

# MEMORANDUM

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## Wassily Leontief and the discovery of the input-output approach

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# Wassily Leontief and the discovery of the input-output approach

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By

Olav Bjerkholt

## Abstract

The paper is about Wassily Leontief's path towards the discovery of input-output economics, as reflected in his 1936 and 1937 articles in the *Review of Economic Statistics* and in the 1941 monograph *The Structure of American Economy, 1919-1929*. The paper sets out an account of Leontief's life from his childhood and early youth in St. Petersburg. Then followed study years in St. Petersburg and Berlin, work as research associate at the Institute of World Economics in Kiel, an adventurous trip to China, and a further year in Germany during which circumstances and luck brought him to the United States in 1931 and to Harvard shortly afterwards. The aim has more specifically been to consider and how Leontief's discovery of an input-output approach was influenced by his earlier studies and research experience and by exchanges with Joseph Schumpeter, Ragnar Frisch, his Kieler colleagues and others.

**Keywords:** Leontief, input-output

**JEL:** D57, B31

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### Introduction

“Whoever thinks of Wassily Leontief thinks of input-output, and vice versa” (Dorfman 1973, p.431). The statement sounds like a truism. Dorfman added, however, that Leontief cannot be summed up in a single accomplishment but a dominant theme running through Leontief’s professional work, providing an important clue for the discovery of input-output economics, was in Dorfman’s view that economics is an empirical and applied science. Fancy theoretical apparatus may sometimes seduce students into intriguing and sterile bypaths but the only valid test of economic research is its empirical and its practical significance.

The paper is about Wassily Leontief’s path towards the discovery of input-output economics, which was marked by two articles in *Review of Economic Statistics* in 1936 and 1937, followed in 1941 by the monograph *The Structure of American Economy, 1919-1929*. The organization of the paper is largely biographical covering Leontief’s life from his childhood and early youth in St. Petersburg, followed by a relatively documentary account of his study years in St. Petersburg and Berlin and his work as research associate at the Institute of World Economics in Kiel. His adventurous trip to China in 1929 is mentioned while his last year and a half in Germany in 1930-31 is set out in some detail including his exchanges with Joseph Schumpeter, Mordecai Ezekiel and others who advised or

influenced him. It was circumstantial luck which brought him to the United States in 1931 and to Harvard shortly afterwards.

An attempt is made in the paper to look for clues of how Leontief arrived at his discovery. The question could be put whether there on Leontief's career path was an "input-output moment" at which the cumulated insight was brought to fruition and the input-output approach was discovered. It can be foreshadowed that such a *eureka* point could not be clearly identified. Perhaps this attempt was a too simplistic way of looking at the discovery process.

Others have touched upon the question of when and where Leontief arrived at his input-output approach. The account of Ann Carter, a close associate of Leontief over many years from the late 1940s is the following:

"When Wassily Leontief first came to the United States, he spent a year or so in New York working at the National Bureau for Economic Research. He was developing a new system, later to be called input-output analysis. One day he received an invitation to join the Harvard faculty. He replied with qualified interest: he would be pleased to come, but he required a grant of \$1500 to cover the cost of a research assistant to help him with the implementation of his fundamental new study. They reviewed his project with considerable skepticism. What he proposed to do was probably impossible and certainly very strange. Still, they seemed to want him. The \$1500 was granted on condition that he agreed 'to report his failure in writing' at the end of the year." (Carter 1976, p.57).

Another long-time Leontief associate, Karen Polenske, placed the "input-output moment" further back in time: "Leontief indicated that his development of the input-output model of an economy was influenced by Quesnay and Walras, not Marx, and that he conceived of the input-output structure in 1927 at the Institute for World Economics in Kiel" (Polenske 2004, p.11). Others have pointed to Leontief's doctoral dissertation *Die Wirtschaft als Kreislauf* as the origin of his input-output approach.

In the autobiographical note provided for his Nobel award in 1973 Leontief gave a brief account of the scientific logic in the path he followed:

"Having come to the conclusion that so-called partial analysis cannot provide a sufficiently broad basis for fundamental understanding of the structure and operation of economic systems, I set out in 1931 to formulate a general equilibrium theory capable of empirical implementation." (Leontief 2015).

The paper thus aims at following Leontief's tracks from his early life in St. Petersburg via Berlin, Kiel, New York to Harvard and in that biographical context look at how it

happened that he arrived at his input-output formulation with no pretense of offering a comprehensive biography. Leontief spoke often and entertainingly about his early. In later years he gave interviews with fascinating details about his experiences.

While always prepared to speak on his scholarly work and contribute his view on a wide range of current issues Leontief was reticent about being placed on a pedestal as an important personality in the field of economics, rather than remaining the active and curious scholar he always had been. Invited in 1988 to contribute a chapter on his “life philosophy” to an anthology with contributions from highly prominent economists Leontief declined without hesitation: “Unlike some of my professional colleagues, I would find it practically impossible to describe ‘My Life Philosophy’ on twenty, a hundred or even five hundred pages. I am not as self-conscious as they seem to be.”<sup>1</sup> Later in the same year Leontief was asked by Samuelson whether he was willing to write an autobiographical piece. Leontief waved it away: “I do not feel self-important enough and I have more interesting things to do than to write about myself.”<sup>2</sup>

As Leontief was an eminently interesting personality whose reflections and biographical notes would have been of great interest, his reticence was unfortunate. It is surely true that Leontief always, even at very high age, focused on interesting tasks ahead and seldom, if ever, looked back and reflected self-indulgently on what he had done in earlier periods of his life. Hence, on this background it is perhaps not surprising that Leontief wrote next to nothing on the origin of input-output economics as it happened in his own life experience. Several of his key co-workers and many students of input-output economics have taken a natural interest in this issue. Some asked him and got answers but not sufficient to trace his steps. This has provided motivation to look a bit closer into the issue.<sup>3</sup>

But to Leontief in his characteristic forward-looking view the importance of the “input-output moment” was the opening up of large fields that needed tilling and cultivating, tasks that would take years or even decades to complete.

The origin of input-output analysis is, however, in the literature discussed also in a different meaning, namely as referring to scholarly work by pioneers of linear models with

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<sup>1</sup> Leontief to Michael Szenberg, 31 March 1988. The volume was published as Szenberg (1993) with contributions by Paul A. Samuelson, Kenneth J. Arrow, Robert M. Solow, L. R. Klein and another eighteen eminent economists.

<sup>2</sup> Leontief to Samuelson, 16 Sept. 1988. Instead Leontief offered his curriculum vitae and a long interview in French, Rosier (1986).

<sup>3</sup> The author found recently in his files that he had asked Leontief in the mid-1990s about his early work, and received the following response: “I am sorry to be unable to provide you with additional materials throwing light on my early works. To secure some money to pay the salary of a secretary, I had to send to Japan not only a large part of my library but also archival materials which up to now was being held by Ann Carter.” (Leontief to the author, 20 November 1995.)

more or less similarity to an input-output model in formal structure. The work of these pioneers, some of which overlapped in time with the early part of Leontief's career, is highly interesting but not discussed further here because not much evidence has been found to underpin essential influence on Leontief's own path.<sup>4</sup>

As the account given of his formative years and early career before he arrived in the USA in 1931 indicates, Leontief was from early on equipped with broad intellectual interests and great receptive capacity. He had received a very good education in St. Petersburg despite the disturbances caused by the political turmoil in 1917 and the ensuing years. Under slightly different circumstances he could have made other choices. It wasn't for very pressing reasons that he left leave Russia for Germany in 1925, or, for that matter, Germany for USA in 1931. Other choices would have led him onto different paths but surely paths that would have served to release the great talent Wassily Leontief had for analyzing social and economic phenomena combining theory, mathematical tools, and his firm conviction that scholars needed to have direct and firm knowledge of the data sources relevant as underpinning of their theories.

Leontief's interest in empirical applications and his hands on approach to data handling is not at variance with Robert Solow's characterization of him as "first and foremost a theorist" (Solow 1998, p.299). A characteristic feature of Leontief as a scholar was his insistence on the empirical relevance of economic research. His general *modus operandi* as a researcher which can be discerned already in the late 1920s distinguished him from many other scholars. He insisted on the pre-eminence of theory: empirical investigations had to be guided by theory, economic data should be compiled and analyzed within a theoretical framework. But Leontief also advised that the researcher should acquire a very thorough knowledge of the empirical realities he was dealing with, by literally wading through the masses of data.

Leontief was one of the charter members of the Econometric Society founded 1930. The main objective of the Society was "...to promote studies that aim at a unification of the theoretical-quantitative and the empirical-quantitative approach to economic problems and that are penetrated by constructive and rigorous thinking similar to that which has come to dominate in the natural sciences."<sup>5</sup> The formulation appealed to Leontief since he first read it in 1930 as probably found it as a poignant expression of his own view.

