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978-0-521-83238-0 - Wassily Leontief and Input-Output Economics

Erik Dietzenbacher and Michael L. Lahr

Excerpt

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Part I

Reflections on input-output economics

1 A portrait of the master as a young man

Paul A. Samuelson

1. The Harvard background

Leontief had a long and picturesque life in three countries, on two continents. Over sixty years his was a first-rate lectureship at Harvard University and New York University (NYU).¹ At the editors' invitation, I speak here for an early generation of Leontief's boys: those in his special workshop within a golden pre-war Cambridge age. Listed in approximate chronological order, I bear witness for Abram Bergson, Sidney Alexander, Shigeto Tsuru, Lloyd Metzler, Dick Goodwin, Jim Duesenberry, Hollis Chenery, Bob Solow and myself. A baker's half dozen that, owing only to age-related inadvertence, omits to mention a few other celebrated names.²

For a long time I was as much younger than Leontief as Solow is younger than I am. However, late in the era of the Soviet Union, revisionist research into Czarist vital statistics pushed back from 1906 to 1905 the birth year of my beloved master. But what signifies age? When I first glimpsed Wassily, brown-suited, dark, scarred and handsome, at the 1934 Palmer House Chicago meeting of the American Economic Association (AEA), he looked much the same as when at 69 he left Harvard in a huff for NYU. Even in the months before he died, in 1999, his appearance had not changed much. I may also add that his foreign accent softened little over the years; but, after my first hour of hearing him lecture, his soft-spoken words came through loud and clear.

We graduate students spun legends in the junior common room about our mentor. At the age of puberty, as a Menshevik, his life was spared by the Bolsheviks in the hope that he would grow up to know better. The scar on his neck was not the wound from a student's duel; actually the

¹ It was a nineteenth-century Harvard graduate who said: "Good Americans, when they die, go to Paris." It is I who says: "Good economists, before they die, go to NYU." Fritz Machlup, Oskar Morgenstern, Will Baumol and Wassily Wassilyovitch Leontief will know I state the truth.

² Marion Crawford (Samuelson) was at least one gender exception; her 1937 summa senior honors thesis was written as Leontief's Radcliffe tutee.

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[More information](#)4 *Paul A. Samuelson*

German operation that produced it did provide him the exit visa to leave the Soviet Union.³ Like an earlier immigrant, Simon Kuznets, the young Leontief at first seemed quite apolitical in America. Later he reversed the usual life cycle: with age, conservative cynicism peeled off – particularly after the Republicans cut back on input-output development.

In 1935 Harvard was just moving from torpor into an Elizabethan renaissance. Frank Taussig had aged. Allyn Young had died prior to returning from the London School of Economics to Harvard. Failing to achieve tenure, Laughlin Currie had recently been banished to Washington. Charles Jesse Bullock and Thomas Nixon Carver had at long last retired. Economic historian Edwin Gay, although he may not have known it, was in his last year at Harvard (thereby liberating Abbott Payson Usher to teach graduate students). John Williams led dynamic seminars that were respectable and, after Alvin Hansen arrived (in 1937, by a Harvard miscalculation!), the two made a great macroeconomic duo. Edward Chamberlin at 35 was, judged retrospectively, at the zenith of his scholarly career; Edward Mason was not yet the important elder statesman he was to become. Other local worthies can mostly be overlooked.

Thanks only in part to Adolf Hitler, the foreign rescuers were on their way: Schumpeter from Austria and Weimar Germany; Haberler from Vienna and the League of Nations. It must have been the newly-arrived-in-Cambridge Schumpeter who plucked Leontief from a brief National Bureau stint to Harvard. I suspect Schumpeter fastened on Leontief as a genius on the basis of the 24-year-old's German article (Leontief, 1929) on how to identify demand and supply elasticities from a time-series sample – a brilliant investment decision even if not 100 percent cogent.

2. Early teaching

It was only in the calendar year 1935 that Schumpeter and Leontief were permitted to lecture on their specialties. That was luck for me since it provided both a telescopic and a microscopic add-on to my training. It rescued me from my miscalculation, which had diverted me from Morningside Heights to the Harvard Yard.⁴

³ Unlike Prokofiev he never went back, except to preach to his fatherland the virtues of input-output analysis.

