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# Understanding the bitterness of Wassily Leontief: Postwar success and failures of input-output techniques

Vincent Carret<sup>1</sup>

March 9, 2022

## Abstract

Although Leontief was and still is one of the most recognized names in economics, inextricably linked to the development of input-output techniques, he remained fiercely critical of other economists' works and of the state of economic science during his whole life. To understand his bitterness, we go back to the root of the split between Leontief and the rest of the economics profession, through an examination of the debates that took place in the late 1940s. From his input-output model, conceived as an operational theory of economic interdependencies, Leontief drew a specific approach to economic policy and planning which had a lot of success with government agencies, explaining how he could durably sustain his split from the profession.

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*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)*

The above 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 statistical inference and its complex procedures (Leontief, 1953: 5-7; 1971: 2-3). Towards the end of his career, this bitterness was fueled by an indictment of the economic establishment (Leontief, 1982: 107), and the regret that the redirection in economic research that he had been calling for during the past half-century had not happened (Foley, 1998: 126).

Leontief adopted an increasingly dismissive tone towards contemporary economics in large part because he felt that he did not influence the direction taken by the profession. However, he maintained a prominent institutional position, as the 1954 president of the Econometric Society, the 1970 president of the American Economic Association, the recipient of the Nobel Prize in economics in 1973, and a member of the Harvard economics department from 1932 to 1975. His debates with other economists during the late 1940s explain a lot of his subsequent trajectory, and the apparent disconnection between his position and his frustration. As academic economists rejected input-output techniques, they spread widely among governments, industries and international organizations: this allowed Leontief to maintain a steady stream of funding and recognition, without giving him the satisfaction of seeing his work change the ways of economists.

Leontief's exasperation with the state of economic thinking can thus be traced back to his original project as it unfolded in the postwar period, and how it was received in professional circles at a time when input-output techniques started to take off as an instrument of economic analysis. Several recent historical studies have looked at Leontief's work during this period and beyond. Bjerkholt (2016) has given an extensive view of Leontief's trajectory before the 1940s, and of his debates with other economists in the 1930s, which have also been presented by Hagemann (2021) who focused on his German period. Kohli (2001) retraced Leontief's collaboration with the Bureau of Labor Statistics (BLS), emphasizing the influence of the latter in the development of the input-output approach. Boumans and de Marchi (2018) have noted the interactions between Leontief and Tinbergen in the early 1950s and the influence of input-output analysis on postwar planning. Two special issues of *Economic Systems Research*, the journal of the International Input-Output Association, examined the relation between input-output analysis and classical economics (Kurz and Lager, 2000) and Leontief's personal trajectory, predecessors and contemporaries working in the same vein (Bjerkholt and Kurz, 2006). Boumans (2009: 21-27, 2016: 419-420) and Akhabbar (2021) both noted Leontief's criticism of statistical inference and his defense of "direct observations," as well as his accusation of circular reasoning towards the type of econometrics developed at the Cowles Commission at the same time. Akhabbar (2005) also started to explore the links between Leontief and the development of activity analysis at the Cowles Commission, with an emphasis on the different methodologies of Tjalling Koopmans and Leontief, and the non-substitution theorem (Akhabbar, 2014). Leontief appears as a bystander in the histories centered on the development of linear programming and activity analysis, which have taken note of his involvement in the 1949 Activity Analysis conference where he was invited but did not go, and the prominence of the input-output model in

the program of this conference (Düppe and Weintraub, 2014: 457-459; Backhouse, 2012: 31-35).<sup>2</sup>

None of these studies however has addressed upfront the aggressiveness of Leontief towards his contemporaries, and how they might have received his criticisms, which were aired in his publications and during the professional meetings where the issues of postwar economic analysis and policy were discussed. The image painted by these works is often that of a technical economist, who developed his own theoretical framework based on general equilibrium and the collection of direct observation, which makes it hard to fathom the level of resentment that he developed. Looking back at Leontief's original project in the context of the rise of economic policy, and perusing through his archives, gives us a picture of an economist deeply concerned with the relevance of his work, never shirking from a confrontation, laboriously working without the assurance of seeing his work recognized.

In a short period, as input-output techniques became more visible, Leontief's disagreements with other economists, especially those of the Cowles Commission led by Koopmans, took a much more public turn, spurring a series of public and private debates with other economists and econometricians. The arguments raised on both sides shed light on the sources of Leontief's aversion to most of the work unrelated to input-output analysis, and perhaps more importantly, on the stakes underlying these conflicting views. Beyond a simple understanding of why input-output techniques followed a trajectory outside of the mainstream of economics, the questions raised in his debates with other economists, in particular those developing macroeconomic previsions and optimal allocation theories, show us that the issues at stake were

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<sup>2</sup> In addition to the works cited above which take a historical perspective on Leontief and the development of input-output techniques, there is a much larger body of work from his followers, almost invariably striking a celebratory tone, sometimes faintly shaded by evasive remarks on his "ruthless" or "blunt" criticisms of other economists (Polenske, 2004: 10), criticisms which are generally shared by the author (Polenske, 2004: 25).

much deeper than technical oppositions on data collection, empirical tools or theoretical framework. The problem that was at the heart of the debate was that of the formulation and evaluation of public policies, and the relationship between the rational individual and macroeconomic phenomena.

## I. The political economy of Wassily Leontief

Nicholas Georgescu-Roegen, mounting a defense of Leontief's system during an afternoon session of the Econometric Society annual meeting in December 1949, distinguished three steps in Leontief's contribution: the first one was achieved in 1936 when Leontief published his paper on "Quantitative Input and Output Relations in the Economic Systems of the United States," where he described a *tableau économique* of the interindustrial exchanges in his country of adoption. The second step developed his theoretical model based on linear production functions in a 1937 paper and in his 1941 book on the *Structure of American Economy* (Leontief, 1941). This approach remained descriptive of the American economy as the model developed was "closed," that is, it did not include exogenous variables. For Georgescu-Roegen, these first two steps were mostly uncontroversial, but the issue resided in the third phase of Leontief work, when, spurred by the needs of the war administration, he tried to reach for "the most hoped for fruit of all economists, prediction" (Georgescu-Roegen, 1950: 214-215). This neat division was arranged to protect Leontief's work on input-output tables from the attacks of the critics who focused on the simplicity of the theoretical assumptions. That this further step in Leontief's work was controversial is indubitable, as it was the occasion for him to give a more definite shape to his views on economic policies, which ran counter to the prevailing approaches. What he developed through his papers of the mid-1940s, was a real political economy of input-output, framing how this approach could guide economic policies and government planning.

