Is the constancy of technical coefficients a matter of tolerance ?

A methodological inquiry about the justification of a controversial assumption 1936-1952*

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Abstract

Whereas activity analysis and linear programming appeared as direct competitors of input-output analysis, Koopmans and Leontief suggested two distinct interpretations and use of Walras' technical coefficients. While Koopmans clearly stated that the constancy of technical coefficients was not relevant as managers (rational maximizing agents) choose the optimal allocation of resources and the efficient combination of activities, Leontief justified the constancy assumption on a methodological ground. Answering to his methodological device, L.R. Klein concluded that Leontief's assumption was the result of a rejection of statistical inferences and revealed a lack of methodological tolerance. Within Leontief's methodology we try to distinguish between the class of legitimate data and the link between theoretical terms and data. A tension between two criteria, operational meaning of concepts and testability, is identified in this methodology.

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Introduction

When studying Leontief's methodology and philosophy of science during the post-war period, we found that his methodological articles (Leontief [1952a], [1953], [1958]) were always focused primarily against the Cowles Commission's methodology. At Cowles it was developed during this period two quite complementary scientific programs around pure theoretical economics and econometrics.

Leontief's relationships with the Cowles Commission, especially with Koopmans during his directorship (1948-1955)[1], were ambiguous and deteriorated by Leontief's rejection of the methodological program Koopmans exposed during the measurement without theory controversy. This disagreement appeared stronger during the linear programming conference Cowles Commission organized at Chicago in 1949.

Linear programming, input-output analysis, and activity analysis were presented during this conference as belonging to the same family, using constant technical coefficients and close mathematical tools. However, Leontief's letters show that he was very skeptical about the scientific goals cowlesmen were pursuing and he decided not to participate in the conference (**Part I.**). This context explains Leontief's 1952 article were he developed for the first time the methodology ruling his studies of industrial interrelationships. We argue that this article is a continuation of the measurement without theory controversy opened by Koopmans in 1947 against the NBER. However Leontief's position was against the methodology of the NBER *and* against Cowles Commission's methodology (**Part II**).

The debates about the assumption of the constancy of technical coefficients Leontief defended were a crucial stake of this methodological disagreement between Leontief and Koopmans. Indeed, Leontief based the justification of his assumption on methodological considerations. However we find nowhere a systematic development of his methodological and philosophical positions. An interpretation of his methodological rules is thus proposed here (**Part III**). This interpretation is focused on the central place Leontief gave to direct observations and explicit definitions when dealing with empirical studies of general interdependence of economic systems. "*Operational meaning*" of theoretical terms is the main criteria developed. This criterion is interpreted as the hard core of Leontief's methodology and, following a speculative path, we ignore many of his moderations.

[1] In july 1948 Koopmans became director of research of the Cowles Commission (and Alfred Cowles was president). See (Cowles Commission [1952]).

Finally we try to re-examine the way Leontief justified the constancy assumption according to our interpretation of his methodological rules (**Part IV**). A tension comes out between this criterion of "*operational meaning*" and testability, crossing input-output methodology: the first criterion eliminates most of "undesirable" concepts like normative criteria of rationality and efficiency but is too strong to permit the making of useful economic laws. Thence we need to introduce testability, a weaker relation between theoretical terms and data, but this time it appears that this concept may be too weak to sustain Leontief's critical statements against Cowles Commission's works.

I. Input-output analysis at Cowles Commission[*].

At the end of world war two, a successful approach of general industrial interdependences was experienced by Wassily Leontief and other economists of the war administration (especially the Office of Strategic Services and the Bureau of Labor Statistics[2]): these latter called it "input-output analysis". It was based on Leontief's "studies of industrial interrelationships" (Leontief, [1936b], [1937a], [1941]) developed at Harvard University (Cambridge).

During the post-war period, George Dantzig, who worked on input-output issues with the US Air Force, wanted to use the model of Leontief to resolve another problem. How do we use efficiently resources and technologies to reach a certain goal? Fascinated by Leontief's approach, Dantzig looked for new tools to "*generalize*" the model of Leontief. The meeting with Tjalling Charles Koopmans gave a concrete realization to Dantzig's program[3] and founded the American operations research. This approach gave rise to linear programming.

In July 1948 Koopmans was nominated, after Marschak, director of research of the Cowles Commission for Research in Economics (Chicago). Koopmans' first task was to find funds as the Cowles Commission faced financial difficulties (see Mirowski [2002]). Dantzig's mathematical problem and the interest of the US Air Force on it seemed to be a way to refund Cowles Commission. Therefore, Koopmans defined a new research program at the Cowles Commission and turned aside from Cowles Commission's first program focused on

[*] As the formal relationships between linear programming and input-output analysis are well known we don't develop this part.
[2] See (Kohli [2001]) about Leontief's collaboration with the Bureau of Labor Statistics.
[3] Neither Koopmans nor Dantzig gave an exact account of this meeting.

econometrics[4]. The new financial partner was in 1948 the Rand Corporation (Santa Monica)[5], and the new scientific program was “*linear programming*”.

Koopmans decided in 1949 to organize a conference on this new technique to be hold at the University of Chicago. The monograph of this conference, published in 1950, played a crucial role in the development of neoclassical economics and input-output analysis. We present here the role input-output analysis played in the early development of linear programming. This role is intimately linked to the scientific program Leontief on the one side and Koopmans on the other side were developing.

Dantzig and the model of Leontief.

As noticed earlier this was Dantzig’s interest in the open model of Leontief which permitted to develop linear programming. The mathematical model underlying linear programming was, according to Dantzig and Wood (both mathematicians at the US Air Force), a generalized model of Leontief (Dantzig-Wood, [1949a], [1949b]). The generalization was a way to give up Leontief’s strong assumptions about the production process: no alternative production process, no substitution between factors of production, no joint production and no dynamic analysis. These assumptions were considered as the fundamental limits of input-output analysis.

In 1949, Dantzig and Wood pointed out that “*the first steps toward the required analysis of interindustry relationships have been taken by Professor Leontief and by the Bureau of Labor Statistics* (…). *Theoretical work now under way by several groups will make it possible to handle these relationships dynamically and with due consideration of alternative procedures*” [1949a]. To do so, the generalized model is “*closely related to the one formulated by von Neumann.*”

Based on these models, linear programming was defined as the research of an optimal solution for a linear program. This was first of all a mathematical technique and the main method of linear programming, the simplex method, was applied to very different problems (nutrition, transportation, allocation of resources etc.).

Dantzig presented linear programming as a generalization of Leontief’s approach but it could be defined first of all as a technique used to solve mathematical problems of

[4] Cowles Commission was founded in 1932 by Alfred Cowles to support to the Econometric Society.
[5] The Rand Corporation (which name is a contraction of Research and Development) was close to US Air Force.

optimization. Koopmans gave another interpretation of linear programming not founded as a *technique* but as an *explanation* of production.

Koopmans and activity analysis.

With Koopmans' activity analysis, linear programming was not only a technique but also a foundation of the general equilibrium theory of production. This theory of production was the result of his use of input-output analysis, linear programming and a renewal of the debate between Pareto, Walras and Barone on the question of the constancy of technical coefficients. Like Leontief, Koopmans built an open model, used matrix algebra and constant technical coefficients to describe the relations between inputs and outputs. These relations were organized thanks to a "technological matrix"[6].

In contrast with Leontief, Koopmans interpreted the production function of Leontief[7] exclusively as a technology[8] and pled for a more general function where substitutions between factors of production would be possible. Koopmans noticed that "*it has long been realized that the concept of a production function representing a given productive "technique" is unnecessarily restrictive. The "technique" employed in production is itself the result of managerial choice (going beyond the discarding of unwanted factor quantities). Managers choose between, or employ efficient combinations of several processes to obtain in some sense best results"* (Koopmans [1951], p.34). Therefore the form of the general "*production function*" was explained by the individual behavior of managers.

The manager is a rational acting agent, who maximizes a predefined objective function. Managers choose the best combination of factors of production in order to reach the "*best result*". As a consequence the combination of factors of production is changing according to the state of affairs (the bill of goods, prices etc.). Because of this choice, if technical coefficients describing a technology are constant, "technical coefficients" describing the best combination of factors (chosen by the rational manager) are variable. Substitutions between production factors are therefore explained by the rational behavior of managers. According to Koopmans this assumption is normative when used for policy making but "*is descriptible of reality if and only if the assumption of efficient choice is a good approximation of reality*" (*Ibid*.), and it seems clear that for him it is descriptible of reality.

[6] Koopmans didn't used the concept of "industry" like Leontief but defined a concept of "activity". An activity is a vector of technical coefficients describing the inputs and outputs engaged into this activity.
[7] We discuss the economic concept of production function and the mathematical concept of function latter.
[8] While we find in Leontief [1937a] more attenuated interpretations.

Inside activity analysis, the managerial problem is defined as the research of an efficient allocation of resources. To produce final commodities (the ones desired), managers have to use inputs (primary factors like labor and services of land, and intermediate commodities that are produced) and technologies. An efficient point in the commodity space is reached when there are no ways to increase the output of one commodity without decreasing the output of another commodity. The set of efficient points thus obtained is the solution of a program of optimization and the resulting "*production function*" is then explained by individual behaviors. One no longer postulates the constancy of observed technical coefficients.

Input-output analysis, a particular case of activity analysis?

Koopmans showed that the constancy of observed technical coefficients (Leontief's assumption) is valid only in a particular case: if labor is the unique primary factor, in an open production model without joint production, and even if it is possible to make substitutions between activities, the optimal combination of factors of production will remain unchanged after a variation of demand or of total amount of labor available. This is the theorem of non-substitution first formulated by Samuelson[9].