Leontief's view on the role of theory in the use of economic data was stated in a hidden away passage in one of his key papers:

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<sup>4</sup> See e.g. Kurz (1995, 2006), Parys (2013), Bidard and Erreygers (2010).

<sup>5</sup> Constitution, *Econometrica* 1, Jan. 1933, p.106.

“The most elaborate statistical investigation furnishes nothing but shapeless heaps of raw material, utterly useless unless fitted into a firm theoretical framework” (Leontief 1937, p.131).

The passage mirrored a statement by Ragnar Frisch a few years earlier: “The material under observation is and remains a dead mass until it is animated by constructive theoretical speculation.”<sup>6</sup> Similar views were shared among several of the early econometricians.

On various occasions Leontief spoke out on such matters of philosophy of science and voiced his opposition to prevailing views and practices. He was e.g. utterly opposed to a strict division of labor between on the one hand theoreticians constructing models and on the other hand researchers who estimated and applied the models, as had been proposed by Tjalling Koopmans in his famous essays on the state of economic science.<sup>7</sup> His sharpest and most explicit critique of prevailing tendencies in the development of economics was stated in the 1970 Presidential Address to the American Economic Association titled *Theoretical assumptions and nonobserved facts* (Leontief 1971).

## The St. Petersburg heritage

The history of the Leontief family in St. Petersburg began when an enterprising ancestor, a merchant from Kiev in 1741 was granted citizenship and permission to open a trading business. St. Petersburg had been founded by Peter the Great less than thirty years earlier on the swamps of the Neva river delta with the clear intention of creating a capital of the Russian Empire and a great European city. The Leontief family adhered to traditional Russian values and had since before it got established in St. Petersburg belonged to the sect of *Old Believers* which had seceded from the official state church over reforms in the holy texts and rituals adopted in the 17th century.

The Leontief trading business thrived and grew as the city itself for the next hundred years. Under the direction of Wassily Iakovlevich Leontief, who was in charge of the family firm from 1859, it expanded vastly and changed from goods-trading to become a large textile factory producing chintz, i.e. printed cotton fabric. The merchant had become industrialist. Wassily Iakovlevich Leontief, who died in 1893, was Wassily Leontief’s grandfather.

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<sup>6</sup> Frisch (1931, p.282; transl. by ob).

<sup>7</sup> See Koopmans (1957), reviewed in Leontief (1958).



The factory site was located at the confluence of the Zhdanovka and Malaia Nevka rivers, a magnificent site still known as Cape Leontief (*Leont'evski mys*). The factory complex comprised buildings for cotton-printing, dyeing, storage and supplementary facilities, as well as housing for hundreds of workers. The Leontief name was visible and known in St. Petersburg also from a family owned shop for (luxury) metal goods in the *Gostinyi Dvor* shopping arcade on Nevsky Prospekt.

In front of the cotton-printing factory on the banks of Zhdanovka river was a large house built to emulate the grand St. Petersburg style of luxurious palaces for which the city became known but of more modest size and cheaper materials. It served as residence for Wassily Iakovlevich and a number of family members and comprised a small chapel. It was a large family; Wassily Iakovlevich Leontief married three times and had been blessed with altogether fourteen children. The male members of the family filled various functions in the management of the family business.

Wassily Iakovlevich's third wife, Mariia Il'inichna Leont'eva, continued to live in the family residence throughout Leontief's childhood. The upward drift of the social status of the Leontief family within the traditional hierarchical orders of St. Petersburg peaked when Mariia Leont'eva upon her husband's death in 1893 by imperial edict was granted hereditary honorary citizenship for herself and for the children of all three marriages of her husband.<sup>8</sup>

A son from Wassily Iakovlevich's first marriage, Valentin Wassilievich Leontief, became the new head of the family firm. He developed the family business further and succeeded in converting the family-owned factory into a joint stock company by July 1917. This was the culmination of the commercial success of the Leontief family enterprise, but it didn't last long as political events soon after fatally disrupted the family's ownership and control over the factory.

The ninth child of Wassily Iakovlevich was Wassily Wassilievich Leontief (1880-1968) whose mother, the second wife of Wassily Iakovlevich, died in childbirth. Wassily Wassilievich was the father of our Wassily.<sup>9</sup>

Leontief did not take a deep interest in his family history when he grew up. Important facts such as the length of the Leontief family's history in St. Petersburg may not have been known to him until very late in his life when he got in touch with Russian scholars.<sup>10</sup>

## Parents

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<sup>8</sup> See Kaliadina and Pavlova (2006, p.337).

<sup>9</sup> Grandfather Wassily Iakovlevich also had another son named Wassily Wassilievich Leontief!

<sup>10</sup> See Kaliadina and Pavlova (2006), Alpers (2013).

Wassily Wassilievich Leontief (whom from now on mostly will be referred to as Leontief Sr.) was an exception in the Leontief family as he pursued academic studies and an academic career rather than finding a place within the family firm as his brothers and step brothers; they were all involved in the management of the textile mill and the shop on Nevsky Prospekt. He studied in Leipzig and Munich and was away from St. Petersburg from 1899 until 1906. To study abroad was not unusual among well-situated Russians and German universities were often chosen. But the long study period abroad for Leontief Sr. may have been forced upon him by circumstances beyond his control, as set out below. Leontief Sr. may have differed from his siblings also by engaging more actively in politics but the political stance of the Leontief brothers is not known.

It became a family legend in the American Leontief family that Leontief Sr. as a young man instigated strikes among the textile workers of the family's own factory. When Leontief recounted the story, it could be with a quip like – “typical Russian behavior” – without giving details.<sup>11</sup> Perhaps he didn't have many details and he told it as it was about a youthful prank. But it was certainly more serious and important than that. The textile industry of St. Petrograd was at the turn of the century the largest industry in St. Petersburg in terms of employment. There were strikes at several textile factories in St. Petersburg in May-June 1896. The strike among the textile workers spread quickly and at one point in June 1896 it comprised all textile workers in St. Petersburg. Another wave of strikes followed in January 1897. The textile workers' struggle for improvement of harsh working conditions was met with sympathy and material support in wider segments of the society. Leontief Sr. had graduated from the gymnasium at 16 years of age in 1896; and his involvement in the strike took place soon afterwards. The conflict resulted in the adoption of a law in 1897 limiting daily working hours to eleven and a half.

Strikes were not tolerated by the oppressive Tsarist regime and might have serious consequences for those involved. Leontief Sr.'s involvement in the textile workers' strike indicates that he was connected with political circles. He joined later the Socialist Revolutionaries (SRs), the ideological heir to the Narodniki (Populists) of the 19th century, which drew its support from rural Russia on a program of agrarian socialism, as opposed to the emerging Social Democratic Workers Party (SDs) which relied on Marxist doctrines and later split into Bolsheviks and Mensheviks. But as the youngster Leontief Sr. was in 1897 his political views may not have been elaborate beyond the views that all the revolutionary movements of Russia shared: political freedom and abolishment of the autocratic Tsarist rule.

Leontief Sr.'s involvement with the strike made him persona non grata with the authorities, quite likely preventing him from entering St. Petersburg University. Perhaps his honorary citizenship as a member of the Leontief family saved him from harsher

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<sup>11</sup> See Kaliadina (2006, p.348).

sanctions? The strike involvement of Leontief Sr. doesn't seem to have caused any fallout with the larger Leontief family. Perhaps the family on the contrary appreciated his political involvement and facilitated and supported him in his voluntary exile.

After having been enrolled at the Higher School for Commerce in St. Petersburg Leontief Sr. left for Germany to study at the newly opened *Leipzig Handelshochschule* in 1899, graduating two years later. After Leipzig he entered doctoral study at Munich University in 1902, completing it eventually in 1906. He suspended his Munich university enrolment, however, in 1904/05 to spend one year in Paris. During that year he met Zlata Bekker (1881-1979), a well-educated and strikingly beautiful woman of Jewish extraction who studied in Paris. Possibly they might have become acquainted earlier. Zlata Bekker (1881-1979) had come to Paris from Odessa in 1901. She belonged to a well off Jewish family with economic interests in transportation business. Jews were barred from university enrolment in Russia; hence Jews who could afford it went abroad to study. Zlata chose Paris rather than Germany and studied briefly medicine at the University of Paris. She changed to a study of art and art history in a Russian school of higher education established in Paris. Zlata Bekker was described at the time as a striking, exotic beauty.