## A. Filling the empty boxes of economic theory

After he arrived at Harvard in 1932, Leontief's project to start his empirical study of the economic system was supported by the Harvard University Committee on Research in the Social Sciences, the rebranded committee that had developed the Harvard Barometer during the 1920s. Leontief's project started from the recognition of the general interdependence of different sections of the economy, "[a]nd yet, 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). His project was thus to give an empirical content to the abstract relationships expressed in abstract functions of production and consumption:

The statistical data collected in our main table fill in the "empty boxes" of the theory of general equilibrium. Hypothetical production and consumption equations gain explicit meaning as soon as the symbolic algebraic signs are replaced by observed numerical values. Once an empirical foundation is thus established, the vague generalities of abstract theoretical statements will acquire concrete empirical significance. (Leontief, 1936: 116)

Even though his language remained mostly non-confrontational in the 1930s, it is striking how much Leontief's descriptions of his project was directed against the schemes built by early econometricians at the same time; in 1937, he started from two pairs of distinctions between general and partial equilibrium and between static and dynamics, arguing that "The general, and at the same time dynamic, type of analysis still remains an unwritten chapter of economic theory, the claims of innumerable 'model-builders' notwithstanding" (Leontief, 1937: 109). Although implicit, this was a slight toward Ragnar Frisch, who had begun his 1933 paper on impulse and propagation mechanisms with the exact same distinctions, taking the position of an aggregated dynamic analysis which gave way to the macrodynamic analysis developed by Frisch, Kalecki and Tinbergen (Carret, 2021). The relationship between Leontief and Frisch in the early 1930s was at an all-time low, as the furious "pitfalls" debate was still smoldering (Bjerkholt, 2016:

92-98). Leontief remained staunchly opposed in the following years to macrodynamics and to the macroeconometric analysis that came after it.

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 "empirical application of the previously developed theoretical devices to analysis of factual data" (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, and his major undertaking of the 1930s was to painstakingly collect technical data from engineering publications and engineers themselves, to paint an empirical picture of the economy.

The heart of the theoretical model behind input-output analysis is to start from the interindustrial relationships embodied by the flows of goods and services between different parts of the economy. These flows are described in a square matrix where the rows represent the output of each industry to other sectors of the economy, and the columns are consequently interpreted as the inputs entering each industry. Dividing each column by the total output (the sum of a row) corresponding to the same industry allows one to obtain the technical coefficients interpreted as the amount of each input needed by an industry per unit of its output. This scheme is decidedly oriented toward the supply side of the economy, and in the earlier version of the "closed" model, households are considered as just another sector producing work and receiving different goods as inputs consumed. The crucial step from the closed to the open model was to detach this "sector" from the rest of interindustrial relationships, to consider the total product of an industry as a sum of its intermediate products sent to other industries and a final demand



which was termed the “bill of goods.” Under this form, the production of each good  $x_i$  by industry  $i$  can be written as  $x_i = a_i X + d_i$  where  $a_i$  is the vector of technical coefficients of industry  $i$ ,  $X = x_1 \dots x_n$  is the vector of output of the  $n$  industries composing the economy, and  $d_i$  is the final demand of good  $x_i$ . The complete system can be written in the matrix form  $X = aX + d$ , and the problem of examining the influence on production of a change in the final demand or “bill of goods”  $d$  can be solved by expressing  $X$  as a function of  $d : X = \frac{1}{I-a} d$  where  $(I - a)^{-1}$  is the so-called inverse Leontief matrix ( $I$  is the unit matrix). This relation allowed him to examine the direct and indirect effects of a rise in the final demand and answer questions about employment changes arising from these shifts in demand (Leontief, 1944: 291).

Leontief was conscious that his theoretical framework, especially the linearity assumption and the treatment of factors of production, amounted to a rejection of marginal productivity theory. He 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). This remained one of his lines of defense throughout his debates with other economists; for instance, in a letter to Klein, Leontief asked him whether the railroad industry could really substitute fuel and workers to run its trains.<sup>3</sup>

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<sup>3</sup> Letter from Leontief to Klein (December 8 1949), Box 6, Folder “NBER Conference,” WLP.

Another reason for Leontief to shun aggregated analysis is that he found it useless to answer usefully and precisely the questions of economic policy which interested him. That much he made clear in a chapter on “Economic Statistics and Postwar Policies,” when he wrote that:

Theoretical economists have tried to report these intricate relationships in terms of a few strategic variables. Some statisticians have taken it upon themselves to depict these generalized variables in terms of indirectly computed aggregative figures. Both these theories and these figures can be conveniently used as arguments in justification of some particular types of policies, but neither can supply a real foundation for a detailed mapping of concrete recommendations or specific actions. (Leontief, 1943: 161)

Leontief rejected aggregated analysis, which had been developed for the past ten years, since the first macrodynamic models had been built by Frisch, Kalecki and Tinbergen, who introduced the distinction between aggregated “macrodynamic” models and disaggregated “microdynamic” models. During the Washington meeting of the Econometric Society in September 1947, Leontief forcefully defended his approach as the next logical step after partial equilibrium approaches, referring to the “shipbuilding cycle” (which had been studied by Tinbergen fifteen years earlier), and arguing that “[u]p to a relatively recent time it was-considered to be too difficult, and certainly not very promising, to indulge in a real empirical general-equilibrium analysis” (Leontief, 1949b: 273). Noting that this trend toward aggregation had been pursued since “the advent of Keynesian theory,” Leontief rejected this approach to economic policy:

Anybody who was concerned with the practical application of econometric analysis, I think, is conscious of the fact that in a large number of instances, these aggregative measures are not very useful. Particularly in connection with many problems of policy-making and of economic planning of any kind, aggregative concepts are very limited in their application, 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)

Thus Leontief argued that it was necessary to consider the effects of policies through the whole industrial structure and not just through an aggregate of production. The integrated framework embodied through general interdependencies would guide the collection of data, the

only way for him to create a coherent set of economic policies based on purposefully collected data: “This lack of integration and coherence is not accidental. It clearly reflects the fundamental lack of coherence in traditional economic policies” (Leontief, 1943: 159-160).

## B. Machines, humans and economic policy

Leontief’s position on economic policy stemmed directly from his model, and he clearly had a very mechanical vision of how economic processes unfolded. 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). His view of the economy as a machine was not far from that developed during the socialist calculation debate by Barone, Lange and others; thus he compared the working of the price system under perfect competition as that of an “impersonal automatic computer” (Leontief, 1966: 238). But he argued that this computer was not infallible, so that 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 was a double-sided image of the economy: on the one hand, the productive system was very efficient in the allocation and production of resources, so that economic policies should not focus on changing the structure of the economy which was taken as given and efficient: “The market is indeed a marvelous machine” (Leontief, 1974: 37). On the other hand, the “computing system” that the market was, like any other machine, could break down, as his experience of other machines had taught him: “any one who has had some practical experience with large computers knows that they do break down and can't operate unattended” (Leontief, 1971: 6), an approach of the

economy which was discussed in media outlets (Figure 1) at a time when the war economy had made planning acceptable.