If we represent a Leontief production function by its isoquants, we obtain curves of the form shown in (fig.1). Technical coefficients, which measure the quantity of a factor used to produce one unit of an output, are assumed to be constant. This means that factors of production are complementary: to increase the volume used of only one input induces a zero increase of the final output. All the "*optimal*" combination of factors will be observed on the line (OA). It follows that the marginal productivity of each factor equals zero (Leontief [1937a]). Even if we don't need to precise the mathematical formulation of the production function (as technical coefficients are self-sufficient, like in Walras' *Elements d'Economie politique Pure*[10] [1874-1877]), it can be put under the form $y = \min(..., \frac{x_{ij}}{a_{ij}}, ...)$[11]. The proprieties of the "*production function*" (of Leontief) are thus based on the constancy of

[9] This is the Samuelson non-substitution theorem. We find in (Koopmans [1951]) demonstrations by Samuelson, Koopmans and Arrow. Samueslon used a traditional open model of Leontief and postulated existence of a general mathematical (production) function. Koopmans and Arrow used activity analysis and they didn't need a mathematical production function.

[10] See (Leontief [1937a]), (Koopmans [1951]), (Akhabbar, Lallement [2005]).

[11] y= output of a commodity *j*; x_{ij} is quantity of an input *i* used to produce *y* of output *j*; a_{ij} is the minimal quantity of input *i* necessary to produce one unity of the output *j*.

technical coefficients, and what we call the production function is in fact a set of relations between inputs and outputs.

If we imagine a more general production function which allows substitutions between factors of production, its isoquants appear to be convex curves (fig.2). But in a static open model of Leontief, with only one primary factor (labor), the observed combination of input belongs to the same line (OA) as if we had strictly complementary factors (like on fig.1). That is to say that "*even if substitution is physically possible, it will be ruled out on economic grounds*" (Dorfman, Samuelson, Solow [1958], p.249).

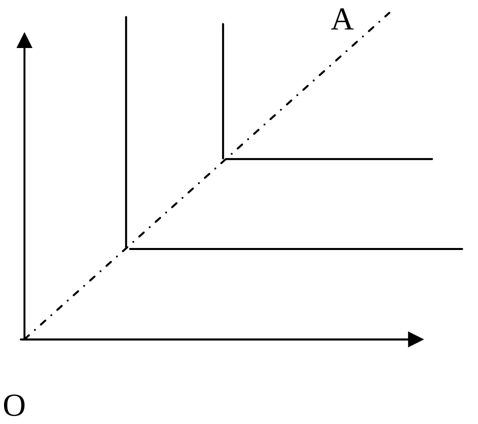

Fig.1

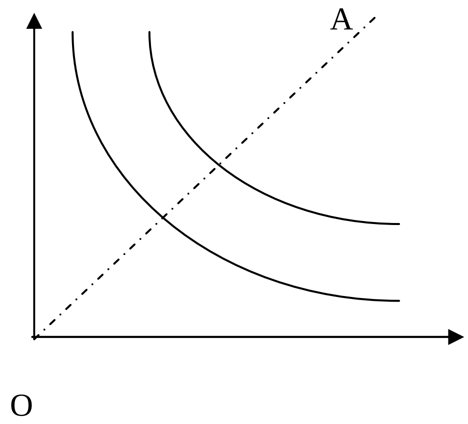

Fig.2

This theorem was presented by Samuelson and Koopmans as a way to attenuate Leontief's assumption of the constancy of technical coefficients, but it was also interpreted as the evidence that the open model of Leontief was a particular case of a more general theory: activity analysis. On the one side Koopmans, Samuelson and Arrow demonstrated that Leontief's system is compatible with the late classical walrasian theory and, on the other side, that input-output models of Leontief are particular cases of this classical theory[12] (constancy is observed only in the conditions described in the non-substitution theorem).

From all this it follows that activity analysis is the continuation of input-output analysis as they are both based on constant coefficients and use matrix algebra in open models. But, at the same time, activity analysis justifies substitutions between factors of production (and finally rejects the production function of Leontief: observed technical coefficients are not

[12] See for discussion about the substitution problem and marginal productivity theory (Schultz [1932]), (Georgescu-Roegen [1935]).

constant) and introduces a model of rational individual behavior[13]. This last assumption introduced a split between input-output analysis and activity analysis. With a maximizing manager, Koopmans defines normative criteria of *rationality* and of *efficiency*, and thus activity analysis appears as a continuation of the mathematical research on walrasian general equilibrium theory.

General equilibrium theory, activity analysis and input-output analysis.

Activity analysis is then the mix between the model of Leontief (and the *generalized* model of Leontief build by Dantzig and Wood) and the tradition of the "Lausanne school" Walras founded. Indeed, Koopmans introduced the monograph of the linear programming conference by quoting the historical sources of activity analysis:

> *(1)The mathematical research on the walrasian general equilibrium theory by Cassel, Neisser, Stackelberg and Karl Menger's mathematical seminar.*
> *(2)Welfare economics: Pareto, Barone, Kaldor, Lerner, Lange, Bergson etc.*
> *(3)Leontief's studies of industrial interdependences.*
> *(4)Dantzig and Wood's model of allocation and programming models.*

Activity analysis appears like a synthesis of these researches following the line drew by Walras and Pareto: rational individuals produce commodities "efficiently" and make substitutions between factors of production.

From all this it follows that if input-output analysis is a particular case of a general theory, this theory is the theory of general equilibrium that Koopmans, Arrow, Debreu and McKenzie were reformulating. The *Monograph 13* of the Cowles Commission (monograph of the 1949 conference) was a crucial step toward a reformulation of the walrasian general equilibrium theory. As Roy Weintraub noticed ([1983], p. 27), "*by mid-1949, and certainly by 1950 (…) the problem of showing the existence of a competitive equilibrium was accessible. The work remained to be done*"[14].

The work was done between 1950 and 1954 around the Cowles Commission. Nevertheless, if input-output analysis was omnipresent during this period, even at the Cowles

[13] Use of convex sets theory doesn't imply meaningful changes compared with the assumption of rational behavior.

[14] The theory of game was already intimately linked to linear programming. Moreover, Dantzig asked von Neumann, before meeting Koopmans in 1947, about mathematical tools to use in linear programming problems.

Commission, from 1954 on we find less and less references to Leontief in history of general equilibrium theory[15]. The reasons of such an elimination of input-output analysis are to be found in a methodological disagreement between cowlesmen[16] and Leontief: they are not playing the same scientific game. Once activity analysis developed, input-output analysis appeared as an accident on the road toward general equilibrium theory[17]. This was the ambiguous situation of input-output analysis at Cowles Commission. Lawrence Klein[18] reminds: "*I found the Cowles group a little flippant about Leontief' work… One day I met Kenneth [Arrow] in the hall and I said something favourable about the Leontief system… Kenneth replied that he thought this was just an accounting system. To people at the Cowles, accounting identities were something to take into account but they did not get one anywhere*" (Klein, in Mirowski [2002], n.47, p.268). We have now to define the scientific games Koopmans and Leontief were playing, which w ere the methodological stakes of the linear programming conference.

II. Methodological moves at Cowles: Leontief and the linear programming conference.

Linear programming and activity analysis resulted from a methodological and philosophical redefinition of Cowles Commission's scientific program under Koopmans' directorship (1948-1955).

When Cowles Commission worked on input-output analysis, this was according to Koopmans' ideal theoretical science and against empiricism. What were produced were new theorems and assumptions and not explanations of new facts. After reading the monograph of the linear programming conference we may be convinced by the powerful laws deduced from these new assumptions but we have no idea of the empirical value of such assumptions. This conference may symbolize the end of the era of econometrics at Cowles (See Armatte [2000]).

As Mirowski noticed (*Ibid*.), "*Koopmans remained indifferent to the computational implementation of linear programming and Cowles had become notoriously contemptuous of Leontief's input-output models, which had also enjoyed lavish Air Force support (…). Cowles*

[15] See, Arrow [1983], McKenzie [2002], Foley [1998].
[16] We find this term in (Mirowski [2002]).
[17] See n.23 p.12.
[18] Marschak brought to Cowles Lawrence R. Klein in 1944 (like Koopmans).

had also abandoned any support or encouragement of large-scale macroeconometric models, such as Klein's U.S. model (…). The "data" had given them [at Cowles] nothing but trouble in the past, as had their status as strangers in a strange land; it was now time to leave all that behind" (p.268-269)[19]. If we have to moderate this statement (in fact Koopmans' and Leontief's positions were more complex as we will see), it seems clear that with Koopmans, Cowles Commission would no more be interested in observation but primarily in theorizing general equilibrium theory. Under Koopmans' directorship, the official device of the Cowles is no more "*Science is Measurement*" but "*Theory and Measurement*". Koopmans revealed his methodological position during the measurement without theory controversy, which enlightens his interest in linear programming.

1. The measurement without theory controversy.

If at that time Leontief wrote no explicit article concerning the philosophy of input-output analysis, Koopmans was leading a methodological controversy against the American institutionalism between 1947 and 1949.

Koopmans opened the measure without theory controversy against the National Bureau for Economic Research (NBER)[20]. This controversy began with the report by Koopmans of Mitchell and Burns' book, *Measuring Business Cycles* (Koopmans, [1947]). According to Koopmans, this book was only an accumulation of data collected without any reference to theoretical concepts or assumptions about the nature of the phenomenon studied. As a consequence, the quantitative analysis of Mitchell and Burns, called "*empirical analysis*" by Koopmans, offered no explanation of cyclical fluctuations and possibly no explanatory models or plausible assumption. The debate that followed between Koopmans and Rutledge Vining (for the NBER)[21], if mixing very heterogeneous arguments, may be summarized in seven points (Vining, [1949]):

(1) Koopmans described the NBER as the Kepler of economics and stood up for deductive methodology. In contrast with the former Vining considered that the first task of the

[19] And Cowles Commission moved from Chicago (Illinois) to New-Haven (Connecticut) in 1955.
[20] The NBER (New-York) was directed by Mitchell and represented the centre of American institutionalism.
[21] See also the positions Mitchell adopted during the twenties (Mitchell [1925], [1928]).

scientist was to observe phenomenon. For Vining, explanation was the result of a long process of observation and theorization[22].