From the beginning of 1905 Wassily and Zlata could follow the news from Russia through the dramatic year which would go down in history as the 1905 Russian Revolution. It began with the Bloody Sunday, the massacre of hundreds of political demonstrators outside the Winter Palace in St. Petersburg. One of the demonstrators' main demands was for a constituent assembly to be convened. For Wassily and Zlata it must have felt difficult to be away from Russia at such a fateful time. The politically engaged Wassily may have been exhilarated at the thought of a new reformed and democratic Russia breaking through. Zlata may not have been less interested in the political outcome but perhaps more concerned with the dangers the political turbulence and violence could mean for her family. But they did not have much choice; Zlata was pregnant. They made a hasty decision to go London to get married; the civil marriage ceremony took place on 17 March 1905. Afterwards the newlywed couple moved to Munich. Zlata Bekker enrolled at Munich University and intended to follow lectures for as long as Wassily had to remain in Munich for his degree. But Zlata Bekker had to suspend her study plans to prepare for childbirth.<sup>12</sup> Wassily Leontief was born on 5 August 1905.

1905 became a hectic year of political demands and struggle. The Tsar made attempts to saving his regime by offering reforms, e.g. the establishment of a parliament (*Duma*), but the Tsar retained absolute power. Thus a constituent assembly was never convened. The military remained loyal to the Tsar and made it possible quashing all opposition,

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<sup>12</sup> Alpers (2013, p.23). Zlata Bekker eventually completed a degree at Petrograd University in 1923.

executing thousands and imprisoning and exiling tens of thousands. A fascinating personal account of the 1905 events is given by in Wladimir Woytinsky.<sup>13</sup>

Several of Zlata's family members in Odessa were involved in revolutionary activities in 1905; one brother was shot, other members of near family were arrested. In the aftermath of the 1905 political turmoil pogroms erupted in the western part of the Russian empire. In Odessa alone, eight hundred Jews were murdered, five thousand injured, and a hundred thousand left homeless.<sup>14</sup> Zlata's parents and a younger sister, Liuba, fled Odessa in October 1905 and after a strenuous journey to Munich they moved in with the Leontief couple and their baby son until they could safely return to Russia.

The doctoral dissertation of Leontief Sr. was on the cotton industry in St.Petersburg and its workers: *Die Baumwollindustrie in Petersburg und ihre Arbeiter*.<sup>15</sup> A part of the dissertation is built on statistics of a cotton-printing factory with about 500 workers which Alpers takes to be the Leontief firm. Wassily Wassilievich discussed in the dissertation the strike action in 1896 without bringing himself into the story. From his dissertation topic Leontief Sr. could be denoted as a labor economist, his special interest was the conditions of the laboring class. But he seemed to have quite comprehensive knowledge of Russian economy and history as well. One may read out of his dissertation a political message expressing belief in what the trade unions can achieve in terms of social and political reform. The dissertation was at its final stage during the political turbulence in Russian known as the 1905 revolution. The revolution comprised bloody suppression but also opened up possibilities for political reform, which in the end were thwarted.

His granddaughter, Svetlana Alpers, has scrutinized the dissertation and characterized it as "neither historical nor analytic, but rather a journalistic affair."<sup>16</sup> The characterization suggests that Leontief Sr.'s dissertation was not a particularly impressive one, perhaps reflecting to some extent that he had conducted a minimal amount of university studies before he embarked on the doctoral dissertation. But even so, its descriptive approach may not have implied that it was of a lower scholarly quality than many others. Leontief Sr. The doctoral advisor of Leontief Sr. in Munich, Lujo Brentano, was a scholar of great credentials; he was an economist adhering to the German historical school with strong interests in social reform, a true *Kathedersozialist*.

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<sup>13</sup> Woytinsky (1961, pp.3-145). Woytinsky who was five years younger than Leontief Sr. took active part in the political activity in 1905, after which he endured four years imprisonment followed by exile in Irkutsk until 1917.

<sup>14</sup> Clark (2016, p.58), Alpers (2013, pp.36-42).

<sup>15</sup> See Alpers (2013, pp.13-19). Munich University liberally accepted Wassily Wassilievich despite having no university exam; the Leipzig *Handelshochschule* did not have university status, cf. Wittich (2016).

<sup>16</sup> Alpers (2013, pp.17-18).

## Childhood and formative years

When the Leontief couple were on their own again and the situation in Russia had calmed down they made preparations for returning to St. Petersburg as soon as all formalities of the doctoral degree had been completed, which was not until July 1906. The preparations comprised Zlata's conversion to Orthodoxy which eventually took place on 2 August 1906. Zlata became Genia; her full Russified new name was Evgeniia Borisovna. Two days later followed marriage in the Greek Orthodox Church in Munich. The motivation for the conversion and re-marriage was, we may presume, the religious conviction of Leontief Sr. but perhaps more important as reverence for the Leontief family who had been supportive of Leontief Sr. throughout his years abroad and to whom he intended to return with his wife and son.

The Leontief couple returned from Munich to St. Petersburg in mid-August 1906. Little Wassily was just over one year old. At the return, Wassily was baptized and entered into the birth register of the Russian Orthodox Church as born on 5 August 1906 in St. Petersburg. The doubly incorrect birth data became the official birth place and date. Throughout Leontief's life in Russia from one to nineteen years of age he (and his parents) lived with this odd fact of being one year older than officially recorded. Leontief's passport issued just before he left Russia in 1925 carried the incorrect birth data.<sup>17</sup> Whether the odd misstatement of the place and year of Leontief's birth ultimately was just an odd and unfortunate accident or a falsification by the Church with connivance of the parents (or the other way around), we leave aside here.<sup>18</sup>

Since the return from Munich the family of three lived in one of four apartments in a house located in the San-Gali township, a settlement on Petrovskii island on the right bank of Neva. It was shared with two other sons of Wassily Iakovlevich: Aleksander and Leonid. A childhood memory for Wassily from this still peaceful time in St. Petersburg was that he and a cousin played with two bear cubs in the garden.

Leontief Sr. brought his little son quite a few times to the house on the Zhdanovka River where Wassily's step grandmother, Mariia I. Leont'eva, lived, not far from the San-Gali house.<sup>19</sup> Wassily retained at high age a vivid memory of the grand house designed by

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<sup>17</sup> The same was the case for Leontief's American passport issued after the naturalization in 1938, and his autobiographical information at the Nobel Prize award in 1973,

[https://www.nobelprize.org/nobel\\_prizes/economic-sciences/laureates/1973/leontief-bio.html](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1973/leontief-bio.html)

<sup>18</sup> The significance of the wrong birth year was that it implied the appearance that Leontief was born of an Orthodox, rather than Jewish, mother. The fact that the Leontief family adhered to the Old Believers may have played a role. It is uncertain when Leontief became fully aware of the facts but as both parents were enlightened and liberal persons he was hardly very old. The naturalization was one of several opportunities to resolve the issue but Leontief acted as if such details meant very little to him, cf. Bjerkholt and Kurz (2006, p.332).

<sup>19</sup> Kaliadina and Pavlova (2006, p.338).

his grandfather who had not been an architect but liked to do everything by himself. The house was modeled on the style prevalent in St. Petersburg and located next to the factory at the confluence of Zhdanovka and Malaia Nevka rivers. Leontief remembered “a greenhouse with high palms in the house, and a huge ballroom where we used to dance” and small rooms like chapels with icons and icon-lamps where “lived a lot of aunts and dependents.”<sup>20</sup>

The Leontief family property also comprised an estate in Finland (under Russian suzerainty until 1918), serving as a *dacha*. It was located on the Karelian isthmus near the river Vuoksi and the Imatra falls, not very far from St. Petersburg. Birch forests and lakes with fishing opportunities made the estate attractive.

A special memory connected with the Finnish estate came to Leontief’s mind when he was interviewed late in life about the political unrest in Russia in his youth: “I saw something on my own. We had a small estate in Finland, on the Karelian isthmus near the river Vuoksa. ... when revolutionaries tried to leave Russia, they on occasion hid on our estate.”<sup>21</sup> This interesting but somewhat vague remark as to specific circumstances, again suggest political involvement on the part of Leontief Sr. The revolutionaries hiding on the estate were hardly just happening to pass by. It had more likely been arranged with the connivance of Leontief Sr.