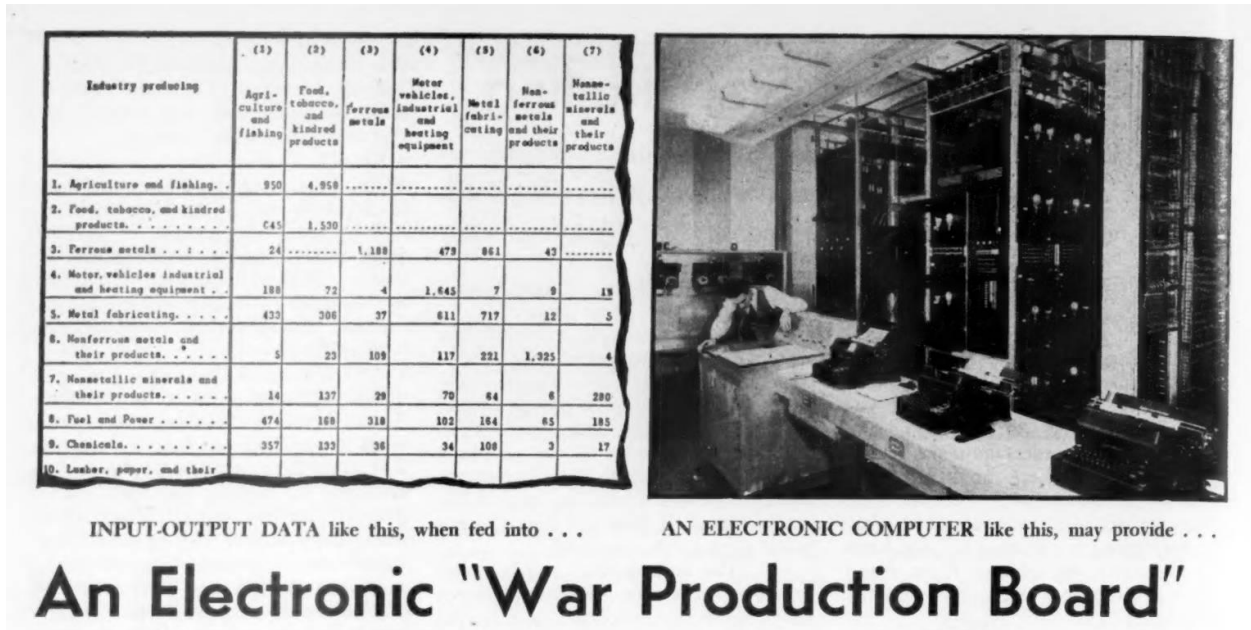


Figure 1: From *Business Week* of October 9 1948.

The analogy with a machine seemed to leave little place to the problem of individual choice in the economic system. 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, expressed his doubts on this omission, arguing that “It would be surprising if economic analysis could be carried on successfully in ignorance of the principles guiding human behavior in connection with the decisions affecting production, construction of capital equipment, and the size of inventories” (Hurwicz, 1955: 634). This position taken by Leontief was indeed one of the issues on which he was attacked by Koopmans in the late 1940s, as it ran counter to the program developed at the Cowles Commission. Leontief always remained

opposed to a theory of economic policy based on a social optimum or on welfare considerations, and he gave several explanations for his point of view.

His position on welfare economics and rational choice was expanded during his Gibbs lecture, where he pointed out how little this analysis was able to say about distribution:<sup>4</sup>

A much more specific description of its properties would have to be required if the social welfare function were to reflect ... concrete normative judgments pertaining, for example, to the problem of income distribution. ... The important contribution of the mathematical approach to our thinking on such controversial issues consists in showing how difficult it actually is to formulate in concise operational terms any specific normative attitude toward questions of public welfare in general and the problem of equitable distribution of income in particular. (Leontief, 1954: 219)

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 down his lecture, after lingering on dynamics, he pulls the trigger and blows up the building:

One has, unfortunately, to admit that neither the simpler type of economic theory nor its most modern dynamic versions have brought us very far along the road toward detailed explanation, not to say prediction, of the specific states of the actually observed economic system. Seldom, in modern positive science, has so elaborate a theoretical structure been erected on so narrow and shallow a factual foundation. (Leontief, 1954: 224)

Moving on to indirect inference, he castigated what he viewed as a limited contribution, and compared the work of inferential econometricians to that 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: "It certainly becomes much easier if we are allowed to look under the hood" (Leontief, 1954: 228). Looking under the hood of the machine to inform

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<sup>4</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.

economic policies was for him the answer both to the abstract theorizing of the welfare theorist and to the blind-flying methods of the modern econometrician.

There was still a place for choice in his wider theory of planning based on the results of input-output models. In a postwar speech on “Choice and Non Choice Models” which he presented during a 1951 conference on linear programming, he started to clarify this position by contrasting models relying on the maximization of a welfare function and models aimed at proposing different alternatives from which a political process could choose.<sup>5</sup> Leontief underlined that a model basing decision on a maximized function would have a real problem in translating “the choice function into a manageable form,” mentioning the contradictions that could arise from such functions. Before the 1950s, we can already find the idea that input-output analysis was aimed at evaluating “the quantitative implications of alternative policies in respect to allocation of primary resources or, say, various patterns of public works or governmental purchases” (Leontief, 1949a: 214). This notion of “alternative policies” later on became a centerpiece of his theory of planning and of democratic government, as he made clear in a series of papers published during the 1970s. Dismissing 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 between alternative scenarios: “Detailed description of economic relationships, and democratic choice among alternative scenarios, are essential ingredients of national economic planning” (Leontief, 1976: 6). Again, this was clearly formulated in opposition to a social welfare function, with Leontief insisting that the economist’s role was to point out to political deciders the different consequences of alternative policies:

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<sup>5</sup> His paper was not published in the proceedings of the conference but the stenographic transcription of his presentation is in his archives; see “Choice and Non Choice Models” (June 1951), Box 10, Folder “Rand,” WLP.

This, I submit, is the reason why a planning process should start out not with the formulation of what theoretical economists refer to as the general 'objective function,' but with an elaboration of alternative scenarios, each presenting in concrete nontechnical terms one of the several possible future states of the economy. (Leontief, 1976: 8)

Leontief drove his point home in a special issue of the *Revue Économique* about planning; in his contribution on the “Planning Approach to Economic Policy Formation,” he took position in favor of planning the economy instead of relying on “the hope ... that the process of quasi-natural selection will sooner or later converge to a satisfactory solution” (Leontief, 1980: 820). For him, the input-output approach was the logical way to go about planning which required a “detailed knowledge of the structural characteristics of all the different sectors of the particular national economy and a quantitative assessment of the mutual interrelationships that can be derived from this approach” (Leontief, 1980: 821). But Leontief noted that this was not a widely shared conception among economists, and he deplored that “[t]he aversion of academic economists to giving up traditional reliance on highly general mathematical formulations linked to tangible reality only by an elegant but very fragile bridge of indirect statistical inference” (Leontief, 1980: 821). His rejection of welfare economics was clearly apparent in this paper where he referred to Arrow’s impossibility theorem and mocked the aimless task of the welfare economist:

I remember having met, many years ago in a large developing country, one of my eminent colleagues who was invited to give economic advice to the government of that country. He started out by visiting, with notebook in hand, the prime minister and a member of his cabinet, and asking them to describe to him the shape of the social welfare function of their country. With this information in hand he intended to proceed with computation of an optimal development plan that would maximize the value of that 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)

Leontief's ideas attracted the interest of social planners very early, with the BLS playing a major role in supporting his work, before it spread in other countries where government planning offices adapted the input-output approach to their different national contexts.