(2) Vining opposed a holistic view to Koopmans' methodological individualism. According to Koopmans, economists had to "*overhaul their theories of individual behavior (…) and express these theories in mathematical form (…). Next [mathematicians have to] perform such aggregations of individual behavior equations as would bring out the most important (aggregate) behavior parameters*" (Vining, [1949], p.91].

(3) Koopmans referred to a mechanical view of economies while Vining referred to the biological image of an organism[23].

(4) The rational maximizing individual was opposed to institutions and historical processes[24].

(5) Against descriptive statistics Koopmans proposed econometrics and the probabilistic approach developed by Haavelmo. According to Koopmans statistics and probability are useful to *test* theories[25].

(6) According to Koopmans, economics had to formulate normative propositions to guide economic policy whereas Vining underlined the presumptuous attitude of Koopmans.

(7) Koopmans, in a deductive approach, designed the scientific program of economics as the research of stable and universal laws (the structure of the economy), and Vining rejected the idea of finding such universal relations.

Koopmans' answer reflects what were becoming two different scientific programs in Cowles Commission' studies: the first, close to Koopmans, was the mathematical reformulation of economics (following the rules of axiomatic methodology), the second, with Haavelmo, was the statistical research renewed by the introduction of probabilities (we call it the theoretical-empirical program). The linear programming conference may symbolize Koopmans' will to give a more abstract direction to the research program of the Cowles Commission.

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[22] Mitchell's methodology can be compared with John Peirce's abduction.

[23] See (de Marchi [1993]).

[24] "*I think that in a positive sense the aggregate has an existence over and above the existence of Koopmans' individual units and behavior characteristics that may not be deductible from the behavior of these components parts*" argued Vining (Vining, p. 81).

[25] "*The excessive emphasis of modern statistics upon certain types of problems was stressed in a part of the discussion of a paper read by M.G. Kendall a few years back. Kendall had remarked that* "the estimation of proprieties of a population from sample is the most important practical problem in statistics and is long likely to continue so*". Mr. Yule denied this proposition:* "(…) The initial problem of the statistician is simply the description of the data presented; to tell us what the data themselves show. To this initial problem the function of sampling theory is in general entirely secondary or ancillary" (Vining, p.84).

Linear programming was then a way to found the general equilibrium theory of production based on rational individuals and defining a criterion of efficiency. The scientist had to make theories and laws expressed in mathematical form, and statisticians had to test against facts these theories and laws. But in Koopmans's views, one has few to wait from the second task compared to the rigorous flows of a mathematical reason. We find here the reasons of Leontief's absence in the early development of linear programming whereas input-output analysis was one of its main sources. But this absence is misleading if we don't explore its reasons.

2. Leontief at the door of the Great Analytics.

Before the linear programming conference[26].

Between 1947 and 1949, that is to say before Koopmans fully developed linear programming and contracted with the RAND to develop planning tools, Leontief and Koopmans had regular and cordial contacts[27]. During the year 1948, letters of Leontief and Koopmans revealed that they contemplated a close cooperation[28]. This cooperation seems understandable as Leontief felt at the end of the war the fragility of his research project in the federal administration. Moreover, the scientific program of the Cowles Commission (summarized by its device "*Science is Measurement*") seemed to coincide with Leontief's interest in empirical and theoretical concerns.

But at the same time Leontief was preparing the creation of a new laboratory at Harvard University (Cambridge). At the end of 1948, thanks to funds from the Rockefeller Foundation and the Ford foundation, and a contract with the US Air Force, there was the creation of the *Harvard Economic Research Project* (HERP). Creation of the HERP had been secretly prepared, especially negotiations with the US Air Force. As a consequence, when Koopmans asked Leontief about their common project, this latter answered that he faced a new situation[29]. This answer signified the death of the Koopmans-Leontief project: Leontief

[26] We refer to archival sources as follows: [HUG] for Leontief's archives (Harvard University, Cambridge); [TKP] for Koopmans' archives (Yale University, New-Haven).

[27][HUG. 4517.5, General Correspondence, Box: (1948-1950, C-G), Folder: "Cowles"]; [HUG. 4517.5, General Correspondence, Box: (1948-1950, I-M), Folder: "Linear programming conference"].

[28] [HUG. 4517.5, General Correspondence, 1948-1950, C-G, "Cowles"], Letter, T.C. Koopmans to W.Leontief, september 27 1948; Letter, T.C. Koopmans to W.Leontief, October 2nd 1948; Letter, T.C. Koopmans to W.Leontief, October 27th 1948.

[29] [HUG. 4517.5, General Correspondence, 1948-1950, C-G, "Cowles"], letter, W.Leontief to T.C. Koopmans, September 27th 1948.

created an independent laboratory and Koopmans was giving a new direction to the Cowles Commission.

Creation of the HERP will not however end the relationships between the Cowles and Leontief. Letters from Koopmans to Leontief showed that the director of the Cowles Commission tried to find a new way, at the HERP, to develop the Cowles and it appears that Koopmans viewed HERP and Cowles Commission as complementary. As noticed earlier, Koopmans defined with Dantzig's problem a new research program at Cowles Commission focused on linear programming. To attract the US Air Force (via the Rand Corporation), this new program corresponded to demand in planning tools and computer sciences like those developed with input-output analysis. We find here the possible reasons of Koopmans' regular questions to Leontief about the organization of the HERP, regardless Leontief's evident reluctance and irony.

In particular Koopmans visited Leontief's laboratory, encouraged exchange between cowlesmen and HERP members, and asked him to meet H.Aïken (responsible of computer technologies at Harvard[30]) to develop computing at Cowles[31]. But this *cooperation* annoyed Leontief who reproached Koopmans for making job propositions to HERP's economists[32]. Since the creation of the HERP, relationships between Koopmans and Leontief were getting worse and the linear programming conference revealed a deep disagreement.

The methodological stake of the linear programming conference.

After Koopmans invited Leontief to participate at the organization of the conference, this latter wrote to Marvin Hoffenberg (BLS) about his deep skepticism as concerns the Cowles Commission's scientific program[33]. In his letter Leontief reproached Koopmans and Cowles economists for their methodological positions. Moreover Leontief expressed the fear that the cowlesmen distorted input-output analysis. This means that Leontief's reluctance to participate at the linear programming conference, whereas input-output analysis was to be one of the central stakes of the conference, has to be understood on a methodological ground.

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[30] [HUG. 4517.5, General Correspondence, 1948-1950, C-G, "Cowles"], letter, W.Leontief to T.C. Koopmans, march 8 1948.

[31] A report of the Cowles indicates that "*computation tasks expected during the next year will include the inversion of high-order matrices, which can not be performed economically on desk calculators*" (Cowles Commission [1952], p.101).

[32][HUG. 4517.5, General Correspondence, 1948-1950, C-G, "Cowles"], letter, W.Leontief to T.C. Koopmans, January 27th 1948.

[33] [HUG. 4517.5, Correspondence general, 1948-1950, A-B, "BLS-Labor Economics Staff (Hoffenberg, Evans)"], letter, W.Leontief to Marvin Hoffenberg, December 6 1948.

From 1947 to 1949, Leontief didn't participate at the measurement without theory controversy. While the creation of the HERP and the linear programming conference seem to be an occasion for Leontief to explain his own methodology, he refused any responsibility in the organization of the conference and finally didn't participate at the conference. Leontief's non-attendance of the conference, if explained by methodological disagreements, is puzzling as input-output analysis was at the center of the conference program. Ultimately, the monograph of the conference published in 1951 (Koopmans, [1951]), if revealing the role of input-output analysis in the development of linear programming, didn't include any paper from Leontief (whereas Koopmans proposed Leontief to include in the monograph an article about the dynamic input-output model[34]).

It appears that Leontief's doubts about Koopmans methodological directions are realized when this latter used linear programming to renew the general equilibrium theory of production. The criticism of the constancy assumption and the solution Koopmans found, introducing rationality and efficiency criterions, made Leontief's assumption fragile: how not to see input-output analysis as a particular case of general equilibrium theory? To answer this, Leontief showed that the scientific rules he followed were those of another scientific game that the one Koopmans was playing. Following this point of view, HERP and Cowles are not complementary and Leontief's approach is an alternative to Koopmans' Newtonian deductive science.

A decadent concept.

Long before Koopmans' activity analysis, the assumption made by Leontief was controversial as it appeared to be a regression toward the short and dark times of general equilibrium theory, when marginal productivity theory was ignored[35]. But in 1949 it seemed that no reasons remained to maintain such a decadent hypothesis: activity analysis explained substitutions and used constant technical coefficients[36].

In the course of a session of the *American Economic Society* (1949) dealing with input-output analysis, the constancy assumption was seriously criticized and participant regretted

[34] [HUG. 4517.5, General Correspondence, 1948-1950, C-G, "Cowles"], letter, T.C. Koopmans to W.Leontief, August 2 1949.
[35] Walras assumed constant technical coefficients in the first editions of the *Elements of Pure Political Economy*, but after Pareto and Barone criticisms Walras assumed variable technical coefficients endogenously determined See (Akhabbar, Lallement [2005]).
[36] Moreover when Leontief repeated that with input-output analysis "*we are dealing (…) essentially with attempted application of the economic theory of general equilibrium*" (Leontief [1949]), we find few reasons to reject Koopmans' point of view.

the absence of a maximizing agent. Koopmans' activity analysis appeared for some as the general theory from which Leontief's model was a particular case[37].