Leontief’s parents loved their son and doted upon him. They called him a Wassiliok, an endearment term used only within the family.<sup>22</sup> He was educated by private tutors at home from a very early age by nurses and later German and French governesses. Leontief acquired high fluency in both languages while his English, much less called for in that period, lagged far behind. Some events left strong traces in memory such as the death of Tolstoy in 1910, which was remembered by Leontief from a documentary film about Tolstoy shown in the school on the day after Tolstoy died.<sup>23</sup>

From Leontief was quite small and up to the World War I he went with his parents on long European trips, perhaps in most of these years. The destination was most often Italy and Florence in particular, not least due to Genia’s interest in art and art history. The travels involved crossing through Germany and either Austria or Switzerland.<sup>24</sup>

During a leisure trip in Russia in 1910 the Leontief family befriended a German professor. He was Karl Stählin, a well-known professor of history at the Berlin University. The trip was on the Volkhov River, up river to Novgorod. They got on well with each other and the trip was vividly remembered also by the five-year old Wassily. They had

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<sup>20</sup> Kaliadina (2006, p.347)

<sup>21</sup> Kaliadina (2006, p.348).

<sup>22</sup> Alpers (1989, p.24).

<sup>23</sup> Alpers (1989, p.7).

<sup>24</sup> Alpers (2013, pp.27-29).

ample time on the boat and Stählin got the full story about how the couple had met in Paris and then lived in Munich for two years. A letter Stählin wrote to his wife revealed that he was spellbound by Genia. “She is charming and a lady of so high intelligence as I have never met before... She is small, with a peculiar gracefulness and with dark great eyes, black as coal. She is from Odessa, surely Jewish extraction.” Stählin was told that he would be welcome to visit the family at the *dacha* in Finland. He turned up at Imatra falls there one week later and was equally fascinated. He wrote home, “It is time I come home, I think, otherwise I will fall in love with a Jewess from Odessa.”<sup>25</sup> Stählin’s instant fascination suggests that also Leontief Sr. may have been mesmerized at the first sight of Zlata in Paris. Stählin had traveled much in Russia as his special field was the history of Russia. He had been a military officer before he became a historian. Leontief would meet him again at his home in Berlin. His overall recollection of Stählin was that of the best kind of conservative German intellectual, very liberal and a typical *Gelehrter*.

In 1911/12 Leontief Sr. was appointed *privatdozent* at the Imperial University in Iur’ev (today Tartu in Estonia) to which he commuted once a week to lecture on public finances.<sup>26</sup> Before he got the position he probably took an exam in Iur’ev, as a degree from a Russian university was required for a teaching position at a Russian university. Leontief Sr. was not dependent upon this teacher position for the living as he still had his share of the larger family property although he did not take very active part in running the business. He succeeded in achieving a position as professor at Petrograd University (as it was renamed) where he lectured on foreign trade policy from 1915.

Wassily was nine when World War I ignited far away from St. Petersburg but Russia quickly got so involved in the war that her losses were the highest of all the warring nations. During the first nine months of the war, from August 1914 until June 1915, Russia lost just under two million men, of whom 764 000 were captured.<sup>27</sup>

After being tutored at home Leontief was sent to school, first a private school then a public one. A memory from the end of 1916 was the death of Grigori Rasputin, the peasant, faith healer and seducer who for years had held a sway over the Tsar and the Tsarina. The gruesome murder took place not very far from Leontief’s home. Leontief was still in the private school at the revolution in March 1917.

The second revolutionary year in Russian history took place in the third year of World War I. It was initiated by strikes, street demonstrations and riots. The Tsar’s control over

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<sup>25</sup> Alpers (2013, pp.30-31, transl. by ob). The letter is printed in a book published by Stählin’s daughter 1968 (private edition).

<sup>26</sup> Iur’ev is Tartu in Estonia, known in German, Polish and Swedish as Dorpat. The university was founded under Swedish rule in 1632 by king Gustavus Adolphus. Tartu came under Russian rule in 1721.

<sup>27</sup> Clark (2016, p.59).

military soon turned out to crumble as workers and soldiers took to the streets and troops refused to follow orders to crush the emerging uprising. The Tsar abdicated and was replaced with a provisional government in March 1917. At the same time workers' and soldiers' councils (soviets) were established and became battleground for struggle between SRs, Mensheviks, Bolsheviks and other factions. Leontief retained memories of the great demonstrations taking place and of accompanying his father to the square behind the Winter Palace to attend speeches by Lenin, Zinoviev and others.<sup>28</sup>

Alexander Kerensky, a Duma member since 1912 and well-known political opponent of the Tsarist regime, adhered to the SRs and became from July 1917 Prime Minister of Russia (after having served in the preceding months as Minister of Justice and Minister of War). Pitirim Sorokin who had studied criminology at the St.Petersburg University and become professor there, served as Kerensky's secretary. Leontief Sr., who adhered to the SRs like Kerensky and Sorokin, got a position in the Ministry of Labor. He had worked for the government also under the Tsar on negotiation of commercial treaties. Throughout 1917 the SRs vied for political power and influence with Bolsheviks, Mensheviks and other contenders with an active role also played by soldiers and military units.

Bolsheviks and Mensheviks were largely outnumbered by SRs in Russia as a whole but in St. Petersburg the situation was more equal. After having suffered several defeats through 1917 the Bolsheviks succeeded however in outmaneuvering their opponents and usurped power by overthrowing the Kerensky government in November 1917. Woytinsky played a more prominent role in the 1917 revolution than in 1905 and wrote an equally fascinating account.<sup>29</sup> He saw the Bolshevik coup as the end of the prospects for a democratic Russia, which turned out to be correct.

The lost cause of a constituent assembly in 1905 was raised again in 1917, as a most important issue. Although there was no disagreement about having a constituent assembly it was tactical maneuvering which caused delay in the date for the election of the assembly. The election date was set to be in September 1917 but was postponed until December 1917. The Bolsheviks had apparently expected to win the election and thereby underpin the legitimacy of a claim on the power already usurped. But the SRs won the election hands down.<sup>30</sup> Among those elected to the Constituent Assembly on the list of the SRs was

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<sup>28</sup> Rosier (1986, p.78).

<sup>29</sup> See Woytinsky (1961, pp.243-399). Woytinsky also ended up in USA where he worked as an economist and statistician after immigration in 1935. His path there is likely to have crossed that of Leontief and Leontief Sr.

<sup>30</sup> The SRs polled more than 17 mill. Votes and the Bolsheviks less than 10 mill., out of a total of 40 mill votes cast (Wikipedia). The Bolsheviks won in the cities and among the soldier while the SR's swept the (numerically much larger) countryside.



Leontief Sr. He does not seem to have been known as a political activist and had perhaps not expected to be elected.<sup>31</sup>

The elected Constituent Assembly convened on 18 January 1918. The event was a decisive point for the future of Russia after the fall of the Tsar. But the Constituent Assembly never came to play any role in Russia as it was dispersed by the Bolsheviks after one day's proceedings; it succumbed to Bolshevik power and was abolished virtually as it was convened.<sup>32</sup>

The Bolshevik coup caused the eruption of the Russian Civil War in which many factions vied to determine Russia's political future. The Bolshevik Red Army fought against loosely allied, but politically highly diverse forces, known as the White Army. Eight foreign nations intervened against the Red Army. The Red Army eventually defeated the White forces and the Bolsheviks at the same time ruthlessly suppressed political opponents, such as the SRs. The Russian Civil War period which lasted for about five years was a national catastrophe with an estimated 7 – 10 mill. casualties during the war, mostly civilians.<sup>33</sup> Needless to emphasize the World War, followed by the upheavals caused by the revolution and civil war caused enormous hardship in Russia through the ravages of war and lack of the most basic necessities, resulting in large numbers of people fleeing Russia.

After the Bolshevik coup Leontief Sr. may have been in danger as a political foe but he was never arrested. In Leontief's view this "probably because of the traditional and deep-seated respect among the Russians for academic people and particularly university professor."<sup>34</sup> REF But Leontief may not fully have understood his father's politics and the perils involved. For a shorter period in 1918 the Leontief family went into hiding in the countryside 400 km east of Petrograd. After returning to St. Petersburg the Leontief family was in 1919 evicted at 24 hours notice from their flat on Petrovskii Island in St. Petersburg by "revolutionary sailors."<sup>35</sup> According to Leontief his parents never complained about their fate or expressed anger about what was going on. They retained their revolutionary view they had supported at the outset of the 1917 revolution. Leontief recalled that the parents once said to him that they used to think that they had enough money to pay for his education abroad, but "now we do not. ... you must try to do it all by yourself." The

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<sup>31</sup> The circumstances surrounding his nomination and election, including in which district he was elected, have not been pursued.

<sup>32</sup> The election of the Constituent Assembly is acknowledged as the first free election in Russia and the only one until the end of the Soviet period.

<sup>33</sup> See Mawdsley (2007, p.287).

<sup>34</sup> From autobiographical notes by Leontief 1954.

<sup>35</sup> Kaliadina (2006, pp. 348-9). In unpublished notes Leontief denoted the evictors as "Lenin's sailor shock troops".