The immediate postwar saw the return of economic meetings, which had been interrupted during the mobilization. The first important meeting was held in January 1946 in Cleveland, where the American Economic Association was followed by the Econometric Society and a few other societies who decided to hold their annual meetings together. Starting in 1947, the Econometric Society also held a meeting in late summer, jointly with the American Mathematical Society. Koopmans, who was vice-president of the Econometric Society in 1949, pointed out that the Christmas meeting with other economists and statisticians was focused on the exposition of broad problems, while the late summer meeting emphasized more technical problems.<sup>6</sup> Thus, a climate ripe for discussions and confrontation was put in place. In 1946 at the Cleveland meeting, two economists from the BLS who had been closely working with Leontief presented their input-output model of postwar employment during the same session where Klein presented his macroeconomic model of the US economy (Econometric Society, 1946: 159-163). The resulting series of debates involving input-output analysis and its critics culminated in 1949, a pivotal year for Leontief's relationship with the rest of the economic profession.

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<sup>6</sup> Letter from Koopmans to Leontief (December 15 1949), Box 4, Folder "Cowles Commission," WLP.



## II. 1949, a watershed year of disputes

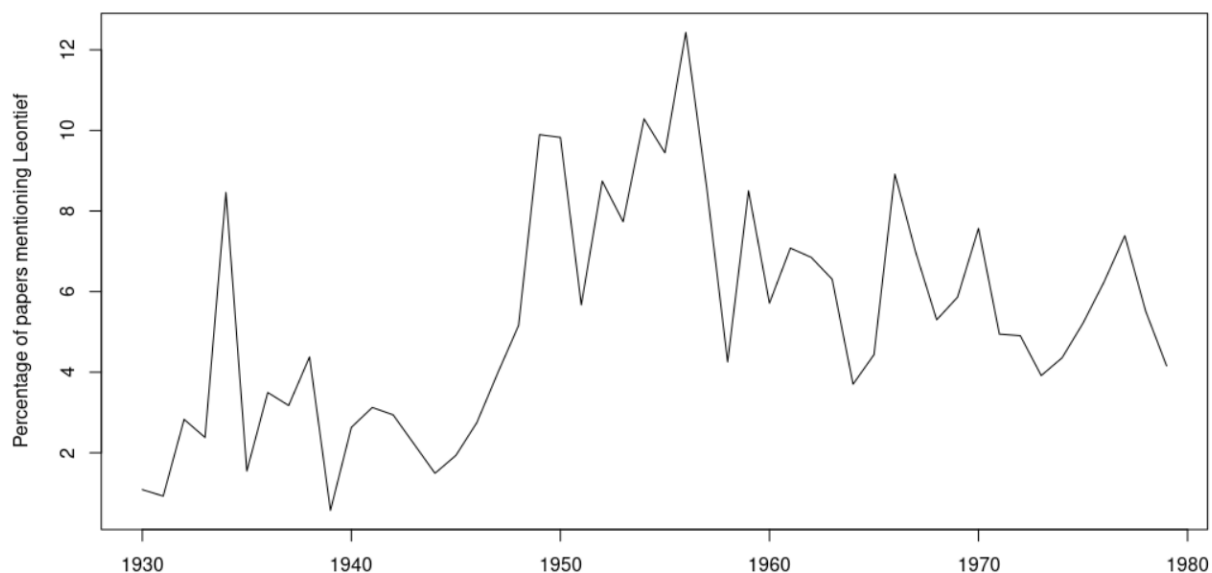


Figure 2: percentage of papers mentioning at least once “Leontief” in *Econometrica*, *The American Economic Review*, *The Journal of Political Economy*, *The Quarterly Journal of Economics*, and *The Review of Economics and Statistics*

Leontief started to gain recognition at the end of the war, something illustrated by the increasing number of articles mentioning his name in the late 1940s (Figure 2). In 1948, he was able to set up the Harvard Economic Research Project (HERP) through a grant of the Rockefeller Foundation and the support of the Air Force, and he was elected to the Council of the Econometric Society in September.<sup>7</sup> As he garnered success, his disagreements with other economists came to a head and started playing out during professional meetings and in private correspondence. The turn of the decade was a pivotal moment in his career, as he both attained a highly respected status and saw his research program being challenged more than ever.

We can distinguish two themes in Leontief’s opposition to contemporary economic research: the problem of statistical inference in relation to aggregate statistical models, and the relationship

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<sup>7</sup> Letter from Cowles to Leontief (September 16 1948), Box 4, Folder ‘Econometric Society,’ WLP

between input-output techniques and the theory of activity analysis, both of which were associated with the Cowles Commission, and both raising the problem of rational behavior.

#### A. Input-output techniques and activity analysis: cooperation or confrontation?

During the September 1947 Washington meeting of the Econometric Society (a large meeting which was jointly held with the International Statistical Institute), Leontief's paper on "Structural Matrices of National Economies" was discussed by Koopmans and Hurwicz among others. Both of them were associated with the Cowles Commission, although Hurwicz had left the preceding year, while Koopmans was to become director the following summer. We do not have the text of their interventions but it is likely that Koopmans and other people from the Commission saw the links between input-output techniques and the approach of production that they were beginning to develop, as Leontief was invited to visit the Cowles Commission at the beginning of 1948. Evsey Domar sent Leontief a detailed program for the few days in February when he was visiting, which was further elaborated and circulated by Koopmans.<sup>8</sup> This program showed that Koopmans was himself working on "Systems of linear production functions," and in a letter to Leontief, Koopmans presented this work as "developed here rather independently of your input-output studies, but involving mathematical problems quite similar to those met by you."<sup>9</sup>

During the Washington meeting, Koopmans had presented a paper on the "Optimum Utilization of the Transportation System" (Koopmans, 1949), which was a first step toward the study of activity analysis models. Koopmans' work was inscribed in the tradition of optimal allocation that harked back to the socialist calculation debate and welfare economics: "In this paper the principles of welfare economics are applied to any transportation system in which

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<sup>8</sup> Letter from Domar to Leontief (January 28 1948) and Koopmans, "Visit of Professor Wassily W. Leontief," Box 4, Folder "Cowles Commission," WLP.

<sup>9</sup> Letter from Koopmans to Leontief (January 30 1948), Box 4, Folder "Cowles Commission," WLP.

goods are transported by means of movable equipment” (Econometric Society, 1948b: 66). It was presented in one of the two sessions devoted to the “Theory of Choice and Utilization of Resources,” along with papers from Oscar Lange and Maurice Allais among others. The ideas contained in this paper were later remodeled into the linear programming framework, presented at the Activity Analysis conference and published in the proceedings, where Koopmans referred the reader to the original Washington meeting paper for “a nonmathematical exposition of this model” (Koopmans, 1951: 222).