In 1946, Leontief answered to such criticisms against the constancy of technical coefficients with a radical argument shifting the opposite arguments out of game: "*The assumption of fixed technical coefficients, which constitutes the basis of [this] empirical analysis, can be questioned* from the point of view *of general equilibrium theory of production. Insofar as the proportions in which the separate factors can be combined within the same production function (i.e., at any given state of engineering information) are variable, these proportions will most probably vary with every change in their relative prices. This theoretical proposition so clearly stated by Pareto in his criticism of Walrasian fixed coefficients of production is beyond dispute. It is not however the fundamental validity of the principle of substitution but its quantitative significance which is important* from the point of view *of empirical analysis*" (we stress, Leontief [1946], p.38-39).

Here Leontief answered using, apparently, the same ground econometricians used: one has to test assumptions and to measure the intensity of phenomenon like substitutions between factors of production. When using testability (or tests methods) to justify his assumption, Leontief is not convincing and his tests are vague[38]. In fact, we find Leontief very pessimistic about the usefulness of testability. His methodological criterion is elsewhere. However Leontief's 1946 answer indicated that to get the point we have to change our methodological glasses and our point of view.

According to Wittgenstein's theory of language games (Wittgenstein [2003]), this answer is a move out of the adversary game: what is meaningful or not depends on the rule of the game (language). As Leontief's methodological rules are different from Koopmans' rules, Leontief's justification of the constancy assumption is out of game. We now have to define the methodological rules of Leontief's empirical science

Indeed to justify his approach Leontief answered in 1952 on the methodological ground Koopmans chose against the NBER, but this time Leontief continued the measurement without theory controversy primarily against the Cowles Commission.

[37] But cowlesmen used only the input-output mathematical models and not the input-output tables, which are the clue of Leontief's input-output analysis.
[38] See (Leontief [1944]).

3. A move in the scientific game: Leontief's empirical science.

Koopmans' justifications of his assumptions about the form of the "production function"[39] are to be find in the tradition of general equilibrium theory and in its scientific device exposed during the measurement without theory controversy. Leontief's justification of the constancy of technical coefficients depends on the meaning of the concept of "*empirical analysis*". With his 1952 methodological article Leontief answered on the methodological ground Koopmans used during the measurement without theory controversy but, this time, Leontief opposed empirical analysis to Koopmans and Vining's positions.

Since 1952, it appeared that, for Leontief, discussing a mathematical model alone was meaningless: theory without measurement is meaningless. This is Leontief's strong philosophical position against Koopmans and the scientific program of the Cowles under his directorship. Against mathematical realism or pure rationalism, economics is defined as an "*empirical science*" and "*as long as the analytical economist is satisfied with "assumptions" rather than actual observations (direct and indirect alike), he is free to roam over the field of his inquiry more or less at random*" (Leontief [1953], p.7).

The first chapter of the *Studies in the Structure of the American Economy* (Leontief *& Al.* [1953]), first published separately in 1952 (Leontief [1952a]), is clearly a continuation of the measurement without theory controversy. Moreover, in a discourse for the *American Mathematical Society*, we find nearly at the same period (1953), useful comments (Leontief [1954]) and complementary propositions.

Leontief criticized, at that time, the purely descriptive approach, the purely theoretical approach and econometrics. This critical assessment was directed against the NBER (Mitchell, Burns, Vining), against the Cowles Commission which was representative of the abstract approach in economic theory (Koopmans, Arrow etc.) and in measurement (Haavelmo, Klein).

In contrast with Koopmans who separated theory from measurement, Leontief underlined that "*empirical implementation is considered as much a part of an economic argument as the consistent development of its logical consequences*" (Leontief [1952a], p.1). That is to say that data bring a part of truth into a logical (but empty) deductive chain. Making theories and mathematical models without specifying the link between theoretical concepts and observations leads to theories devoid of "*operational meaning*" (*Ibid*. p.3).

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[39] The "production function" has constant return to scale, allows substitutions between activities; it is based on the rationality of managers looking for efficiency.

Leontief argued against Mitchell and the NBER, that they "*developed occasionally in ignorance and often in defiance of abstract theoretical tradition*" (*Ibid*.) methodological procedures whose main tools are aggregation and averaging. However these indirect measures are actually already full of theory. Radical empiricists use them as if they were direct observations: when not dealing with direct observations, they are deceiving themselves. Data have to fill theoretical boxes or are vain.

In anyway, according to Leontief, the deductive approach and radical empiricism, "*has been recognized as one of the principal faults of economic science*" (*Ibid*. p.2).

The last "*school*", econometrics, is also guilty for using indirect observations. Indirect inferences, like averages, aggregates or indirect statistical inferences, bring no "*operational meaning*" to concepts and theories. Indirect observation leads to operate careless theorization and leads to theoretical dead-end (see Leontief [1954]).

From this it seems clear that Leontief distinguished between theory and (direct) observation, but if he clearly advocated the use of direct observation he didn't clearly explain the relations between theoretical concepts and direct observation[40]. Moreover it remains to explain the exact connections between the rules of empirical science and the constancy of technical coefficient.

Indeed it appears that a theory is said to be *operational* when direct observation fill in concepts and abstract relations. However by saying this we mix up two different problems*:* "*the relationship between theoretical and factual analysis*" on the one hand, and the class of legitimate data (direct observations or indirect inferences) on the other. Do we have to build operational meaning to economic concepts and, if no, why don't we simply try to find crucial tests against facts? If the word "*operational*" encloses the two problems (relationships between theory and observation, and legitimate data), we have to clarify the distinction between them[41]. To do so we reread Leontief's first articles in order to propose an interpretation of his methodological rules[42]. Thus we rebuild the justification of the constancy hypothesis on a methodological ground.

[40] We don't discuss here Leontief's theory of the internal structure of functional relationships (Leontief [1947]).
[41] The term "operational" is systematically used in this 1952 article.
[42] See for other recent points of view, (Bjerkholt [1995]), (Carter & Petri [1986]), (Davar [1994]), (DeBresson [1996], [2004a]), (Kurz, Salvadori [2000]), (Polenske [2004]).

III. The golden rules of empirical science

To investigate the role Leontief's methodology played in the justification of the constancy assumption, we distinguish five crucial methodological rules: *first*, concepts have to be defined explicitly using familiar terms and primary terms exclusively; *second*, the primary terms are direct observations; *third*, we have to use effective observations and not virtual observations; *fourth*, forecasts about directly observable phenomenon bring direct confirmation; *fifth*, theory must be logical and coherent[43]. In what follows we develop the justifications and the significance of the two first rules only (the third is implicitly admitted, the fourth and the fifth are out of question)[44].

In the 1952 article, Leontief insisted for the first time on the importance of direct observation. Even if we find no indirect inferences in the studies of industrial interdependence, Leontief didn't clearly advocate for direct observation during the thirties. Before 1952, Leontief insisted much more on the link between theoretical terms and data than on the value of observations. Therefore Leontief's 1952 article enlightens its previous methodological comments or articles (especially Leontief [1937b]). Our historical and philosophical interpretation of Leontief's methodology assumes that it is a coherent and homogeneous body. A three steps logical path is followed:

(1) The first papers on the studies of industrial interrelationships (1936-1937) contain a plea for systematic use of explicit definitions. It appears that observations are primary terms, but it is not specified the class of legitimate data;

(2) We find in his attack against implicit theorizing (1937) the formulation of the link between concepts and primary terms. These links are explicit definitions. But Leontief didn't precise the form of the primary terms to employ in the *definiens* (in the right side of a definition);

(3) In 1952 Leontief clearly stated that direct observation are the primary terms.

[43] We may say that rules (1) and (5) belong to syntactical rules of scientific language, when (2), (3), (4) deal with the application of language (semantic).
[44] Moreover we don't discuss the difference between indirect observation and direct observation. We admit Leontief's distinction.

1. Theoretical concepts and explicit definitions.

The temptation of emptiness.

Referring to J.H. Clapham's image (Clapham [1922]) Leontief noticed in 1936 that "*despite the remarkable increase in the volume of primary statistical data, the proverbial boxes of theoretical assumptions are in this respect as empty as ever*" (Leontief [1936b], p.105). The input-output table published in 1936, the rational accounting system he developed and the concrete calculus of technical coefficients brought the data to fill in these boxes[45]. Leontief explained that "*the statistical data collected (…) fill in the "empty boxes" of the theory of general equilibrium. Hypothetical production and consumption equations gain **explicit meaning** 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*" (*Ibid.*). This statement expresses the hard core of the philosophy and methodology of input-output analysis. This is according to Leontief the way to avoid the uncontrolled proliferation of theoretical concepts without any references to data. When they don't use explicit definitions economists manipulate empirically meaningless concepts: these concepts are empty boxes. This empirical methodology has to discourage scientists from the temptation of emptiness.

The scientific procedure then described is the one an explicit definition operates. Such a procedure is a very strong one: to accept a theoretical term inside a scientific theory we must be able to *replace* it by observational terms. This means that the theoretical term is synonymous with the measure operation. Leontief pled for such a methodology when he criticized the "neo-Cambridge" school (Leontief [1937b]).This article is a plea for the use of direct definitions against implicit definitions. According to admitted primary terms, any term must be defined by using exclusively these primary terms.

Primary terms and theoretical concepts: the role of explicit definitions.