Bolshevik coup meant the end of the Leontief family's control of the company and a severe loss of family owned assets.

The Bolshevik regime made early attempts at reforming the school system and from 1918 Leontief's school became a Unit School. The teaching staff remained much the same as before the revolution with little or no inculcation of politics. But politics had become a subject of bitter divisions among the pupils who split up and fought with each other, driven by their political positions and sympathies. Curiously, Leontief had as class mates both the son of Kerensky and a daughter of Trotsky. Leontief graduated in the spring of 1921.

In Leontief's reminiscences about his early life the politics of his father is unduly subdued. Leontief regarded himself as coming from the bourgeois class and emphasized that his father, a true Russian intellectual, was in no way a defender of the old Tsarist regime.<sup>36</sup> In fact most intellectuals and also many others Russians were opposed to the Tsarist rule. And it was a rather backward leaning way of describing Leontief Sr. as the evidence indicates that he since very young had been politically engaged through the SRs, the Russian brand of socialism and political thought. The abolishment of the Constituent Assembly in 1917 followed by the suppression of the SR's a couple of years later could hardly have been felt by Leontief Sr. as anything but a crushing defeat, and even more so when the civil war ended in entrenched Bolshevik power with diminishing hopes for a political reshuffle.

Leontief Sr.'s salary as professor was too low to provide a living after 1917; he complemented his income by teaching in various educational institutions and also by writing reports for the government on technical economic subjects.

### **Student at Petrograd/Leningrad University 1921-1925**

Leontief entered the Faculty of Social Sciences, Petrograd University in the autumn of 1921 with special permission of the Ministry as he was only 15 years of age (but really 16). Leontief has commented about entry to the university that he started with philosophy. On finding that it was not exactly what he wanted; he moved to sociology but didn't find that matching his interests either. He then "descended" into economics and found that he there had a thousand ideas he would like to pursue.<sup>37</sup>

Leontief studied economics with professors S. I. Solntsev and I. M. Kulisher, and history with professors Sergei Fyodorovich Platonov (1860-1933) and Evgeni Viktorovich Tarle (1874-1955). These were the teachers who meant most to Leontief. He also followed lectures by and had scholarly contact with others, foremost with professors A. Iu.

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<sup>36</sup> Rosier (1986, p.78).

<sup>37</sup> Rosier 1986, p.77.

Finn-Enotaevskii, O. D. Khvol'son and V. V. Stepanov.<sup>38</sup> Tarle was the best known historian of his time, particularly for his work on Napoleon's invasion of Russia.<sup>39</sup>

Leontief was interested in broader social studies and sat in on workshops and lectures given in the faculty but not part of the economics program; on his graduation certificate there is a note that he passed a non-obligatory exam in sociology. He attended lectures given by Pitirim A. Sorokin until he was forbidden to teach some time in 1922.<sup>40</sup>

A characteristic feature of Leontief's study habits was his intellectual appetite on classical works: "When I was a student, I used to read a lot ... studying the original works of economists of the 17th and 18th centuries. At that time no one but me read those books."<sup>41</sup>

He read in Russian, French, German and English, "We didn't have new editions; I used the first editions, big leather-bound volumes".<sup>42</sup> In an interview in 1986 he gave more details: "I read many books of political economy in French, all the old master since Boisguilbert". He stated further that he had read François Quesnay in 1921-1922, "...I had no thought yet of input-output".<sup>43</sup>

Leontief was regarded from early on as one of the most talented and promising students. Already from the first year he wrote research papers. Under the guidance of Professor S. I. Solntsev, he wrote a paper on *Analysis of the formulas of social reproduction*.<sup>44</sup>

In the summer of the same year 1923, Leontief, together with A. V. Mikeladze, translated from German a book by Carl A. Schaefer on systems of currency stabilization.<sup>45</sup> It thus was concerned with problems of great actuality for the country whose monetary system was in a lamentable state. The translation evoked praise from Professor A. Iu. Finn-Enotaevskii, who wrote of Leontief that he "... knows how to clarify the most complex problems of economics." The experience was a cherished memory for Leontief, so

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<sup>38</sup> Kaliadina and Pavlova (2006, pp.339-340).

<sup>39</sup> The historians Tarle and Platonov were both arrested in 1930. Tarle was exiled to Alma Ata until 1934. Platonov was exiled to Samara where he died in 1933. Tarle suffered the sad fate of being pressured to succumb to Stalin's demand for rewriting the history of the Napoleonic wars.

<sup>40</sup> Kaliadina (2006, p.349, n.6).

<sup>41</sup> Kaliadina 2006, p.351.

<sup>42</sup> Alpers (1989, p.21).

<sup>43</sup> Rosier (1986, p.77 & 89, transl. ob).

<sup>44</sup> Kaliadina and Pavlova 2006, p.339. Unfortunately, nothing more is known about this paper. The title is titillating, knowing Leontief's later interests.

<sup>45</sup> Published as *Classical Systems of Currency Stabilization* [in Russian], Petrograd, 1923 from original published in Germany 1922.

apparently he learnt much from it and got him his first honorarium.<sup>46</sup> Another summer experience for Leontief was to be research assistant in an astronomical observatory.<sup>47</sup>

Leontief who was deeply interested in Russian economic history completed in 1924 a study of the Russian economic administration in the eighteenth century.<sup>48</sup>

His voracious reading appetite was noted by the director of the library who discussed with Leontief about his research ideas and suggested to him to write a paper which Leontief titled *Laws in the social sciences – the experience of abstract-logical analysis* and described in the following words:<sup>49</sup>

“The article was about the development paths of science, about the causal and normative approaches in science. I reviewed the development of the two approaches, starting from the philosophers of the 18th century, through Kant and Hegel, finishing with Bergson. It was an historical-analytical article. It was very far from any politics or ideology.”<sup>50</sup>

After presentation in a study circle on the methodology of social sciences and political economics, the paper was accepted by the academician E.V. Tarle for a journal published Academy of Science.<sup>51</sup> But censorship intervened and prevented the publication. Leontief’s reaction, as recalled at high age, was,

“...if they forbade even such an article...I understood that it was not possible to work as a scientist here. Well, may be to do partial work, but not to work normally. And my work – that was the most important thing in my life.”<sup>52</sup>

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<sup>46</sup> Rosier (1986, p.79).

<sup>47</sup> Alpers (1989, p.15).

<sup>48</sup> In Leontief’s application to NBER in 1930 the paper was listed as *Russian economic administration in the XVIII century*, a documentary study prepared for the Russian Central Archive, 1924.

<sup>49</sup> It may be the same paper as listed as *Philosophical background of the methods of political economy* (1925, Leningrad) in the CV referred to in the previous footnote..

<sup>50</sup> Kaliadina (2006, p.351).

<sup>51</sup> The library director was Ernest N. Radlov, philosopher and head of the Russian Public Library. Academician Evgenii V. Tarle was a well-known historian . The journal *Annally* published by the Academy of Science was a new but short-lived one, published only 1922-1924. See Kaliadina and Pavlova (2006, p.339), Kaliadina (2006, p.351, n. 1 & m).

<sup>52</sup> Kaliadina (2006, p.351).

The experience left a deep impression on Leontief. It became within a short time a crucial experience with regard to his wish to pursue a scientific career. It led eventually to his irrevocable decision to leave Russia.

There was much political discussion among the students in Leontief's first and second study year and Leontief took an active part:

“Students are students, always and everywhere. They always discuss world problems, they always dispute. And I disputed a lot with my student-comrades. ... We argued about whether we needed freedom in our state or not, whether it should be limited or not. We argued about the situation in higher education. And I argued a lot with my communist colleagues. So when I said things that they found entirely impossible, I got taken to the Gorovkhaia, was held for several days, and then released again.”<sup>53</sup>

Leontief got on well with his fellow students. He was interested in politics but not politically engaged or committed. He was also very young. Leontief was arrested several times and interrogated about opinions he had stated. He presumed that this was due to denunciations, including some by classmates. In one interrogation Leontief was confronted with what he had said in a face to face conversation with a fellow student whom he liked and knew that he was convinced communist with a “correct” understanding of Marxism, hence he could identify the denunciator. At the same time the investigators that this student had conveyed a very positive opinion of him and he was released shortly afterwards.<sup>54</sup>

The most serious incident which led to the arrest of Leontief and many other students took place in 1922. After unrest and a teachers' strike at the university in the spring of 1922 some professors were banned from teaching, one of them Leontief's teacher Pitirim Sorokin. The unrest continued and the government prepared retaliatory measures to suppress “the influence of anti-Soviet groupings of the intelligentsia.” The majority of students supported the teachers. Leontief took part in mounting large posters on military building walls. He was arrested together with an older girl student carrying posters and a pail of glue.