In early 1948, while Leontief’s input-output framework was already well-formed, activity analysis was thus still in its preliminary stages. Koopmans worked on 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, 1949b: 74-75). Koopmans still referred to welfare economics, and clearly presented the bases of his activity analysis model, arguing that “this model differs from similar models discussed by Leontief in that the number of possible activities exceeds the number of desired end-products, thus permitting choice and substitution between production methods” (Econometric Society, 1949b: 74). At the same meeting, George Dantzig, then a “Mathematical Advisor” at the Air Force, presented during a symposium on the theory of games a paper on “Programming in a Linear Structure,” referring to models by Leontief, Schlesinger, Wald, von Neumann and Koopmans, and arguing that his model applied to a dynamic system (Econometric Society, 1949b: 73-74). Leontief was absent from this meeting, but he was certainly aware of the work being done, as he was elected to the Council of the Econometric Society at the same time this meeting took place. The fact that several groups were working on similar subjects became clearly apparent, and with it two possibilities: cooperation or confrontation.

Shortly after this meeting, Koopmans reestablished contact with Leontief, hinting at a possible new visit to Harvard in the fall:<sup>10</sup>

I have presently further pursued the mathematical and economic theory underlying the technique of 'linear programming', being developed by Dantzig and others in the Air Force. The subject is essentially the same as that which I discussed at the time of your recent visit in Chicago, and is also related to the subject of input-output models as studied by yourself.

Koopmans also suggested the possibility of cooperating, adding that:

The Cowles Commission is envisaging a somewhat more systematic research effort into the mathematical and economic theory of linear programming. I should like very much to discuss these plans with you at some time in the near future, also in order to learn what your plans are and to explore how to increase cooperation and avoid duplication of effort.

Koopmans ended his letter by asking Leontief's opinion of Georgescu-Roegen's mathematical ability, as he saw him suited to work in the Cowles Commission new orientation that he had underlined. 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 Koopmans that he was going to receive government funding, which led to the creation of HERP.

Koopmans visited Harvard again in November, and in December 1948 they were both at the annual meeting in Cleveland, where the discussion started heating up. Leontief talked about the "application of the economic theory of general equilibrium to empirical quantitative analysis of the concrete national economy," and he presented his approach as a way to go beyond partial equilibrium theories to say something in the "pragmatic language of active policy making" (Leontief, 1949a: 211). In his presentation, he took the time to dismiss both atheoretical empirical analysis (Leontief, 1949a: 214) and highly aggregated analysis (Leontief, 1949a: 218); but he also remarked, in a nod towards Koopmans, that one of the "most promising developments in pure theory of production is the study of maximizing and minimizing choices

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<sup>10</sup> Letter from Koopmans to Leontief (September 27 1948), Box 4, Folder "Cowles Commission," WLP.

between discontinuous, discreetly defined, alternative sets of linear input ratios” (Leontief, 1949a: 221), a subject to which a special session was devoted during the meeting.

The discussion that followed was overwhelmingly critical in tone, with most critics concentrating on the assumption of fixed coefficients of production. In addition to the criticisms, Leontief was aware that the Cowles Commission and its affiliates were presenting input-output models as a special case of linear programming. During the linear programming session, Dantzig presented an “elaboration of the ‘input-output’ model developed by Professor W. Leontief and the Bureau of Labor Statistics” with an emphasis on dynamics and multiple production processes from which the best solution was “selected by maximizing an objective function” (Econometric Society, 1949a: 160). In another session, Herbert Simon, who was 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, 1949a: 173).

Koopmans himself participated in the discussion of Leontief’s paper, and he was rather critical, although he did not stress the problem of the constancy of production coefficients, but instead looked for the “points of contact” between Leontief’s models and “certain sections of current economic theory” (Fabricant et al., 1949: 234). While Leontief had insisted during his presentation on the general interdependence embodied by his analysis, Koopmans took issue with the idea that input-output analysis could be conceived as “general equilibrium analysis,” arguing that “This term usually connotes analysis of the process whereby the exercise of optimizing choice by each of a number of individuals or firms brings about determinate prices and quantities. In the model that has been presented there is no study of choice” (Fabricant et al.,

1949: 234). Koopmans thought that the model developed by Leontief was more akin to a planning model, and sought to draw the similarities existing between this approach and the theory of resource allocation that he was himself developing at the same time, and which was discussed during the session on linear programming. By referring to the “theory of allocation of resources developed by Meade, Lange and Lerner,” Koopmans underlined again the difference between an approach of economic policy based on welfare considerations and optimization, and that of Leontief, for whom the choice intervened only after the input-output analysis.

By then, it was apparent that the work of at least three different groups was overlapping: the input-output approach of HERP, the activity analysis of the Cowles Commission, and linear programming at 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 activity analysis conference which took place in June, but he also actively tried to poach some of the economists working with Leontief at HERP. In particular, it seems that Leontief did not take well the offer made by the Cowles Commission to Georgescu-Roegen, who was one of the frontline soldiers of HERP; in late January 1949, a few weeks after the Cleveland meeting, Leontief told Koopmans as much, after he enquired again about the status of Georgescu-Roegen in relation to HERP:

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>11</sup>

It was against this background that the Activity Analysis conference was prepared. Disagreements notwithstanding, Leontief was still planning to attend the conference in May:

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<sup>11</sup> Letter from Leontief to Koopmans (January 28 1949), Box 4, Folder Cowles Commission, WLP.

“Georgescu will definitely come to the Linear Programming Conference, and I hope very much that I will also be able to attend although this will mean sacrificing a large part of my very short summer vacation.” Koopmans invited him, along with Armen Alchian and Dantzig, to chair the committee preparing the conference, and plans were going well until tragedy struck: Leontief was hospitalized after falling ill with what his secretary called “virus pneumonia” in early June. Georgescu-Roegen was apparently uncomfortable at the prospect of going alone to the conference, which can be explained in light of Koopmans’ offer. He asked Leontief permission to attend, and, from his hospital bed, Leontief insisted that he went.<sup>12</sup>

Georgescu-Roegen was thus sent as the lone representative of HERP, although Leontief was not without allies as both Evans and Hoffenberg from the BLS presented papers during the conference (they were not published in the proceedings however). The conference itself has been the object of other studies (Backhouse, 2012; D uppe and Weintraub, 2014), and it has been generally noted that input-output analysis was frequently cited by the participants, however what is less noted is that it was referred to as a special case of linear programming and activity analysis. Four papers presented during the conference were also focused on the so-called non-substitution theorem, by Georgescu-Roegen, Samuelson, Koopmans and Arrow. This theorem was important for Leontief and it is not anecdotal that it was proposed by Georgescu-Roegen, as it proved that the limitational assumption, that there is one production process per commodity, was not a problem in the Leontief model: “As Samuelson expresses it, although alternative processes may exist, only one is actually used and, therefore, only one can

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<sup>12</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.

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.

Two points should be emphasized from the interactions between Leontief and the Cowles Commission: first, linear programming was intrinsically linked to the idea of maximization, and thus to welfare consideration once it evolved into a theory of general equilibrium relating competitive equilibrium and pareto optima. Second, the way in which activity analysis was developed made input-output analysis a subordinate theory, because it concentrated on Leontief’s theoretical model, emptying it of its main concern, the collection of data. Both of these points explain Leontief’s increasingly difficult relations with Koopmans and his group.