In this attack of the "neo-Cambridge school" Leontief didn't make explicit the legitimate primary terms to be found in *definiens* but only the way one has to connect primary terms with theoretical terms: "*The outstanding characteristic of what for brevity will hereafter be referred to as the Cambridge pattern lies in a peculiar use of definitions. Within the*

[45] This task was far from being self-evident at the beginning of the thirties. H.L. Moore, for instance, noticed that "*one of the greatest difficulties in dealing with moving general equilibria [sic.] is the problem of the determination of the coefficients of production. In the works both of Walras and of Pareto, these coefficients are purely hypothetical…*" (Moore [1929], p.88).

structure of any theoretical analysis, we can distinguish two elements. The first comprises a set of fundamental statements, which are to be scrutinized within the body of the given theory to any large extent, only so far as may be necessary to verify their logical compatibility. The source of these fundamental postulates may be direct observation; or they may be derived as the conclusions of some other theory; or they may be inductively unverifiable (normative) postulate. The other part of a theory consists of a larger or smaller number of logical implications obtained from the primary set of fundamental propositions" (we underline, Leontief [1937b], p.340). Leontief insisted on the logical proprieties and on the cognitive value of direct definitions in contrast with implicit definitions where theoretical terms are introduced in the definition (instead of primary terms). Implicit theorizing uses intermediate definitions which are to be eliminated according to Leontief.

Against implicit theorizing Leontief quoted R.F. Kahn's concept of "*marginal utility of money*" (*m.u.m*). This one was said to be implicitly defined as its definition refers to another theoretical term, the "*average consumer*". We then have to find the definition of the second concept (average consumer) and we find another particular concept: index numbers. So, according to Leontief, Kahn used an implicit definition of the *m.u.m* using two other concepts…

When using implicit theorizing we can create new concepts indefinitely following a logical chain of definitions. The crucial question Leontief asked concerned the meaning of implicit theorems and terms. Moreover, we may introduce new theoretical terms (implicitly defined) only to save the logical consistency of a theory (See (Carter-Petri [1986])). To avoid such operations explicit definitions permit to define theoretical terms using only primary terms.

From Leontief's indications and the canonical form of an explicit definition, we define: If a predicate (Q) is applied to an object (x), we have an explicit definition if we get:

$$[D:] \quad Q(x) \equiv \dots x \dots$$

The *definiendum* is on the left side (Q) and the right side is the *definiens*. The aim of an explicit definition is to permit to replace the *definiendum* by the *definiens*. That means that they have to be synonymous (≡).This process permits to precise the necessary and sufficient conditions of the use of a term, and theoretical terms may be replaced by observed numerical values.

Moreover, when Leontief criticized Kahn's concept of marginal utility of money he also stated that the choice of primary terms is a delicate one. To use a primary term implies an existential belief. When referring to index numbers we express the belief in such entities: "*the* *strong believe in their existence constitutes one of the most important items in the credo of some present day economists*" (Leontief [1936a], p.39). The problem is that when "*assigned the task of measuring certain objects, the statistician became doubtful of their very existence*" (*Ibid*.). To build such statistical entities we need to make assumptions that are to be verified. From his work on index numbers Leontief expressed a radical skepticism on the possibility of founding explicitly such entities: "*what appeared to be a practical difficulty seems to reveal itself as a logical impossibility*" (*Ibid*.). In contrast with such indirect inferences, direct observation seems to be the only one on which a theory should be founded.

During the years 1936-1937 Leontief advocated, more or less clearly, for a systematic use of explicit definition using observations. But it remains to precise the nature and value of primary observational terms.

2. Primary terms and direct observations

Direct observations & cognitive value.

The 1952 article shows with little doubt that the primary terms which one has to find in explicit definitions are direct observation. Leontief's tolerance about the range of admitted primary terms disappears when defining the methodological rules of empirical science. If in 1937 he admitted, to focus his argument on explicit definitions, normative statements and theoretical statements, in his empirical science these latter have to find empirical meaning and the former are out of game.

When Leontief criticized the "neo-Cambridge" school on a methodological ground his argument was based on the *cognitive value* of a theory more than on the logical proprieties of a theory. As a consequence Leontief wrote that his methodology "*shall adhere to this specific distinction between logical and methodological aspects of the analytical procedure, bearing in mind that the latter is definitely psychological in its nature*" (Leontief [1937b], p.339).

Therein the value of direct observations and explicit definitions is firstly based on a cognitive element. Leontief's fundamental assumption is that a theoretical statement referring only to directly observable entities is more easily understandable than a purely theoretical statement: the cognitive value of direct observations justifies their exclusive use. Moreover direct observation's value comes from the fact that they are free from theoretical concepts as they *are* "*often observed by someone else rather than the economist*" and "*usually described* *in ordinary, every day language or in the technical language, not of economics, but of some other discipline*" (Leontief [1954], p.54).

As a first conclusion, the primary terms one has to employ are direct observations from which we formulate explicit definitions. If references to explicit definitions are regular (explicitly or implicitly) during the first period of the studies of interindustrial relationships (1930-1941), during the second period (1942-1954), input-output methodology was focused on the concept of direct observation. The two concepts are however closely linked. If the use of explicit definitions and direct observations is based on the ground of human cognitive capacity, this is also because economics is an empirical science which has to deal with complex systems. This link between the complexity of interdependences and methodology brings another justification of Leontief's methodological rules.

Direct observation & complexity.

Leontief's main argument in 1952 was the operational value of direct observation. This argument seems at first to be an answer to Koopmans and Vining's discussion about the role of descriptive statistics and probability. We argue here that the importance of direct observation is not backed up by an interpretation of statistical methods (like in the Koopmans versus Vining debate) but firstly by a general interpretation of national economies as complex systems[46]. We find there another justification of the use of direct observation.

The value of direct observations *and* the reference to a theory of general interdependence are indeed justified by the general paradigm of complexity:

(1) "*The principal merit of the general equilibrium theory lies in the fact that it enables us to take account of the highly complex network of mutual interrelationships which transmits the impulses of any local primary change into the remotest corners of the existing economic system.*" (Leontief [1937a])[47].

(2) When Leontief underlined that "*for a study of a set of quantitative interrelationships as complex as those underlying a modern economy indirect statistical inference, however refined methodologically, simply not do*", he referred to Warren Weaver's article on "*science and complexity*" (Weaver [1948]). According to Weaver, dealing with economic systems means to deal with problems of organized complexity, from which statistical methods are of no use. As interdependence of a great number of variables is the problem, to use indirect

[46] See on this point (DeBresson [1996], [2004a]).
[47] This will have to be developed elsewhere.

inference means to erase some of these interrelations, and Leontief's theory of the internal structure of functional relationships is a way to study these interrelationships (Leontief [1947]).

From this it follows that the general idea of "complexity" precedes "general equilibrium theory". Leontief is not *applying* general equilibrium theory, but studies real economies viewed as complex systems. Input-output analysis is a step in the science of complexity which is an empirical science. Therein the reader is disconcerted by Leontief's regular reference to input-output analysis as an application of general equilibrium theory. Indeed, at many times Leontief underlined the close link between walrasian general equilibrium theory and input-output analysis. In 1949 he repeated that with input-output analysis "*we are dealing (…) essentially with attempted application of economic theory of general equilibrium*" (Leontief [1949])[48]. The question here is not to distinguish between classical influences and walrasian ones but to define the word "*application*". If we interpret input-output analysis as a particular case of an *a priori* and deductive theory, Leontief's empirical science is no more the development of a new science (operational or empirical science) but the development of application techniques and we don't need to change our methodological glasses. That is to say that in this case his empirical science belongs to the same paradigm as econometrics.

Since Leontief noticed that this methodology leads to changes that are "*not of a tactical nature; it is not simply a question of better performance in one or another direction*" but "*requires redefinition of our general strategic objectives*" (Leontief [1952a]), the way he used the term "*application*" must be distinguished from the way, say H.L. Moore used it. What is to be founded is something much more general and evasive than Walras' general equilibrium theory: economics belongs to the very twentieth century "sciences of complexity".

There is then a close link between direct observation and the reference to a theory of general interdependence (as complex systems). Empirical science is something else than application of predefined theories. Moreover, economics as an empirical science is a science of complexity. To finish, dealing with complexity requires direct observations, and the rejection of indirect inference is not understandable in the Koopmans versus Vining controversy about the nature of the aims of statistics.

[48] Moreover, from 1936 on Leontief noticed that his statistical research "*has been undertaken with the definite aim of supplying an empirical background for the study of the interdependence between the different parts of our economy on the basis of the theory of general equilibrium*" [1936b]. In 1937 he underlined the walrasian flavor of the production function of the closed model. In the 1952 article, input-output analysis was defined as a "*simplified version of the general equilibrium theory*" (Leontief [1952a]) etc.

Leontief's criticism of implicit theorization is based on the same argument as the justification of direct observation. Actually, to study complex systems like national economies, we have to deal with complex interrelations between a big numbers of interdependent variables. The cognitive value of our logical chains depends on human being's capacity to deal with such complex systems: we have to distinguish between "*the point of view of Laplacian superhuman intelligence, which would be able to see without the least mental friction all the infinite number of logical implications of any given system of assumptions*" (Leontief [1937b], p.338) and "*a limited human intellect*" (*Ibid*.) As a consequence, the cognitive value of a theory depends on explicit theorizing methods: with explicit definition we must be able to define concepts using only primary terms. With Leontief's 1952 article we know that the primary terms are direct observations (as economics is an empirical science).

3. Structure, meaning and testability.

Following these rules we have a very small theoretical freedom. Leontief conceived theory as an image build thanks to direct observations. This is expressed by his motor metaphor: studying a complex system is to look under the hood and describe the operations between elementary elements. To do so, two methods are possible.

The first one is to stop the system and make experimentations: "*It would, of course, be even more convenient if it were possible to stop the motor, take it apart, and subject each of its components to any desired tests and measurements. That is what experimental scientists can do and economists cannot.*" (Leontief [1954], p.40)[49]

The second one is to look under the hood of the motor: "*direct factual study and quantitative descriptions of the structural properties of the economic system, detailed in content, comprehensive in coverage, and systematically designed to fill the specific requirement of an appropriate theoretical scheme, seem to offer the only promising approach to empirically significant understanding of the operational characteristics of the modern economy*" (*Ibid*.). This is what Leontief called *direct structural analysis*. It encloses the principle ruling the connection between theoretical terms and primary terms (explicit definition), and the legitimate class of primary terms (direct observation)[50].