⁴ When the Social Science Research Council (SSRC), my Medicis, dictated that I leave Chicago, midway locals without exception advised choosing the Columbia of Wesley Mitchell, Harold Hotelling and J. M. Clark. Joseph Schumpeter, I was told, was the eccentric who believed in a zero interest rate for the stationary state. Leontief neither I nor they knew anything about. Before Seymour Harris was an “inflationist,” Lloyd Mints warned me against him as one. Independently of any Chicago reading list, I had

That first registration day I gladly burned my bridges. Defying indescribable high authority, William Tell refused to take economic history from Gay. (I already knew it from John U. Nef.) That made room to take two advanced courses – one of which was from Chamberlin. Twenty-one years later, when I substituted for him to teach the basic elementary Harvard graduate course in theory, I encountered precisely the same unchanged reading list: J. S. Mill, A. Marshall, E. H. Chamberlin and J. V. Robinson! Eschewing Gay in the spring semester, I was able to learn genuine modern statistics from E. B. Wilson, bypassing Edwin Frickey (who, with Leonard Crum, taught at Harvard courses *against* modern statistics!). But all was not lost.

For the first time Wassily gave a one-semester mathematical economics seminar; it was camouflaged as “Price Analysis” but that didn’t fool me. We were a small class. Abe Bergson, then a third-year graduate student, was one attendee. Another was Harvard honors senior Sidney Alexander. Maybe Shigeto Tsuru and Philip Bradley were auditors, as was Schumpeter occasionally.

Here is what we learned from late September to almost November Thanksgiving. (a) Specified two-good indifference contours, non-intersecting and “convex to the origin.” (b) A negatively sloped budget line. (c) No indicator of *cardinal* utility at all. The commodity (numbered 2) on the vertical axis was specified to be numeraire good, so that P_1/P_2 determined the absolute slope of the budget line. (d) As this price ratio changed, the budget line pivoted around the intercept where it hit the vertical axis. (e) What could we *prove* about the signs of $\partial q_1/\partial(P_1/P_2)$ and $\partial q_2/\partial(P_1/P_2)$? But first, (f), what might be true of the signs of income elasticities or of $\partial q_i/\partial(I/P_1)$ when I/P_1 is defined as $(P_2/P_1)q_1 + q_2 = I/P_1$, the budget constraint?

We learned that, in so-called “normal” case(s), *both* income elasticities would be positive. But also there could be cases where one, but *not both*, of the income elasticities could be negative. Finally, somewhere between Columbus Day and Thanksgiving, we found the Holy Grail at the North Pole.

Theorem. In all “normal” cases, own-price elasticities were indeed negative. However, in a case where a good’s income elasticity was negative and much was spent on it, Giffenosity could obtain to make $\partial q_i/\partial(P_i/P_j) > 0!$

discovered on my own the *Theory of Monopolistic Competition* (Chamberlin, 1933) on the SSRC reserve room shelf. That predisposed me toward Harvard. But, truth to tell, it was because I expected Harvard to be like Dartmouth – located around a New England green common, with a white chapel tower and much ivy on the walls – that I arrived by tram, unannounced, at the Harvard Yard.

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Excerpt

[More information](#)6 *Paul A. Samuelson*

We didn't learn this by writing down in our notebooks the professor's dictated statements of the theorem. We *proved* it by 2×2 determinants! Ah, bliss.

No other course I ever took so profoundly set me on the way of my life career. It was, so to speak, slow motion, and all the better for that. It prepared me to master Edwin Bidwell Wilson's exposition of Willard Gibbs' thermodynamic analysis. Leontief assigned no readings in Pareto or Allen-Hicks; or, for that matter, W. E. Johnson (1913) or Eugen Slutsky (1915) – only our own laboratory work. Then, after Thanksgiving, we replaced the linear budget equation by Haberler's (1933) concave “opportunity-cost” curve – thereby mastering Leontief's (1933) own vindication of Marshallian (1879) offer curves in international trade. Obviously we were prepared for James Meade's (1952) later graphics of international trade.

I have told more than once how Haberler's resistance to indifference curves provoked from one brash Leontief student the rebuke: “Well, without indifference curves, your 1925 Vienna Ph.D. thesis on index numbers evaporates into thin air” (see Haberler, 1927). The theory of revealed preference (see Samuelson, 1938a, and 1938b) was born one second later as I listened to what *I* was saying.

Although Wassily rarely lectured on *his* current researches, this was a golden decade in his own life. (Also, it was that for Abba Lerner far away in London. And for the Oskar Lange whose muse left him after his patriotic return to post-war Soviet-satellite Poland.) Notable and already mentioned was Leontief's (1933) paper on indifference curves in international trade. Less noticed was his (1934) paper – in German, but translated in Leontief (1966) – on cobweb dynamics of non-linear supply and demand curves. Here his topological explorations into multiple periodic motions came close to chancing on modern *chaos* theory. Already his Harvard lectures introduced testable partial differential equations for disaggregation separability. In my 1941 thesis (see Samuelson, 1947, p. 178), I referred to the *Leontief* condition for additive-utility independence of goods x and y , namely the vanishing of $\partial^2 \log M(x, y) / \partial x \partial y$, where M denotes the observable marginal rate of substitution between x and y .

