protect it from disintegration, I would have found it personally distasteful—not to say unprofitable from the point of view of sound research policies—to use any kind of insistent persuasion bordering on pressure. You can be assured that this applies in particular also to all my past, present and future dealings with Georgescu and all other members of my group whom you have approached with repeated job offers in the course of the last two months.<sup>24</sup>

Because of a last-minute illness, Leontief ended up not going to the conference organized by the Cowles Commission in June, but gave his blessing for Georgescu-Roegen to go.<sup>25</sup> The latter was sent as the only representative of Harvard, although Leontief was not without allies as both Evans and Hoffenberg from the Bureau of Labor Statistics presented papers during the conference (they were not published in the proceedings however).

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<sup>23</sup> Letters from Koopmans to Leontief (September 27, 1948) and from Leontief to Koopmans (September 30, 1948), Box 4, Folder ‘Cowles Commission,’ WLP.

<sup>24</sup> Letter from Leontief to Koopmans (January 28, 1949), Box 4, Folder Cowles Commission, WLP.

<sup>25</sup> Letters from Koopmans to Leontief (May 4 and May 16, 1949), from Ruth Kahn to Koopmans (June 8 and June 13, 1949), and from Georgescu-Roegen to Leontief (June 13, 1949), Box 4, Folder ‘Cowles Commission,’ WLP. Letter from Ruth Kahn to Georgescu-Roegen (June 15, 1949), Box 23, Folder “Leontief,” GRP.

While Leontief, Klein and Koopmans stood as suppliers of policies and beliefs, competing for the attention of buyers in policy circles, administrations and the rest of society, when it came to institution-building, they were on the demand side of funds, vying for grants and researchers to sustain the life of their institutions and ensure the continuation of their research program. In return, this competition had a direct impact on the research program, in particular through the movements of researchers.

#### IV. Beyond the Ivory Tower

Why was Leontief so successful, despite numerous challenges and repeated confrontations? One explanation for the continuing success of his research program on input-output is the tenacity with which he built, not only the theory and its empirical content, but also the beliefs legitimizing his enterprise and the institutions sustaining it. At each step of this construction, Leontief found competitors and collaborators, and everything in between. His efforts in defending institutions, science and beliefs ensured the continuous spread and vitality of the input-output research program. But Leontief remained disappointed in his inability to convert the majority of the economic profession to his ideas. At the heart of this disappointment was his implied vision of the relationship between science and society.

The place for the expert that Leontief gave in his description of planning policies drew a direct line from the ivory tower to political choice. This was the spirit of his time, when many scientists, from their ivory towers, thought that they could directly influence, control and organize the public space (Caldwell, 2020; Dekker, 2022; Alacevich, 2022). In arguing that they took the political element out of economic expertise, they ignored the mediation of their ideas through a belief system. In the case of Leontief, input-output analysis led to the belief that a detailed knowledge of the economy was both possible and useful for the conduct of economic

policies. But only Nazi Germany, Soviet Russia and Maoist China could obtain a degree of information centralization and a power so concentrated that a decision could rain down the chain of command, with all the consequences that this had for social organizations (Eucken, 1948). Leontief failed in this respect, but he saw this failure as a consequence of the unwillingness of governments and economists to align their efforts with his work, rather than a problem ingrained in his theory of economic policymaking.

Leontief's belief system was so tightly connected with a need for funds to collect data and feed the input-output models, that any attack on a part of his system endangered the survival of input-output. The "grandiose dreams" of input-output did not leave room for the existence of competing research programs, but in the end, through the competition between different research programs, input-output came to take a place in the arsenal of information tools to conduct economic policies. It did not become the blueprint that Leontief had been calling for, and this failure, amidst the continued successes of input-output to draw the attention of researchers and funders, explains the continued resentment that seeped through Leontief's speeches and publications, in the face of the impossibility to make input-output the central piece of economics and economic policies that he was hoping for.

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