[49] This is what Rudolf Carnap called the behaviorist method (Carnap [1997]).
[50] This is what Carnap called, using the same metaphor, the method of structural analysis.

One of the main characteristics of this methodological device is that it is not based on testability of theories. When Leontief referred to testability this is first of all to criticize this method: economics is not an experimental science and we can hardly make powerful tests. In 1937 Leontief underlined the fact that testing a theory is vain as the "*verdict becomes available only after completion of the proposed task*" ([1937a], p.109). Moreover testability allows introduction of indirectly observable concepts: we indirectly infer from tests the empirical significance of a concept. As a consequence we may say that indirect observation (statistical inference) and testability are closely linked. Indeed, in a review article of Richard Stone's book titled *The Role of Measurement in Economics,* Leontief [1952b] asked: "*Can the empirical testing of a theoretical hypothesis really be separated from the estimation of relevant parameters? Can the question of prediction be considered apart from these other two? Should not the "empirical constructs" underlying the modern National Income Statistics be judged in the light of an* <u>*explicitly formulated*</u> *theoretical hypothesis describing the operations of a national economy?*" ([1952b], p.169). It appears that structural analysis, explicit definitions and direct observations were viewed as an alternative to tests methods.

Actually we find no "theory of testability" in Leontief's methodology while he developed a very strong method about the operational meaning of theoretical concepts[51]. The link between concepts and data is direct in what we called the hard core of his philosophy.

This defiance against test methods may be explained by the weak relations tests introduce between theory and data, especially when using probabilities. Instead of connecting directly concepts with observations as it is done with explicit definition, tests and more generally rules of correspondence don't indicate the *necessary* and *sufficient* conditions of the use of a concept. In 1947, for example, Klein noticed about the use of econometrics models that "*on the basis of the limited number of observations available for testing different economic models (…), it is not yet possible to select an* [sic] *unique model. More than one model are consistent with the observations (…). The reader is* ***free to choose*** *among the models, all of which rest on different hypotheses*" (we underline, Klein [1947], p.112-113). Testing is the clear conscience of those who are tempted into making empty theoretical boxes.

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[51] Christian DeBresson gave another interpretation to the term "operational": "In all scientific activities two goals coexist: understanding a phenomenon _often referred to as the *positive* aspect of science_ and mastering the phenomenon _the *normative* or prescriptive aspect. The positive aspects of scientific activity limit themselves to examining the past, after the fact (*ex post factum*); the prescriptive aspects, in mastering a phenomenon, examine facts as they happen (*ex ante*). Only if the latter goal is attained can a theory be *operational*." (DeBresson [1996], p. 3). The question we raise in next part concerns descriptive terms which connect actual facts and potential facts, like disposition terms.

That's why testability doesn't appear as a methodological rule when we analyze Leontief's methodology[52].

The disagreement between Koopmans and Leontief finds his philosophical reasons in the link between concepts and observation: the link Koopmans asked for is much more flexible that the one Leontief tried to found[53]. So, when introducing the assumption of the constancy of technical coefficients, Leontief referred to the quantitative significance of the concept of constancy but he doesn't clearly state the way theses rules justify his assumption[54].

Nevertheless, answering to repeated attack against his constancy assumption Leontief finally noticed that (controversial) tests indicated that the assumption was bearable. The use of test by Leontief appears more as a *concession* and we find in the five rules a much stronger reason to reject the variability hypothesis (Part IV) as Klein and Hurwicz noticed[55].

3. Two comments in the Cowles's first program spirit: Klein and Hurwicz on the constancy of technical coefficients.

As concerns the methodological rules underlying input-output analysis, we find few comments from cowlesmen and especially Koopmans seemed to ignore Leontief's methodology. However, review articles of the *Studies in the Structure of the American Economy* (Leontief & Al [1953]), by Leonid Hurwicz in *The American Economic Review* (Hurwicz [1955]) and by Lawrence R. Klein in *The Journal of Political Economy* (Klein [1953]), both economists close to the empirical-theoretical program of the Cowles Commission, underlined the role played by methodology in the justification of theoretical assumptions in input-output models.

Leontief's empirical science in the eye of Cowles' econometricians

Both underlined the complex interactions of empirical, theoretical and methodological arguments in the work of Leontief and other economists at HERP, and Hurwicz discussed in his report nearly exclusively the methodological justifications of the constancy assumption. If

[52] The five rules (the golden rules) are enounced here p. 17. We may notice that Leontief seems to suppose implicitly that the criterion of operational meaning is a sufficient criterion of truth when forecasts are correct. However these forecasts are too general to permit tests of precise and independent assumptions.

[53] This is the reason for Koopmans' labor division between theoretical economists and statisticians (rejected by Leontief).

[54] In his answer to Koopmans Leontief pled for the use of direct observation, which was connected with his previous methodological arguments.

[55] The question we raise is not to know if tests were confirming or not the assumption, but whether testability was a criterion of operational significance of a theory or not?

the operational meaning of technical coefficient is recognized by Hurwicz, the assumption of the constancy of these coefficients is criticized: "*For it seems that there are two irreconcilable approaches in economics: one where everything is proportional and the coefficients fixed, the other where the absence of proportionality and coefficient fixity is the cornerstone of the theoretical structure. However, while it would be unrealistic to deny divergence of emphasis and specializing assumptions, one takes comfort in the fact that the developments of the last decade have resulted in a theoretical structure broad enough to accommodate both the traditional (curvilinear) and the fixed coefficient (linear homogeneous) models as special cases of a more general model, to be here called the generalized activity model (which is an extension of "activity analysis" encompassing nonlinear, as well as linear process). Among the situations covered by the generalized activity model are: the linear programming model (of which Leontief's input-output model may be considered a special case) and the traditional curvilinear model (as, say, in Allen or Hicks)* " (Hurwicz [1955], p.629-630).

According to Hurwicz, the constancy assumption "*is not a matter when one is merely providing a historical description of a given year*" but where "*predictions are envisaged (…) the assumption that variations in the bill of goods (…) would leave the technical coefficients unchanged must be questioned*" (*Ibid*., p.631). From this Hurwicz noted that it is "*genuinely difficult to determine in what spirit the assumption of coefficient constancy is being made*" (*Ibid*.). Is it a simplification? In this case the constancy assumption may be "*acceptable as an expository device but is obviously dangerous*" for making prediction. Is it an approximation? In this case, the non-substitution theorem showed that "*Leontief's empirical case is stronger than one might at first believe*". However, this justifies the constancy assumption in very special cases only.

To make predictions, we have to examine the constancy assumption "*in the light of the observed phenomena*" (*Ibid*., p.632). After giving some examples (extracted from the *Studies*), Hurwicz found that "*the reviewer, perhaps unduly impressed by the thinking and what Friedman has called the "casual empiricism" of many generations of economists, would expect to find price sensitivity in many important areas of the economic process*." (*Ibid*., p.633). From that Hurwicz deduced that "*it would be surprising if economic analysis could be carried on successfully in ignorance of the principles guiding human behavior*" (*Ibid*., p.634). By this Hurwicz adopted the methodological individualism Koopmans opposed to Vining during the measurement without theory controversy.

When methodology determines results (I)

Reasons of the rejection of this methodological device (to reject individualism implies the rejection of Koopmans' rational manager who makes substitutions between factors of production) are to be found, according to Hurwicz, in Leontief's attitude toward statistical procedures.

Firstly, as Leontief rejects econometrics, he follows a process which is presented "*as the process of fitting a one-parameter curve (a straight line through the origin) to a one-dot scatter diagram*" (Hurwicz [1955], p.634). But, according to Hurwicz, it has not been established that this process is more realistic than a traditional econometric regression with more variables. Secondly, "*where behavioral aspects of the structure are to be studied, experimentation is usually (though not always) ruled out and one is forced to resort to the examination of the history and time patterns of the relevant variables*" (Ibid.). As a consequence, economists have to use time-series analysis and other econometric techniques. Using these techniques we are able moreover to estimate sampling variation and error.

We find in Klein's review a synthesis of these critics: "*the path of empirical research in input-output study is dangerously close to the realm of arbitrary personal judgment. Probability, statistics, and modern method of econometrics cannot be indefinitely neglected or ignored for separating good from bad results*" (Klein [1953], p.262). It seems clear for Hurwicz and Klein that Leontief's assumption is guided by a methodological device which cost is may be too high to be accepted. "*Leontief has strong ideas*, wrote Klein, *about the appropriate course of quantitative research in economics and a low level of tolerance for alternative approaches*" (*Ibid*., p.260). However these critics were focused on the value of direct observation only (second rule).

In what follows we give an interpretation of the problem of the relationships between theoretical concepts and observation and the problem of the value of direct observation, by studying Leontief's production function. This interpretation is clearly influenced by the link we make between Leontief's methodological reflection and a contemporary criticism of modern philosophical empiricism.

IV. The operational meaning of the constancy concept

Following our interpretation of Leontief's methodology, we explore the meaning of Leontief's reference to the "*operational meaning*" of the assumption of the constancy of technical coefficients. Here, one is not dealing with the measure of the intensity of some substitution effects but with the way a concept gain *operational meaning*.

1. Empirical science and deductive science

Maximizing behavior and operational meaning.