3. **Afterthoughts on input-output**

Leontief's middle and final decades were increasingly preoccupied by input-output researches (see Leontief et al., 1953; Leontief, 1966). These were of tremendous value to society and to him. His Nobel Prize properly cited them. Well and good; a scholar should follow his own instincts and volitions. Still, I have to confess to a certain regret. Max

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Excerpt

[More information](#)

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7

Born (the physics Nobel laureate who helped to found the better post-Planck and post-Bohr quantum mechanics theory) expressed my sentiments when he wrote to the Albert Einstein who, from the age of 45 on, concentrated all his energies on creating a new unified field theory combining gravity, relativity, quantum theory and cosmology. To do this, Einstein chose to cut himself off from most of the frontier developments in 1925–1955 physics. Born wrote to his admired master: “We are left to struggle on without our leader.” I am much like Oliver Twist, who always asks for “More!” So original and lively an economist as Leontief, in my contra-factual history, could well have given us another volume of diverse and sparkling collected papers like those in his classic 1966 book. The whole world appreciated the genius of Wassily W. Leontief. But we his disciples knew the full measure of his inspiration and potential.

At Berlin Leontief was lucky in his teacher Ladislaus von Bortkiewicz, a keen contributor to statistics and to mathematical economics. Matching this depth came the width from Werner Sombart, the grandiose creator of theories for economic history. From von Bortkiewicz’s improvements on Marx must have come an early interest in the Quesnay-like circular interdependence of input-output. But, from my later explicit quizzing of him, I can rebut the innuendo that he ever did know the work of Vladimir Dmitriev (1898). Just as Sraffa’s (1960) book on input-output never cited Leontief, Leontief’s 1925–1999 writings seem never to have cited the work of Sraffa.

I try not to make those venial mistakes. I am conscious of how much I have benefited from teachers like Leontief: at Chicago Jacob Viner, Henry Simons, Frank Knight and Paul Douglas; at Harvard Edwin Bidwell Wilson, Joseph Schumpeter, Leontief, Gottfried Haberler and Alvin Hansen. It is humbling when one weighs accomplishments against advantages. Old school ties are dummy variables that unfairly boost one’s R^2 . And, when your teachers pass off the stage, your students step in to add on their push. All the while the wind is broken for us by contemporaries such as Abram Bergson, Robert Solow, Kenneth Arrow, Gerard Debreu, Abraham Wald, Lionel McKenzie and the rest of the Invisible College.

Sixty-five years have not dimmed memories of that golden age in the Harvard Yard: so to speak, Wassily Leontief on one end of the log and I on the other.

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Cambridge University Press

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Erik Dietzenbacher and Michael L. Lahr

Excerpt

[More information](#)

8 Paul A. Samuelson

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2 Leontief's "magnificent machine" and other contributions to applied economics

Karen R. Polenske

1. Introduction

Wassily W. Leontief was an excellent theorist. As I note below, others have reviewed his extensive contributions to economic theory. In this paper, I focus instead on the many major contributions to applied economics he made by conducting detailed empirical analyses. He was often ahead of his time, especially by sensing that computers would soon be able to handle the difficult and complex empirical studies he kept saying needed to be done. I begin with a brief review of his most important empirical contribution, namely the development and extensions of the input-output model. Then I discuss five areas of applied economics in which he made an innovative set of insights: (1) automation; (2) disarmament; (3) the environment; (4) foreign and interregional trade; and (5) spatial and world analyses. In each area of study, he usually constructed a novel framework in which to conduct a unique analysis.

2. Linking theory and applications

One of Leontief's major contributions to economics, of course, was to economic theory. Others (e.g. Dorfman, 1973; Carter and Petri, 1989) have discussed most aspects of his theoretical work, so I will not focus on them here. In order to understand his contribution to applied economics, I do review his important thinking on linking theory and applications. He mentioned this link in many of his articles, feeling it was critical for superb economic analyses. As recently as 1998, Leontief stated: "My tendency was to combine empirical and theoretical. In economics that combination requires mathematical concepts, such as systems analysis" (Foley, 1998, p. 126).