On the first point, 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. As we saw, Leontief was pushed to clarify his position about the role of choice in economic models, which he did during the 1951 linear programming conference when he argued that the problem of choice should not be posed as a choice between different combinations of production processes, but as a choice of alternatives based on the different scenarios that could be produced by an input-output model.

On the second point, the idea that activity analysis was a theory more general than input-output analysis must have looked completely besides the point to Leontief, who had always been adamant in his opposition to theories without empirical content. In a letter to John Hicks in



the early 1950s, Leontief made clear the difference between the program pursued at HERP and at the Cowles Commission, after Hicks enquired where to send his students:

They [the Cowles Commission economists] 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 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>13</sup>

This approach was not limited to the Cowles Commission, but to most other researchers who worked on linear programming; in a session sponsored by the American Economic Association at the 1949 New York meeting, Charles Hitch, the head of RAND, showed that this was a prevalent opinion when he casually talked about a “promising production function technique ... ‘linear programming’ or its variant, the Leontief input-output matrix” (Hitch, 1950: 198).

How could Leontief have seen his model as a subset of linear programming? He had always been clear that the simplicity of the model was necessary both to simplify the astronomical task of collecting interindustrial data, and also to be able to obtain a solution from these data; it was already a difficult task to get an inverted matrix from the early computers built by Howard Aiken at Harvard, without having to take into consideration more difficult shapes of production functions. The resentment that Leontief let out in the following years is much more understandable when one thinks of the years of careful collection of data which were brushed aside in just a few years by an “intrepid band of modern welfare theorists precariously perched ... high above the ground of factual experience” (Leontief, 1955: 250).

One economist that was not perched too far from factual experience was Lawrence Klein; this did not mean that he was on friendlier terms with Leontief.

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

## B. Input-output analysis against macroeconometrics

Klein, as the master builder of a macroeconomic model of the United States during his time at the Cowles Commission, was the leading economist trying to weave together the statistical analysis of economic time series and the economic modeling that had sprung up from the interpretation and importation of Keynesian ideas. As such, it is not surprising that he became one of Leontief's targets, and that he was the most interested in his criticisms against aggregation, statistical inference in time series, and macroeconomic policies.

As soon as the war was over, during the first postwar economic meeting which took place in Cleveland, Klein presented his macroeconomic system of the United States, in the same session where Evans, Cornfield and Hoffenberg, all economists of the BLS, presented their input-output analysis of the US economy (Econometric Society, 1946: 159-163). Postwar reconstruction was the main theme of the conference, and Evans regretted that Leontief was not here to join the discussion: "I was sure that you would be there to join in heckling the postwar discussion. As a matter of fact, we could have used a good solid heckler."<sup>14</sup>

During the next meeting in Atlantic City (January 1947), Leontief was absent again but Klein presented a paper on the "Use of Econometric Models as a Guide to Economic Policy" which showcased how different his views on models and economic policies were different from Leontief (Klein, 1947: 112). Klein presented 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

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

were also based on behavioral equations relating for instance consumption and income or profits, private production and savings. The importance of individual behavior for the construction of the model and the relationship of this problem with aggregation questions was addressed at the same time by Klein in several papers (Klein, 1946a,b), and offered another contrast with the purely technical relationships developed by Leontief in his input-output analysis.

A few months later, during the September 1947 Washington meeting, Leontief aired his grievance against Keynesian theory and the “device of aggregative analysis,” arguing that “[a]nybody who was concerned with the practical application of econometric analysis, I think, is conscious of the fact that in a large number of instances, these aggregative measures are not very useful,” especially for the problems of economic planning and policy-making (Leontief, 1949b: 274). This was not a view that was shared by every economists; during the following winter meeting in Chicago, problems of aggregation featured prominently in the discussions of several sessions, including one on the “Relationships Derived from Aggregate Data” chaired by Samuelson (Econometric Society, 1948a: 202-204).

The next year, both Leontief and Klein attended the December 1948 meeting in Cleveland, and Leontief did not shy away from expressing his views on aggregation theory. He clearly stated during his presentation that he viewed statistical inference on time series and the aggregation of variables as two sides of the same coin:

It is easy to understand why aggregation and correlation go together. Direct observation can have only very limited use in discovery and explanation of quantitative interdependence of highly aggregative quantities. The very process of aggregation obscures the sharp outlines of the underlying structural relationships to such an extent that one is naturally forced to give up the simpler methods of direct induction and take recourse to "blind flying" by the complicated but hardly fool-proof instruments of indirect statistical inference. (Leontief, 1949a: 218)

It is hard not to read Leontief's comments as a direct attack on Klein's methodology; Klein could also not ignore that Leontief was exhorting economists to abandon macroeconometrics:

The progress of empirical general equilibrium ... analysis will to a large extent depend upon our ability to eliminate highly aggregative procedures which have dominated this field since the middle thirties; to be more precise, since the emergence of various attempts at the empirical verification and numerical application of different Keynesian models. (Leontief, 1949a: 218)

During the discussion, in addition to his comments on general equilibrium, Koopmans had defended the tools of statistical inference which had been an important part of the Cowles Commission macroeconomic program during the past few years, arguing that Leontief's dynamic model would reintroduce the same difficulties (Fabricant et al., 1949: 234). Klein was present during this discussion, and it was inevitable that the two economists would eventually collide more frontally.

The occasion for a direct clash finally occurred in November 1949, when Leontief was invited to discuss Klein's paper on "Investment in econometric models" during the upcoming NBER conference on Business Cycles. 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>15</sup> Klein's presentation was an in-depth analysis of current studies in investment behavior, with applications to the railroad industry and the electric light and power industries in the United States (Klein, 1951). His approach was to estimate the relationship between investment explained by operating income, the stock of fixed capital and the interest rate on new bonds of a particular industry. Several variations were tested and Klein discussed extensively his results, their accuracy, and the explanatory power of his hypotheses.

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<sup>15</sup> Letter from Geoffrey Moore to Leontief (July 1 1949), Box 6, Folder "NBER Conference," WLP.

In his comments, Leontief focused on the role of output changes on investment, distinguishing this relation from Klein's hypothesis that investment depended on income. Leontief referred to studies conducted at HERP by Chenery to question "Klein's radical decision to disregard entirely the change in the output of an industry as a possibly important factor in the analysis of the rate of investment in that industry" (Leontief in Klein, 1951: 311). This was presented by Leontief as his substantive point, but given that his second criticism was addressed at the basic methodological principles adopted by Klein, it probably had a deeper effect on the tone of the conversation:

I submit that, in turning to the indirect statistical estimation of parameters of his final explanatory equations, Klein neglects almost completely, and he could not do otherwise, all the various important considerations enumerated in the introductory sections of his paper. Technical changes, and the replacement requirements, for example, to mention two of the most significant of these explanatory factors, are left entirely out of the final analysis. To be explicitly used in actual explanation, these factors would have to be not only mentioned but systematically described in concrete quantitative terms, admittedly a task of no mean proportions but which, I suppose, will have to be accomplished before one proceeds toward a real interpretation of the investment behavior of the American railroads or of any other industry. I surmise that, once this indispensable factual information has been collected and put to work, it will also carry the main weight of the actual explanation, while the indirect statistical inference used by the author as the principal operational basis of his analysis will be relegated to a secondary, albeit still important, role. (Leontief in Klein, 1951: 310)