Leontief opposed "*empirical science*" to Koopmans' "*deductive science*". This opposition is quite disconcerting since we find a plea for logical chains in Leontief's methodological rules. Input-output models are deductive-mathematical models on which, after the Cowles' linear programming conference, is based a wide range of theoretical researches. Lionel McKenzie, for instance, explored the proprieties of the Leontief technology matrix from the point of view pure general equilibrium theory[56]. Thus, we may say that empirical science is a deductive science and the opposition Leontief made is not primarily between *deduction* and *induction*.

Moreover, for empirical science the question is not to eradicate theoretical concepts but to guaranty the meaning of these concepts. From the point of view of empirical analysis, theoretical terms have first to be directly connected to observations and secondly observations admitted are direct observations. It seems then that the opposition between Koopmans is first of all about the links between theory and data and secondarily the class of legitimate data.

However, the opposition between Cowles Commission econometricians is based firstly on the class of data (direct observations versus statistical inferences). According to Klein and Hurwicz, Leontief's constancy hypothesis is determined by his rejection of indirect inferences, that is to say by the class of legitimate data. As a consequence, behavioral aspects of the economic structure are ignored. However this criticism neglected the role of explicit definitions, explored in what follows.

Explicit definitions and logical empiricism.

Indeed, when introducing the constancy assumption, Leontief wrote that "*theoretical economists deal with production function in their quite general form. More specific characteristics, if introduced at all, take the form of hypothetical assumption <u>rather than</u> systematically observed and measured facts*" (Leontief [1937a]). To fill our economic boxes

[56] The mathematical literature about the Leontief model is too vast to be quoted, but it appeared after the linear programming conference (See McKenzie [2002], [1957]).

we then have to use explicit definitions to find the meaning of theoretical concepts. This latter is given by a synonymous concrete measure.

The logical method underlying Leontief's use of explicit definitions was well known at that time and was one of the main arguments of logical positivism. However, when Leontief was studying the industrial interrelations and founding his methodology on explicit definition and direct observations, the philosophical idea of founding science with explicit definitions[57] was deeply criticized. Indeed Carnap [1936-1937] showed that most of theoretical terms could not find explicit definitions. We explore here how this critical analysis may apply to input-output analysis.

2. Predictions, laws and empirical meaning.

Since one has built an input-output table, the concept of technical coefficient finds an explicit definition[58]. In contrast with Walras, Leontief brought to the concept of technical coefficients an operational meaning. This operational definition was far from being self-evident at the beginning of the thirties[59]. Thanks to a coherent rational accounting system we can define and measure the flows of outputs produced by an industry A, the amount of the product of the industry B used by the industry A to produce its output, and we can define the term "industry"[60]. As we observed and measured actual values these concepts are operational. Now, with the mathematical definition of technical coefficients as a linear function ($x_{ij} = a_{ij}X_j$) and since the explicit definitions of each concepts used in this definition are operational, we obtain the operational concept of technical coefficients[61].

At this stage of our analysis we have mainly four operational concepts but only a point in a multidimensional space (see below, fig.3). If we want to explain or to predict facts, we need a *law*, as a law is a sentence used for making predictions[62]. For instance, to predict how the amount of the product of the industry A will evolve if the final demand of the product of industry B rises we need a law connecting the bill of goods and the units of production.

[57] This is particularly the case of P.W. Bridgman who founded operationalism.
[58] See p.20 for the logical form of an explicit definition.
[59] See n.41 p.18.
[60] The question is not to know if we measure real objects or whether we are making "empirical constructs" but if our theoretical terms are defined with explicit definitions.
[61] Is only considered here a particular way to measure technical coefficients.
[62] A law-like proposition is a *universal statement* asserting a regular connection between two condition orders. We are not asking here if a law is true or not, but how can we use a law-like statement within a methodological device, here Leontief's methodology.

Technically speaking, a law permits to sustain a counterfactual conditional[63]. In the studies of Leontief a law states that the values of technical coefficients are constant (remain unchanged).

From this it follows that the "production function" (because it is the set of relations between inputs and outputs, this is an operational concept; and we don't need the mathematical production function as it is not directly observable[64]) is subject to the law of constant return to scale and that factors are strictly complementary. We now have more than a point in a multidimensional space: we draw the isoquants of our production function (fig.4).

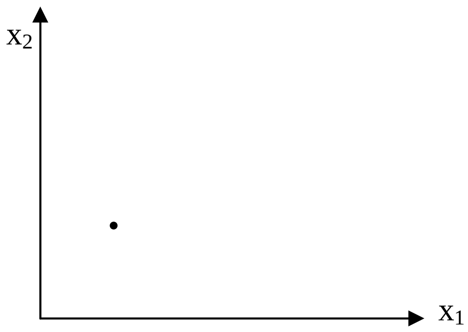

Fig.3

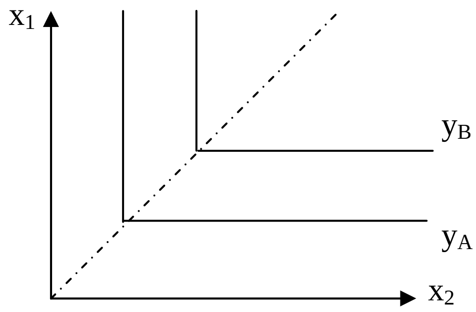

Fig.4

Laws as true sentences.[*]

The problem is that the law is required to be a *true* sentence used for making predictions[65]. Following the methodological device of Leontief's empirical science we need to give an operational meaning to our theoretical term "*constant*". To do so we have to look for an explicit definition of this term[66]. A definition of a predicate is explicit whether the *definiens* contains no theoretical terms. Ultimately, we find a definition explicit when containing only measure operations using terms already known. We find in (Leontief [1937a],

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[63] Leontief's laws of production (constant returns to scale, factors of production are complementary etc) sustain a counterfactual like: "If we had employed more of the production factor x_1 and the same quantity of x_2, the final output would have been unchanged".

[64] See p6.

[*] The concepts employed here (like laws, disposition terms etc) were discussed intently between 1936 and the fifties by Carnap, Quine, Hempel, Goodman etc. See (Jacob [1980]).

[65] The problem doesn't occur in a friedmanian methodology. See (Friedman [1953]). But Leontief, like Koopmans was opposed to Friedman's instrumental philosophy.

[66] If Q= to be constant, x= technical coefficient (i,j), we have to find: $Q(x) \equiv ...$

[1937b]) a plea for such definitions, but it is also at the beginning of the thirties the ideal of most of empiricists (like Carnap or Bridgman). The empirical meaning of theoretical assumptions must be determined by this process.

In Cambridge, in 1936-1937 Carnap demonstrated that some terms, like *dispositions-terms*, couldn't receive any explicit definition. With Carnap's two articles [1936-1937] started a critical period for empiricism. Indeed *verification* was dependent on the possibility of finding an explicit definition for each theoretical term[67]. However Carnap showed that following this rule no synthetic sentence is ever verifiable.

The question is not here to know if Leontief was building or not a theory of verification based on the idea of an empirical meaning, but whether his methodological device faces the difficulties Carnap underlined at the same time[68].

Disposition-terms.

To make predictions Leontief introduced in his theory a disposition-term: "*constant*". Carnap wrote at the same time that about "*the question the so-called disposition-concepts can be defined, i.e. predicates which enunciate the disposition of a point or body for reacting in such and such a way to such and such conditions, e.g. 'visible', 'smellable', 'fragile', 'tearable', 'soluble', 'indissoluble' etc (…) such disposition-terms cannot be defined by means of the terms by which these conditions and reactions are described*" (Carnap [1936], p.440). In other words it is not possible to find explicit definitions for disposition terms (definition based on the description of observable experiences).

Let say that S means "soluble in water"; E(x,t) means "the body x is placed into water at the time t"; and F(x,t) means "the body x dissolves at the time t". "S" is a disposition predicate; "E" and "F" are observational predicates; "*then one might perhaps think that we could define* 'soluble in water' *in the following way*: "x is soluble in water" *is to mean* "whenever x is put into water, x dissolves" (Carnap [1936], p. 440). In symbols:

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[67] "*Taking empirism for granted, we which to discuss, the question what is meaningful. The word 'meaning' will here be taken in its empiricist sense; an expression of language has meaning in this sense if we know how to use it in speaking about empirical facts, either actual or possible ones*" noticed Carnap ([1937]., p.2).

[68] We introduced the concepts of law and counterfactual before the problem of the definition of disposition-concepts. This is not Carnap's order of presentation in (Carnap [1936-1937]). However, these problems are so closely linked that, as noticed Goodman, "*a satisfactory definition of scientific law, a satisfactory theory of confirmation or of disposition terms* (…), *would solve a large part of the problem of counterfactuals. Conversely, a solution to the problem of counterfactuals would give us the answer to critical questions about law, confirmation, and the meaning of potentiality*" (Goodman [1983], p.3). Our thinking is that if disposition terms can't receive explicit definitions, we thus have difficulties to enounce laws…

$$[D:] \qquad S(x) \equiv (t)\ [E(x,t) \supset F(x,t)].$$

This definition means that "x is soluble if and only if, when one places x in water at the time t, it dissolves at the time t." S(x) is defined when the *definiens* is true. However, the logical form of the *definiens* is a conditional. Then when the antecedent of the conditional is false, whatever is the truth value of the consequent, the conditional is true. As a consequence each time an object will *not* be immersed in water, the term "soluble" will be defined! It follows that disposition terms can't receive explicit definitions: 'S' "*cannot be defined by D, nor by any other definition*" (Carnap, *Ibid*.) (See also Jacob [1980], p135-137).

These works by Carnap showed that operationnalism led to dead-end problems. Carnap and many others used therefore for such terms more flexible relations never unique for the same term and which are more *tests* than definitions[69]. We can't find explicit definitions for most of theoretical terms and then a direct connection between concepts and observations: *potentialities* enclosed especially in disposition terms, are not observable. This critic provoked troubles in positivist theories and justified the research of weaker relations between theoretical terms and observations.