Leontief was extremely critical of most economic theorists, especially of those who failed to understand economics as an empirical and applied science. As an example, he sharply criticized the neo-Cambridge group of economists who supported Keynes (Leontief, 1937), arguing that

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Excerpt

[More information](#)10 *Karen R. Polenske*

analysts needed to beware of defining theoretical arguments in such a way as to ensure that given conclusions would be reached. This is most likely to occur when theory is not tested by empirical observations. In an early article on the interrelationship of subsets of variables (Leontief, 1947), as well as in his 1970 presidential address to the American Economic Association (AEA) (Leontief, 1971), he wrote about the need to confirm theory with the use of detailed data. In a 1958 article, he went even further in arguing that supporting theory with data should be done by the same person. He sharply criticized Koopmans, who wanted to help correct problems with econometric work by separating the pure theorists from the empirical investigators, with the first building internally consistent models, and the second testing them with observed facts. Leontief (1958, p. 104) said that such a separation, “instead of alleviating the trouble, is bound to make it very much worse.” He referred to a quote from Quesnay, who said that “theory and observation, which are reconciled perfectly, if combined in a single person, whenever they are separated wage against each other an incessant, but futile, war” (p. 106, fn. 1). Leontief was as ruthless in his criticism of most empirical economic analysts as he was of the economic theorists, saying that an empirical analysis should be a “descriptive complement of its theoretical analysis” (Leontief, 1954, p. 229). He expressed dismay that some economic theorists believe they are doing empirical studies, but try to “depict the operation of the entire economic system in terms of five, four, or even only three aggregative variables” (p. 229).

During the late 1930s and throughout the 1940s and early 1950s, when others at Harvard University and elsewhere were not using mathematics, Leontief encouraged his students to learn it (see Solow, 1998). By the late 1960s, however, Leontief felt that most economists were spending far too much time in developing more sophisticated mathematical models, rather than in working on data issues that would help them assess the way the real world worked. He stated his concern in the following blunt words (Leontief, 1967a, p. 2): “I submit that already the greatly perfected engine of economic theory is using up the available factual information much faster than the limited resources devoted to collection and organization of such basic data can now supply.” Leontief also said, “I am essentially a theorist. But I felt very strongly that theory is just a construction of frameworks to understand how real systems work. It is an organizing principle, while, for many economists, theory is a separate object” (Foley, 1998, p. 123). He was also adamant that techniques of statistical inference “yield only marginal improvements in terms of solid, factual results” (Leontief, 1967a, p. 5). As an alternative, he claimed that data had to be collected first-hand, tedious though it may be to do so, and organized into appropriate frameworks. The input-output accounts and the models

developed from those accounts are part of his solution to this perception of what was missing in economics.

3. The input-output model – the “magnificent machine”

What is the magnificent input-output machine that Leontief designed? How did he come to develop it? Leontief indicated that his development of the input-output model of an economy was influenced by Quesnay and Walras, not by Marx, and that he conceived of the input-output structure in 1927 at the Institute for World Economics in Kiel, Germany, after leaving Russia in 1925 (Foley, 1998, p. 118).¹ He had studied mathematics and was well aware of Frobenius matrices, which economic analysts today know of as Leontief matrices. Underlying his interest in the mathematical/theoretical construct of the model was an even deeper desire to see how an economy actually operated.² This desire seems to have been the prime motivation for his theoretical and empirical input-output work (Foley, 1998). He wanted to open the “black boxes of economic theory”; or, to use one of Leontief’s many analogies, he wanted to look under the hood of the machine (the economy), in fact, to take the motor apart and “subject each of its components to many desired tests and measurements,” whereas most theorists were interested only in building the machine (Leontief, 1954, p. 228).

His first paper on input-output analysis appeared in 1936, entitled “Quantitative input and output relations in the economic system of the United States.” Although this was a “novel and important contribution to economic theory,” Leontief laid stress in the paper on “the numerical description of the American economic structure” (Dorfman, 1973, p. 434). Based upon that work, the US government asked him to come to Washington, DC, to construct a 1939 input-output table. He refused

¹ It was in Kiel that he met a group of Chinese, who had the Chinese ambassador in Berlin ask him to go to Nanking, China, as an economic adviser to the Ministry of Railroads. In his usual innovative manner, while in Nanking, he had photographs taken from an aircraft in order “to make estimates of farm production by region as a basis for planning rail lines” (Silk, 1976, p. 157).

² There is some dispute as to whether or not Leontief developed several of his input-output theory and accounting ideas while still in the Soviet Union (Kurz and Salvadori, 2000). He claims he did not (Foley, 1998), but he did publish a critique of the work of Popov, a Soviet statistician, on the balance of the economy of the USSR. Popov developed detailed accounting systems somewhat like the input-output tables (Leontief, 1964). Leontief learned later that Remak “proposed a theoretical input-output formulation of an economy seven years before Leontief’s earliest paper on the subject, [and] a mathematician, H. E. Bray, had written in a similar vein seven years before that” (Dorfman, 1973, p. 431). Regardless, none of his predecessors structured the accounts in as explicit and comprehensive a way as Leontief, and none tried to collect such detailed data as he did for the US economy.