In his answer, Klein recognised that both he and Leontief were interested in detailed structural informations, but noted that:

We are, however, at opposite poles on the problem of mixing this information with methods of statistical inference. I interpret Leontief as arguing that information obtained from engineers, legal sources, and the like should be dominant in the empirical determination of investment behavior equations. I offer the countersuggestion that this sort of information is invaluable as far as it goes, but that it leaves us hopelessly distant from any useful objective. ... Even in the case of the estimation of production functions, where technical engineering information carries its greatest weight, there are very few processes that could be estimated from such information alone, without the aid of statistical inference. (Klein, 1951: 314)

Klein argued that direct information collected from engineers or other experts was only useful so far as it helped the economist to identify which variable he should select in his model. He

noted that the problem of identification was inherent in the nonexperimental observations used by economists, and that “Leontief in his approach must unfortunately face the identification problem also” (Klein, 1951: 315). The arguments published in the proceedings were certainly as much the result of the actual discussion as they were the consequence of the letters exchanged between Leontief and Klein in the weeks following the conference.<sup>16</sup>

A few days after the conference had ended, Klein wrote to Leontief that he had not had the time to go into more details during the discussion, but he was visibly annoyed by Leontief’s remarks as he sent a long explanation justifying the use of operating income rather than output in the explanation of investment decisions, arguing that “I considered the whole matter as a small point and would not bother to go into it further except for the fact that you have made such an issue of it.” In his answer, Leontief criticized the shape of the production functions used by Klein, but he made clear that this point was in any case subordinated to his more general rejection of the inferential methodology:

My reasons for making a basic issue of this matter, rather than dismissing it as you do ‘as a small point’ are these. Your selection of profits as the central variable and the justification, or rather the lack of justification, of this choice is symptomatic of the statistical formalism combined with analytical pragmatism characteristic of certain sections of the recent econometric literature.

Leontief then proceeded to itemize his grievances against the econometric approach developed at the Cowles Commission, rejecting:

(1) The reliance on symbolic and only superficially, or should I say nominally, operational theoretical models. ... (2) A reluctance to utilize, and an even greater unwillingness to seek, direct and detailed factual evidence ... (3) Undue emphasis placed on qualitative (shapes of functions) and quantitative (magnitudes of coefficients) conclusions reached through application of various more or less elaborate methods of indirect statistical inference applied to very limited sets of empirical data under highly adverse experimental conditions.

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<sup>16</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.

Leontief ended his letter almost apologetically, arguing somewhat ironically that he had so far restrained himself from being too controversial in his “impassionate plea for realistic analysis”:

I have abstained from stressing it in controversial form in print or from a platform because I know well how gleefully such statements are received by the proponents of pure theory without facts, on the one hand, and the advocates of pure facts without theory, on the other. By now, however, the econometric approach has reached a state of maturity at which a serious affliction cannot be any more dismissed as an awkward but harmless symptom of youthful exuberance.

Klein was outraged by this letter and replied to Leontief upon receiving his diatribe, arguing that he was making use of all the information at his disposition to identify the relevant variables in his model, before airing his own grievances against Leontief’s accusations:

I think that it is very unfair of you to charge that I or other econometricians do not use factual evidence as a main basis for model construction or that we rely on sets of data that are too limited. I am willing to use any outside information available to help construct satisfactory models. ... Since we all work widely with linear systems in empirical studies, what more can we do than let this factual evidence lead us to a choice of variables and to an identification of equations. You should be the last person in the world to raise an issue like this. Some day I hope that you can explain to me the identification of relationships in your linear input-output schemes.

It is clear from his letter that Klein was tired of hearing the same criticisms from Leontief, as he noted that Leontief had defended the same position for some time now:

At the winter meetings in 1948, I was well aware of your feelings on the basis of what you had to say concerning the input-output analysis. It seems to me and a few others that you were saying the same things last month at the Conference. I regard this as highly unfortunate. I have always admired and supported your approach even arguing for its adoption in some countries where I thought it could be of immense use, but I never regarded it as an alternative to econometric model-building. The two should be complementary, each one handling problems that are outside the scope of the other. 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.

The idea of a complementarity between input-output analysis and macroeconometrics was something Klein cared about, as one who knew how much his own analysis was one of demand while Leontief’s was focused on supply. Years later, in his 1977 American Economic Association

Presidential address, Klein was still promoting a reconciliation of Keynes and Leontief (Klein, 1978a,b), at a moment when supply-side economics came back with a vengeance.

Despite Klein's plea for complementarity, upon receiving his answer Leontief put an abrupt end to their discussion, arguing that "[t]his 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>17</sup>

There were still some exchanges between the two economists in the following years, as Klein worked on the interpretation of Leontief's system in addition to his own empirical work; he also asked Leontief to visit him at the Survey Research Center in Michigan in early 1952 and the visit led to a cordial exchange between the two; but Leontief was clearly not interested in anything resembling a collaboration, and he continued in the following years to air his grievances against statistical inference, macroeconometric models and empty theorizing. In his review of *Studies in the Structure of the American Economy* (Leontief, 1953), 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)

The tone in his review was accordingly rather dismissive, even though he underlined the valid effort to expand the analysis towards a dynamic version of the basic scheme. Something to note in the 1949 exchange with Klein is that in the clash of two different conceptions of econometrics and empirical research, each tried to define the limits of econometrics as his own methods: this was obvious in Leontief's dismissive tone, but Klein also contrasted in his answers "econometric model-building" and input-output analysis, showing that he clearly identified his methods to

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<sup>17</sup> Letter from Leontief to Klein (December 20 1949), Box 6, Folder "NBER Conference," WLP.



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, who 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” when he asked him to write the chapter on econometrics of his upcoming *Survey of Contemporary Economics*.<sup>18</sup>

In the following years and decades, the rift between Leontief, the econometricians and economists only widened, even as input-output techniques spread in government planning agencies and were applied to burning questions in international trade and environment policy.

### III. Leontief’s entrenchment

During the second half of 1950, Leontief went to Europe where he met with the different European groups of economists working on input-output and econometrics: Frisch in Oslo, Tinbergen in Holland, Richard Stone in Cambridge, Hicks in Oxford... In September, he attended the first international conference on input-output which was organized following his initiative in Driebergen, by the Netherlands Economic Institute. Koopmans (a Dutch himself and an old student of Tinbergen) was the only representative of the Cowles Commission in attendance, and most of the other attendants hailed from central statistical offices and planning agencies in Ireland, Norway, the Netherlands, the United States and the United Nations in Geneva, as well as from university centers working on closely related topics in Paris, Oslo, Harvard, Kiel, Princeton, Cambridge (UK) and Amsterdam (NEI, 1953: vii-viii).

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<sup>18</sup> Letter from Ellis to Leontief (April 2 1947), Box 2, Folder “Books and Articles 1947,” WLP.