Again, theoretical concepts appeared like black-boxes: "*from this black box we derive observational statements and the exact value of the statements of the black box is shelved*" (Jacob [1980], p.148)[70]. As a consequence, to admit terms like dispositions terms we have to modify our rules.

When methodology determines results (II): the role of explicit definitions.

The isoquants of a Leontief production function are "directly" deductible from the measure of technical coefficients. This is not the case for more general production function like those imagined by Koopmans. From a set of constant technical coefficients, organized in a technological matrix, Koopmans needed to introduce a maximizing agent and a criterion of efficiency, to find "*variable*" observed technical coefficients. That is to say that Koopmans' scientific language is more "complex" than the one Leontief used. In other words, if the point in a multidimensional space which represents the value of a technical coefficient is assumed

[69] Explicit definitions permitted to precise the necessary and sufficient conditions of the use of a theoretical term. But there are any explicit definitions for disposition terms. We have to use weaker rules of correspondence between theoretical terms and primary terms. For instance, the logical form of a rule of correspondence will not be $Q(x) \equiv \ldots x \ldots$;

but $[R_1] \quad \ldots Q \ldots$, and we can enounce many rules $[R_i]$ for one theoretical term.

[70] We may note the fact that the problem of disposition terms occurs even with simple experimental process.

to belong to the class of primary terms (language L_o), the "production function" of Leontief is closer to L_o than the one defined by Koopmans[71].

If we choose the disposition-concept "*constant*" we don't need to introduce other theoretical terms to have a production function. If we choose the disposition-concept "*variable*" this is much more complicated. The term "*constant*" is a particular terms as in this case it permits us to keep operational concepts like "technical coefficients", "output of industry A" etc.

This is not the case of the term "*variable*" as no operational terms are compatible with. We find here a reason of the rejection of "*variable*". Leontief noticed about the maximizing behavior that "*the economist must be prepared to make up his mind whether he is aiming at a positive explanation of observed facts or at setting up normative rules for, in some sense, "reasonable" behavior and tracing out their logical implications*" (Leontief [1954], p. 27).

There are any operational analyses according to Leontief of individual behavior. As a consequence, to the question Koopmans raised to know whether the assumption of a rational maximizing agent is normative or descriptive, Leontief answered in contrast with Koopmans that this assumption was normative and not operational. However, to make laws we have to abandon the first rule (explicit definitions) for some terms. We thus have to reformulate input-output methodology to allow the use of disposition-terms[72].

3. *More rules: from meaning to testability*

Explicit theorizing or direct observation?

If we only admit explicit theorization, we fall on the disposition-terms problem and have to use testability[73] which permits us to use a very large range of theoretical propositions: data constraint less and less theory. When Leontief noted that "*the freedom of theoretical*

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[71] Carnap noted that "*it is clear that to accept the requirement of complete confirmability or that of complete testability means to exclude generalized sentences and hence to state L_o. The step of dropping that requirement and choosing one of the wider languages instead of L_o is a decisive one. One of the chief reasons in favour of this decision is the fact, that both methods of interpreting physical laws in the case of L_o which we mentioned above are not very convenient for practical use and, above all, are not in close conformity with the actual method adopted by physicists.*" ([1937], p. 26).

[72] One may consider here two possibilities: the first supposes that Leontief's methodology doesn't suffer from the problem of disposition terms and then that our five golden rules are a misleading interpretation. The second is considers that the disposition terms problem is pertinent to Leontief's philosophy and that we have to find a formulation of this methodology which challenges the problem of disposition terms. This is this last possibility we test.

[73] Carnap imagined other possibilities but as economics is not an experimental science, according to Leontief, we ignore the other rules of correspondence between theoretical terms and observations. See about Leontief and probabilities (DeBresson [2004a], [2004b]).

choice [is limited considerably] because the numerical values of all parameters must be ascertainable on the basis of available statistical information" (Leontief [1937a], p.111) if it was because of his conception of what a definition is, it follows from the problem found by Carnap that this freedom is in fact considerable and, from that, interpretations of theoretical terms are possible.

Facing such a difficulty we can imagine almost two solutions to balance our initial methodological interpretation of the constancy assumption:

[A]: The five golden rules remain (operational criterion), but we have also to add two auxiliary rules:

(A1) we admit weaker relations between theoretical terms and data, like tests (indirect relations)

(A2) One chooses among the possible theoretical systems the one which minimizes the number of un-operational terms.

Or

[B] We reject the golden rules to admit weak relations between theory and data (testability) and choose the most "promising" theory.

Leontief's methodology may be [A]. The "tolerance" of this device depends on the way we interpret the two auxiliary rules. If we apply without constraint the second auxiliary rule, we fall again in the disposition-terms problem. If we admit some un-operational terms, then the first auxiliary rules may be too much tolerant to eliminate "undesirable" concepts. In this last case, this methodology is much more tolerant and permits much more theoretical freedom than Leontief supposed initially. For instance with [A] we may accept mathematical production functions and even utility functions like those proposed by Samuelson's operationism[74]. Indeed, with [A], theoretical terms need no more to be directly observable even if only direct observations are tolerated. Moreover, since we admit indirect relations between theoretical terms and observations it seems difficult to reject probabilities as a tool to measure the value of our assumptions. Thus we may think that this is because of such difficulties that we find at the HERP more and more tests methods (and un-operational concepts), whenever in 1952 Leontief's tests methods were vague.

[74] Indeed, as we no more need to have directly observable concepts, we may admit mathematical functions which allow predictions of directly observable facts. For instance, if the utility function of the individual A is U(x), I'm supposed to observe the consumer choice x_i (which is a direct observation). But U(x) is not directly observable.

It follows that we find inside input-output analysis a tension between a weak methodology [A] and a hard methodology (the five rules). This tension is the one between the actual world and the possible worlds. According to the link we find between these worlds, our philosophy of science completely changes (and conversely).

The actual world and possible worlds[75].

To make predictions we need laws. To enounce laws we often need theoretical terms which cannot receive explicit definitions. But what any explicit definition will never express is that the proprieties attributed to objects are subsisting even when not measured. What is not caught by these definitions is that these proprieties are not only *actual* but *potential* (See Jacob [1980], p.135)[76]. Between the point in [fig.3] and the isoquants in [fig.4] we design a crossing from the actual world through possible worlds. Such predicates like disposition terms do not deal only with actual world but also with possible worlds, potentialities.

To cross this frontier we need laws and, since explicit theorization is vain, to take risks and make theoretical speculations. Shall we maximize this risk or minimize it? That is the choice between Koopmans' production function and Leontief's one. Between them is the belief in testability. According to empiricism testability is too vague to avoid empty theoretical concepts. We have to find explicit definitions and use only direct observations and when making indirect inferences, we make assumptions about facts which are most often false. But many theoretical terms have no explicit definition. We thus have to use testability…

This position is just in the front line between testability and meaning where Carnap was at the same time, between 1936 and 1937. At that time Carnap thought that this tension between testability and meaning could find a solution which would get Popper's methodology and logical empiricism closer. But from that time the former and the latter are subject to radical critics as empirical meaning is not far from being an illusion and testability far from being a sufficient criterion to indicate the value of hypothesizes and avoid addition of auxiliary assumptions to protect theory.

[75] See Goodman [1983].
[76] We have to use a conditional statement. See also (Hempel [1954]).

Conclusion

The general methodological device Leontief build during the 1930-1954 period, a time when instrumental methodology was not an answer to the troubles linked with the research of empirical foundation to economics, had to lead economics to a radical "*redefinition of (…) general strategic objectives*" [1952a]. These were not only the methods that had to be redefined, not only the ways to pass through the emptiness of economic concepts, but the aim of economics, the point to reach. That's why economics had to be founded as an empirical science.

This is the radical dimension of this research project that we try to grasp. Following this line the empirical science is not an improvement of the first program that Schultz, Moore, Frisch, and Leontief were following during the twenties and the beginning of the thirties.

If the strategic objectives are to be removed, the scientist's task is not to apply theoretical concepts defined in the world of pure ideas. This is the message Leontief addressed to Koopmans' labor division between the scientists and the statisticians. From this point of view Leontief's use of walrasian technical coefficients is merely a coincidence to which it is not to give a misleading interpretation. Leontief's idea of application is neither the one Walras defined, nor Moore or Koopmans. We find in Leontief's methodology a hard line close to logical empiricism: here "*empirical* implementation is considered as much a part of an economic argument as the consistent development of its *logical* consequences" (we stress, Leontief [1953a], p.4).

To answer Koopmans' and Cowles' econometricians' attacks against the constancy assumption, Leontief brought the debate on a methodological ground. The question of the substitutability of technical coefficients has not to be considered from the point of view of theory but from the point of view of empirical analysis.

As Leontief didn't give a systematic exposition of what an empirical science is, we propose an interpretation of his methodological device. A hard core is found, enclosing five rules: *first*, concepts have to be defined explicitly using familiar terms and primary terms exclusively; *second*, the primary terms are direct observations; *third*, we have to use effective observations and not virtual observations; *fourth*, forecasts about directly observable phenomenon bring direct confirmation; *fifth*, theory must be logical and coherent. These rules determine the operational meaning of a theory.

However these five rules are too strong to permit formulation of useful economic laws to make prediction (this was the problem of disposition terms). We thus propose two auxiliary rules: (A1) we admit weaker relations between theoretical terms and data, like tests (indirect relations)

(A2) One chooses among the possible theoretical systems the one which minimizes the number of un-operational terms.

The resulting methodological device suffers from a tension between the five strong rules defining operational meaning, and, depending on the tolerance of the second auxiliary rule, the criterion of testability (A1). This tension may appear more intently with the dynamic input-output framework.

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