“We called for freedom: freedom of the press, freedom of expression, freedom in the state. We protested against the suspension from teaching of our professors. We went with the posters at three at night. But probably they knew about us in advance. We were followed almost from the very beginning. We went and stuck up the posters, but

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<sup>53</sup> Kaliadina (2006, p.349). Gorokhovaia was the headquarter of the Petrograd Cheka (predecessor of GPU, NKVD, KGB) was located in the center of Petrograd (on Gorokhovaia street).

<sup>54</sup> Kaliadina (2006, p.349).

half a *verst* away some people followed us, then they overtook us and arrested us. We were sent to the Gorokhovaia.”<sup>55</sup>

This incidence resulted in long imprisonment (for a student), perhaps three months. The experience left very vivid memories which Leontief told and re-told with many intriguing details. He described the cell (or one of them) as having 4-5 meters to the ceiling with a very weak bulb (10 watts) high above him. His parents were entitled to bring book and he had requested a book about Rodbertus as he was then preparing a paper about Rodbertus at the university. But he could read the book only by “standing on one foot on the plank bed, to be a little closer to the light ... so in this manner I prepared that work.”<sup>56</sup>

Leontief’s account had descriptive feature known from other accounts, such as being held much of the time in solitary confinement, “in half-dark, verminous, cold cell” with interrogations at night, beginning say at three o’clock. “Usually there were two men ... they scare you, they say, you know, we can shoot you.”<sup>57</sup> But when you are fifteen, one cannot, doesn’t imagine.” Leontief’s version of the interrogations he was exposed to was much less sinister. He tended to give a rosy and somewhat naïve account of the whole affair, e.g. that the interrogators were intellectuals with whom one could have long discussions on Hegel, Marx and Russian philosophy. Leontief was, however, not aware of the dangers. To him the demonstrations which led to his arrest and interrogations amounted to nothing more than “insignificant episodes.” But Cheka arrests were much more serious than that. He regarded the Cheka interrogators he met with as intellectuals, “even sincere, convinced intellectuals” with whom he could continue in prison the kind of discussions he had with communist students at the university.<sup>58</sup>

He did not realize understand how ominous these confrontations really were. He never felt afraid, although he knew that his parents were very worried, as he didn’t realize the danger. In a moment of reflection over his attitude then he admitted: “Maybe I did not quite understand, I was young, only 16 in 1922. The most important in life for me was always science.”<sup>59</sup> Leontief and the other students arrested did not know, however, how fortunate they were. They were treated correctly, interrogated by intellectuals, and released after relatively brief incarcerations. Several had earlier paid with their lives for less serious activities. The mild reaction after the 1922 demonstrations was a lucky coincidence, caused by a power struggle within the Cheka. The activity of the Petrograd Cheka had

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<sup>55</sup> Kaliadina (2006, p.350).

<sup>56</sup> Kaliadina (2006, p.351).

<sup>57</sup> From autobiographical notes by Leontief 1954.

<sup>58</sup> Kaliadina (2006, p.350), Alpers (1989, p.11), Rosier (1986, p.79).

<sup>59</sup> Kaliadina (2006, p.350).

caused dissatisfaction, frightened the intelligentsia, and caused emigration to an extent that called forth critique at the highest level within the Soviet government. The head of the Petrograd Cheka was removed at the end of 1921 and the investigators replaced by others “more experienced as chekists.”<sup>60</sup>

In September 1922 the Soviet government expelled Sorokin at the same time as 160 scientists, many of them professors were embarked on a ship and sent abroad.<sup>61</sup> Sorokin left separately and went to Prague, the headquarter of the SRs.

In 1923 while still a student in the University Leontief developed a deformity in his jawbone diagnosed as sarcoma, i.e. a malignant type of cancer. In unpublished notes Leontief stated that was operated on by the former surgeon of the Tsar. In the course of the operation it was found that the trouble was due to a fibroma, i.e. a benign tumor. After the operation he was in convalescence for some months. The scar from the operation was visible the rest of his life and lasting memory from it was a piece cut out from his right cheekbone.

Leontief graduated in 1924 as Learned Economist in the Social Sciences.<sup>62</sup> In the interim between graduation and his departure from Russia he was selected for retention at the university and appointed as research assistant. In his petition for retention S. Solntsev evaluated Leontief as follows:

“In his work, Leontief revealed excellent research capacities in the field of political economy, good capacities in the field of economics work, good knowledge of the literature in theoretical economics, a subtle understanding of the basics of economic Marxism, and great assiduity in and love for science.”<sup>63</sup>

It was announced, however, that the entire Faculty of Social Sciences at which Leontief had studied would be abolished in the following academic year. He would have to move to Moscow. In May 1925 (i.e. after his departure from Russia) he was listed as employed both at Leningrad University and at a research institute at Moscow State University.<sup>64</sup>

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<sup>60</sup> See Kaliadina (2006, author’s notes 6, p.354).

<sup>61</sup> Kaliadina (2006, p.349, n6&7).

<sup>62</sup> On January 26, 1924, five days after Lenin's death, Petrograd was renamed Leningrad and the university followed suit soon after. Leontief thus graduated from Leningrad University.

<sup>63</sup> Kaliadina and Pavlova (2006, p.340).

<sup>64</sup> Kaliadina and Pavlova (2006, p.340). In the brief CV (*Lebenslauf*) submitted at his doctoral exam in 1927 Leontief stated that he after graduation in 1924 became “scientific assistant” both at the Leningrad University and at the *Volkswirtschaftlichen Hochschule*. (*Lebenslauf*, 1927, Humboldt University).

Leontief had decided on pursuing a scientific career. To leave Russia was not part of the plan. It was the censorship he had been exposed to that made him consider in depth to leave Russia. The censored article had not been political or provocative in any way. He concluded that despite his natural attachment to Russia continuation of scientific work abroad had become a preferred alternative over remaining in Bolshevik Russia.<sup>65</sup>

Leontief went to the surgeon who had operated on him and obtained from him a certificate stating that he had sarcoma and needed treatment abroad. He attached this document when he applied for passport to leave the country and it may well have been helpful. Possibly Leontief Sr. may also have been able to exert an influence through personal relationships. After some waiting time the application was granted and passport issued in the beginning of 1925.

After that remained only the final decision. It was not an easy decision. The experience of the first few years of the Bolshevik regime had been tough in various respects but Leontief stated that he wouldn't have liked to be without that experience. It had not made him angry and filled with hostility as was the case for many *émigrés*, e.g. Alexander Solzhenitsyn. He did not agree with the people in power but understood what they were doing.<sup>66</sup> The censorship episode was serious but did not rule out that he could get conditions for scientific work. To leave his loving parents, not knowing when he would see them again, was hardly easy, although the parents did nothing to hold him back. Leontief was not in doubt. There was always an adventurous streak in Leontief, maybe was decisive. As Leontief reminisced about it sixty-five years later: "There was no selfishness; they really felt that I should get away."<sup>67</sup> He was still only nineteen.

Leontief acquired a broad educational background through four years (seven semesters) at Petrograd University with studies in economics, philosophy, history, and sociology and graduated as a top level student. The mathematics he had with him from school and university is not known in detail but sufficed to make him a mathematical economist. He had undertaken exceptionally comprehensive readings of classical literature and had had opportunity to pursue research sufficiently to develop a strong interest in making that a life-long pursuit. An interest in overall functioning of the economy can be traced back to his study years. One may wonder whether Leontief was exceptionally lucky in not having the study years in his late teens disrupted or seriously affected by civil war, supply disturbances, political unrest, etc. or by imprisonment and cancer. When he left Leningrad in 1925 his baggage comprised in addition to the knowledge acquired as recorded on his university certificate, all the wealth of cultural experiences of a St. Petersburg upbringing, a substantial amount of close at hand political observations, and a personality exuding

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<sup>65</sup> Kaliadina and Pavlova (2006,p.340).

<sup>66</sup> Rosier (1986, p.79).

<sup>67</sup> Alpers (1989, p.17).



intellectual curiosity and tolerance without the bitterness and fanaticism prevalent among many of those who left Russia at the same time but often under much unhappier circumstances.

It is almost as if the picture drawn by Leontief of a happy childhood and eventful school and university years in memories rendered late in life on many occasions in interviews and informal reminiscences can be doubted as a too rosy picture, underrepresenting dangers, deprivation, and traumas, as if the latter had been washed out of his memory.