The advancement made in spreading input-output was summarized by Chenery, who was working in 1950 for the Economic Cooperation Administration (ECA, the US government agency in charge of enacting the Marshall Plan). Writing in November 1950 to Leontief about the results of his “conversations with Tinbergen and his group” Chenery seemed optimistic on the prospect of input-output for the organization of the European reconstruction, stating that

[w]e have made considerable progress in getting the Input-Output idea accepted, although the scale of the proposed operation to be undertaken initially has been reduced. The first memorandum was generally accepted in ECA as a basis for further discussion. The Dutch, as you will notice, were considerably more pessimistic but did agree that this was at a minimum a useful way to organize existing data and should be tried out in the OEEC.

Chenery also planned to spread the word in Italy where he was being transferred, although he was more pessimistic about their prospects in France. It is clear from his letter that Leontief had been advising the OEEC (the Organisation for European Economic Co-operation, the European equivalent of the ECA) on the possibility of using input-output, and that discussions among the planners of the European reconstruction were under way.<sup>19</sup> This led to the publication a few years later of the first input-output table at the scale of the European economy (Kirschen, 1958).

Leontief’s trip to Europe was a success, as input-output became the government planning tool that he was advocating for. He did not completely break with the economic profession but clearly made himself scarce in the following years in the meetings of the Econometric Society and the American Economic Association; it is telling that his famous “Leontief paradox” about the capital position of the United States in international trade was published in the *Proceedings of the American Philosophical Society* and not in an economics journal (Leontief, 1953).

Leontief met both with Klein and Koopmans again in the early 1950s, first when he went to the Symposium on Linear Inequalities and Programming held in Washington DC in June 1951,

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<sup>19</sup> Letter from Chenery to Leontief (November 14 1950), Box 4, Folder “C,” WLP.

where he presented his paper on “Choice and non choice models” which was not published in the proceedings. In early 1952, he was also invited by Klein to the quantitative research seminar organized at the Survey Research Center of the University of Michigan. In the late summer, Leontief went back to RAND, but the highlight of his year was surely the Conference on Income and Wealth held in October 1952 in New York. The conference focused on input-output studies, and initiated in the recognition that input-output analyses had absorbed a lot of funding, but “remained a controversial subject 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:

I venture the prediction that contributions made through this program to the understanding of particular industries, of the process of technological change, and like matters, which at this stage seem like unimportant by-products, will turn out to be the lasting and important contributions of the program to economic knowledge; that they will be with us long after the grandiose dreams of predicting by input-output analysis the detailed consequences of major changes in the economic environment have been abandoned. (Friedman in Christ, 1955: 174)

The conference apparently did not lead anyone to change their position, and this was compounded for Leontief with the drying out of governmental funding during the Eisenhower administration; it was only resumed in the early 1960s when the Kennedy administration came into power (Kohli, 2001: 191 and 207-208).

Chenery’s efforts in Italy paid off; the second international conference on input-output was held on the shores of Lake Como in the Summer of 1954 (Barna, 1955). This conference was another stepping stone in the spreading of input-output, and the reports of national experiences in Denmark, France, Italy, the Netherlands, Norway and the United Kingdom showed how much

progress had been made in just a few years. But there was a conspicuous gap with economists made evident by the lone presence at the conference of Harry Markowitz as a representative of the linear programming approach developed at RAND and the Cowles Commission.

After 1949, a pivotal year which brought to the fore of the academic debate the disagreements of Leontief with theorists and empiricists alike, there was a clear retrenchment of the Harvard economist in the ivory tower of input-output analysis. From the 1953 Gibbs lecture all the way down to the 1980s, through Leontief's 1970 American Economic Association Presidential address, his discourse on the rest of economic science changed little and his interactions with the rest of the profession withered down to harangues and exhortation to abandon the econometric tools that were developed much to his dismay.

Although his work was widely recognized, his bitterness seeped through his rejection of what had become the mainstream of econometric thought, at a time when he was facing the fact that econometrics had taken a path that he did not approve of. His 1970 Presidential Address in front of the American Economic Association was a scathing indictment of purely abstract models far-removed from any empirical reality and of contemporary econometric practice: "Alongside the mounting pile of elaborate theoretical models we see a fast-growing stock of equally intricate statistical tools. These are intended to stretch to the limit the meager supply of facts" (Leontief, 1971: 2-3). Leontief scorned the value scale that he believed had been put into place, and which ranked empirical analysis below "formal reasoning." He scoffed at the fact that "[d]evising a new statistical procedure, however tenuous, that makes it possible to squeeze out one more unknown parameter from a given set of data, is judged a greater scientific achievement than the successful search for additional information that would permit us to measure the magnitude of the same parameter in a less ingenious, but more reliable way" (Leontief, 1971: 3).

Leontief's bitterness was that of the scholar feeling deeply that he is right, and helplessly watching others shun, dismiss, neglect his work and the direction he was showing. This was compounded by the departure of many of his students, either because they could not get a permanent position at Harvard or because they agreed with at least some of the criticism addressed towards Leontief. Georgescu, the frontline soldier of 1948-1949 which Leontief called in the 1960s "Dear Puiu," a term of endearment literally meaning "Dear boy," moved to Vanderbilt during the summer of 1949 to secure his position; Robert Solow wrote to Leontief after receiving the text of his 1953 Gibbs lecture, and although he enjoyed it, he added that

To prove that there was something solid inside your velvet glove, I find many places where I disagree with you. Although, with you, I don't believe multiple regression to be the philosopher's stone, I am not so optimistic as you are about the success of direct empirical analysis. ... Also, I think I would rate the marginal productivity of pure theoretical speculation somewhat higher than you would, and that of extensive empirical work somewhat lower.

Finally, even Paul Samuelson, while he regretted that Leontief abandoned economists alone to find their way, could never break away from his master (Samuelson, 2004: 6-7). Writing to him after Leontief left Harvard to NYU, Samuelson wrote in his usual style, quoting Wordsworth:

Your impact on Harvard economics (and hence on world economics) has been phenomenal and permanent. Everyone knows that. ... when you first began to teach advanced economics ... "Bliss was it in that dawn to be alive, But to be young was very heaven!"

Somewhat shaken by this mark of appreciation, Leontief gives us in a few words the other side of bitterness, of years of fighting a lonely battle and seeing an immense success overshadowed by the failure of his goal to reshape economists' work:

Never has a letter addressed to me by a professional friend moved me so much as that received today from you. Thinking and exploring is a lonely occupation; and as you know so well, success tends to enhance isolation.<sup>20</sup>

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<sup>20</sup> Letters from Leontief to Georgescu-Roegen (March 9 1964), Box 23, Folder "Leontief," GRP; from Solow to Leontief (January 27 1954), Box 57, Folder "L," RSP; from Samuelson to Leontief (June 17 1975) and from Leontief to Samuelson (June 19 1975), Box 48, Folder "Leontief," PSP.

## Archives:

WLP: Wassily Leontief Papers, Harvard University  
PSP: Paul Samuelson Papers, Duke University  
GRP: Nicholas Georgescu-Roegen Papers, Duke University  
RSP: Robert Solow Papers, Duke University

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