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On the eve of his departure from Petrograd Leontief received from Professor I. M. Kulisher a letter for Werner Sombart dated 20 March 1925:

“I allow myself to recommend to you the bearer of this letter, Mr. Wassily Leontief, as one of my most gifted students. I have repeatedly had opportunity in my seminar on financial science at Petersburg University, which he has completed, to learn to know him as a hardworking participant. He has also undertaken interesting archive studies on the history of Russian industry in the 18th century, and these, as I have seen from his accounts, been conducted wisely, but due to his illness he has been prevented from finalizing them. Now, that he has left for extended studies in Berlin, he has particularly wanted to continue his further scientific education under your guidance, as I have heartily advised him.”<sup>68</sup>

The transition from Leningrad University to Berlin University was obviously eased by the connection between Kulisher and Sombart. It is interesting to note already here that Leontief was guided in a somewhat similar way from Berlin University to the Kiel Institute, from the Kiel Institute to NBER, and from NBER to Harvard. Obviously, the recommendations and invitations were all founded on the conception of Leontief being a highly gifted, or even quite exceptional, candidate.

## **Berlin 1925-1927**

At the end of March 1925 Leontief entered a train that took him from Leningrad to Berlin with his suitcase and his recently issued passport. He retained a memory of a half empty train passing through Estonia.

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<sup>68</sup> Kulisher to Sombart, 20 March 1925, transl. by ob. The letter in the Leontief archive seems to be the original, perhaps it was returned to him by Sombart. Another of Leontief’s teachers, Professor Solntsev, had studied in Berlin and knew Borkiewicz well.

Hundreds of thousands Russians left their country during and after the civil war to become *émigrés*. Most of the *émigrés* settled Germany and France, at least temporarily, and particularly in Berlin and Paris. Berlin alone accommodated between two and three hundred thousand Russian *émigrés*, many of them clustered in the southwestern suburbs of Charlottenburg, Schöneberg und Wilmersdorf. The number of Russians swelled also in Paris and Prague. But Berlin was a particular popular choice. It was easy access from Russia. And there was a strong interest in Russia and Russian matters in Germany.

Germany was not unfamiliar foreign territory. “Our family was a very intellectual family and Europe was close.”<sup>69</sup> As a small boy Leontief family had travelled extensively in Europe with his parents and crossed through Germany on many occasions. He had learned German very well and had had opportunity to practice it while growing up. After leaving Bolshevik Russia for Germany he was prepared to spend his life there: “I began a regular German career, scientific career and everybody accepted it”.<sup>70</sup>

Leontief did not have friends or acquaintances among the *émigrés* and generally eschewed them. He did not regard himself as an *émigré* and felt he had a different background from those who had fled Russia. He had not fled but his departure was still irreversible. He didn’t know of any student in Petrograd who like him had left for studies in Berlin. He was very much on his own (“I had practically no social life”).<sup>71</sup> He ran across Russians in Berlin, of course, but did not develop personal relations with very few of them. One was a banker of Russian origin named Mitnitsky who assisted Emil Lederer with the editing of *Archiv für Sozialwissenschaft und Sozialpolitik*. Mitnitsky had a son about five years younger than Leontief. At Mitnitsky Sr.’s request Leontief gave the son his first lessons in economics.<sup>72</sup>

Leontief’s life in Berlin was simple and materially poor, in particular to begin with. Leontief’s father had hardly been able to supply his son with much support in terms of financial resources when he left Russia. Leontief Sr. was still professor at Petrograd University but his income hardly sufficed for much beyond necessities after the confiscation of the assets of the family business. Leontief succeeded, however, in finding a way to earn some money in Berlin. From 1925 to 1927 he wrote for the largest Russian commercial weekly review general coal and iron market reports for all leading countries. The review was published (in Russian) in Berlin and Leontief had got the review

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<sup>69</sup> Alpers (1989, p.17).

<sup>70</sup> Alpers (1989, p.24).

<sup>71</sup> Alpers (1989, p.23).

<sup>72</sup> Mitnitsky’s son studied economics in Heidelberg before he emigrated in the late 1930s to USA where he became Mark J. Millard, and made a career on Wall Street. Leontief retained contact with him until Millard died in 1985: “He still called me Wassiliok, the only man who to the very end called me Wassiliok.” Alpers (1989, pp.23-24).

assignment through the Russian commercial mission.<sup>73</sup> In addition to the pecuniary benefit the experience may even have been helpful when he made iron and other raw material markets and prices an object for study in Kiel a few years later.

By his own words Leontief was “very poor” in Berlin. He lived two years or so in Berlin under miserly conditions. He sublet a room, first in one flat and then in another. He remembered it as “living in a small cold-water flat” without a hot plate. His diet was correspondingly simple: “I didn’t have coffee, but I had milk. I made sour milk out of it” and digested it with potato pancakes which could be had very cheap, or bought a frankfurter which came with as much bread as one wanted.<sup>74</sup> He recalled an early purchase: “I still have in the country the first jacket I purchased myself in Germany, kind of a synthetic jacket, which looks very much kind of a Nazi green.”<sup>75</sup> An odd piece of memorabilia to keep!

The first task in Berlin was, naturally, to inquire about enrolment at the Berlin University.<sup>76</sup> Leontief soon found out that his Soviet gymnasium degree was not recognized as equivalent to the *Abitur* required for entry at German universities. There was only one way of addressing this obstacle, namely by sitting for the required supplementary entry exam (*Ergänzungsprüfung*). The university allowed those without *Ergänzungsprüfung* to be auditors (*Gasthörer*). Leontief was registered as *Gasthörer* on 27 April 1925 and retained the same status for three semesters. As *Gasthörer* Leontief could attend lectures but was not admitted to seminars.

Soon after arrival Leontief went to see Professor Werner Sombart to introduce himself and deliver the letter from Kulisher. Sombart welcomed him and made Leontief his research assistant. Leontief liked him despite their differences in scholarly interests.

Leontief passed eventually the *Ergänzungsprüfung* in October 1926 and was permitted to enroll as a matriculated student (*mit grosser Matrikel*) in the philosophical faculty.<sup>77</sup> Sombart entrusted Leontief with being the manager of his seminars, a responsibility which also comprised admitting new participants. The seminar comprised fifteen to twenty people. Leontief recalled having admitted a number of students to Sombart’s seminar and also the many interesting discussions having taken place there. The discussions were often

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<sup>73</sup> Fields (1986, p.70), Alpers (1989, p.25). In the early post-WWI years more Russian-language books were printed in Berlin than in either Petrograd or Moscow!

<sup>74</sup> Alpers (1989, p.23).

<sup>75</sup> Alpers (1989, p.19).

<sup>76</sup> Berlin University was then *Friedrich-Wilhelms-Universität zu Berlin*; it changed name to Humboldt University in 1949.

<sup>77</sup> Kaliadina and Pavlova (2006, p.340n18). The *Ergänzungsprüfung* comprised Latin. Leontief had picked up some Latin in Petrograd and didn’t seem to have much trouble passing the exams.

more on sociology than economics and Marxism was frequently brought up.<sup>78</sup> In retrospect Leontief remembered and summed up Sombart in the seminar as “tall, elegant... obviously originally influenced by Marx, but wrote a more or less Nazi book under the Nazis...quite eloquent and very interesting.”<sup>79</sup>

As a matriculated student Leontief worked mainly in the seminars of professors Sombart, Borkiewicz, and Breysig. There are no records from these seminars. The three professors were rather different in scholarly terms but seem to have shared the view that Leontief was an extraordinarily gifted candidate.

Werner Sombart (b.1863) had studied law and economics in Italy. His doctoral degree from Berlin University had been supervised by Gustav Schmoller and Adolph Wagner, the foremost members of the Younger Historical School. Sombart himself was denoted leader of the Youngest Historical School. Sombart was an economist and sociologist, knew Marxism well, had a radical image as a social reformer, and was very influential in social sciences in the first quarter of the twentieth century with a reputation far beyond Germany.<sup>80</sup> He was professor at Berlin University from 1917 to 1931 when he retired from his chair but continued to teach. Sombart was productive and wrote many books; very well-known was his multivolume work on modern capitalism, the third volume of which was published in 1928. Sombart was with Max Weber an active member of the *Verein für Sozialpolitik* which became the professional association for adherents of the historical school, and promoted large scale statistical studies of social and economic issues. Whether what Leontief learned from Sombart was useful on the scholarly path he pursued is less obvious. Leontief joined, perhaps at Sombart’s suggestion the *Verein für Sozialpolitik*, as his first professional association.

Leontief enjoyed the advantages of achieving an assistant position and a relatively close relation with Sombart:

“Possibly once I was in Sombart’s home. But there was, however, after the seminar, it’s typical German – you’ve visited Germany, you know – you go to a restaurant and drink beer and continue the discussion. And of course, I had the privilege because I could lead continuing discussions. It was Kempinski, later now is a hotel

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<sup>78</sup> Among those Leontief admitted was Hungarian born Thomas Balogh who became a British economist and politician, and eventually member of the House of Lords.

<sup>79</sup> Alpers (1989, pp.20-21).

<sup>80</sup> A well-known statement about him is Friedrich Engels’ assertion that Sombart was the only German professor who understood *Das Kapital*. He denoted himself as Marxist but had no qualms about embracing National Socialism in 1934.

in West Berlin. It was a gigantic restaurant. Everybody drinks beer. And of course there were political discussions and whatnot.”<sup>81</sup>

Ladislaus Bortkiewicz (b.1868) was born in St. Petersburg of Polish ancestry and spent his early life in Russia. He graduated in 1890 from the Faculty of law, St. Petersburg University and completed doctoral studies at Göttingen supervised by Lexis. He spent most of his professional life in Germany and was professor at Berlin University from 1901 until he died in 1931. He was famous as a statistician of the St. Petersburg school but was highly proficient in mathematics and actuarial science and familiar with all major economic theories with great expertise in the mathematical formulation of economic theories. His familiarity with Walras is of interest here, as also his analysis of the Marxian transformation problem.<sup>82</sup> At the founding of the Econometric Society in 1930 Bortkiewicz became one of eleven Council members but died prematurely at 62 years of age in July, 1931 shortly before the first meeting of the Society.

Bortkiewicz was obviously the teacher Leontief had most to learn from in Berlin. But details of what Leontief learned of statistics or economics from him are not recorded. Both Sombart and Bortkiewicz could teach Marxism, but surely in very different ways. Leontief would have preferred Bortkiewicz’ mathematical approach. In his analysis of the transformation problem in Marxian economics Bortkiewicz drew on earlier work by Vladimir K. Dmitriev, an outstanding pioneer of linear economics. But no trace has been found that Leontief ever heard of Dmitriev.

Kurt Breysig (b.1866) was historian with an orientation towards sociology and cultural anthropology. He studied in Berlin and became later professor (extraordinary) in 1896 until he retired in 1933. His recent books were e.g. *Entstehung des Gottesgedankens und der Heilbringer, Von Gegenwart und von Zukunft des deutschen Menschen* and *Die Macht des Gedankens in der Geschichte*. It seems unlikely that much influence from Breysig can be traced in Leontief’s work, but Breysig appreciated the bright and active student in his seminar..

Berlin gave opportunities to attend lecture by famous scholars who visited or lived in Berlin. Leontief recalled in high age that Keynes had visited Berlin, Leontief saw him but not close enough to shake his hand. He also went to lectures by famous scholars in other fields, such as Max Planck, the famous physicist. I went to his lectures. Also in Berlin Leontief spent much time in the library. Many years later Leontief came again to Berlin. It was then the capital of the German Democratic Republic; the university had become the Humboldt University, and Leontief’s theories had become known also in East Germany.

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<sup>81</sup> Alpers (1989, p.28).

<sup>82</sup> Gehrke and Kurz (2016).

During his visit Leontief was escorted to the library and shown a list of the books he had taken out during his study years about half a century earlier!

Before Leontief's departure his father had given him the address of Professor Karl Stählin in Berlin and wrote at the same time to Stählin about his son's plans. Stählin was happy for the opportunity of having a reunion with Leontief whom he had not seen since 1910 when Leontief was only five. Soon after Leontief's arrival Stählin invited him home for dinner with his wife and daughter. Leontief recalled it as a formal, German professor dinner and both pleasant and interesting, not least the lively discussion. Thomas Mann's latest book, "The Magic Mountain" (*Der Zauberberg*) had been published the previous year (1924) and on that it became a "big discussion." One might presume perhaps that what impressed Leontief was not the exchange between Mr. and Mrs. Stählin in front of a visitor, but rather the daughter discussing freely with her parents. Leontief's 1989 recollection comprised a vivid memory of the daughter.<sup>83</sup> Towards the end of Leontief's time in Berlin Karl Stählin went out of his way to help Leontief to get around administrative hindrances he met at the university, see below.

Leontief Sr. was in 1927 appointed Councillor at the Berlin office of the Soviet Ministry of Finance.<sup>84</sup> Leontief's parents settled in Berlin-Schöneberg, perhaps with no intension of returning to Leningrad. Relations with Germany were more important for the young Soviet state than with any other country for a host of reasons, not least financial. Leontief Sr. may have been helped by his old network of connections to get this appointment for which he was well qualified. The Leontief parents were naturally highly interested in seeing their son again but it seems surprising that it would happen via an official appointment by the Soviet government.

The Berlin office of the Soviet Ministry of Finance was part of the Soviet legation. The ambassador had since 1921 been Nikolai N. Krestinsky, a Bolshevik since 1903 who had been *Commissar* of Finance and held high-ranking political posts. After siding with Trotsky in the power struggle with Lenin since 1921 he had been demoted but found trustworthy enough to take on the important post of ambassador in Berlin.

There were, however, some means in the Leontief textile factory which had escaped confiscation. One of Leontief Sr.'s step sisters had at the end of the nineteenth century married a Russian born entrepreneur who had been naturalized in England and owned a machinery factory in Manchester. The Leontief textile factory had outstanding credit with this firm from advance payments on orders that were cancelled. To have these funds

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<sup>83</sup> Alpers (1989, pp.19-20).

<sup>84</sup> *Generalvertretung des Volkskommissariats für Finanzen im Auslande.*

returned to Russia would have run the risk of confiscation. Leontief's father was in a position to dispose of these financial assets after moving to Germany in 1927.<sup>85</sup>

Leontief's time in Berlin coincided with the best years of the Weimar republic. Berlin had a particularly lively cultural scene in 1925-1927. Leontief could hardly afford the time and money to enjoy very much of it. But he enjoyed the theatre and there were cheap tickets to be had. He also went to concerts, often they were concert rehearsals of the Berlin Philharmonic, conducted by the legendary Wilhelm Furtwängler.<sup>86</sup> Another memory was the Brecht/Weill *Dreigroschenoper*, which premiered in 1928.

After the three *Gasthörer* semesters Leontief reckoned at the end of 1926 that he needed only two semesters as matriculated student to complete his dissertation. He proceeded accordingly and completed the dissertation over the next two semesters. But the swift rush through the doctoral study met at first with disapproval by the university and skepticism in the Ministry supervising the universities. Leontief won through the administrative hindrances thanks to support from his doctoral advisors, Bortkiewicz and Sombart, but also from others. His doctoral dissertation was approved and the oral exam passed in the beginning of 1928 by which time Leontief had already worked at started to work at *Institute für Weltwirtschaft* in Kiel.

The administrative entanglements that needed to be resolved, are recounted in some detail below, as well as details of the evaluation of Leontief's dissertation, and his oral examination. But first a brief look at two articles published by Leontief during his Berlin years, we denote them by the key words in the original titles as the *Bilanz* article and the *Konzentration* article, respectively.

### **The *Bilanz* and *Konzentration* articles**

Leontief's first publication, in English translation titled *The balance of the economy of the USSR* was a review of a report by a team of Soviet statisticians led by the P.I. Popov on the Russian economy in 1923/24 (Leontief 1925).<sup>87</sup> Leontief alluded to Quesnay when he wrote of the report that it aimed at representing "in numbers not only the production but

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<sup>85</sup> This is another shrouded issue in the Leontief family history but may indicate that Leontief Sr. in the access to the funds had the support of his family in Leningrad.

<sup>86</sup> Wilhelm Fürtwangler was the principal conductor 1922-1945 and maintained within reason an anti-nazi stand until the end, while Herbert von Karajan with a different attitude was waiting eagerly in the wings to replace him.

<sup>87</sup> In the bibliography maintained at Leontief's Institute for Economic Analysis at New York University Leontief (1925) was on the top of the list until around 1990 when the translation of Schaefer's book on currency stabilization published 1923 dethroned it, see footnote 37. Two early book reviews written in Berlin never appeared on the bibliography, Leontief (1926, 1927b).

also the distribution of the social product, so as to obtain a general picture of the entire process of reproduction in the form of a *tableau économique*.”<sup>88</sup>

Leontief had left Russia for good but had not in any way surrendered his interest in the development of the Russian economy.<sup>89</sup> Leontief was very positive about Popov’s work which was a pioneering attempt at providing national accounts for the Russian economy. Leontief pointed out in his review that the adherence to the Marxian dogma of exclusively material accounting prevented a complete picture of the turnover of the economy.<sup>90</sup> He also noted other methodological shortcomings in Popov’s work and its presentation, related to the conception of production taking place in stages. He offered a brief numerical example to illuminate how to avoid double counting.

Leontief clearly found the Popov volume as very stimulating. He suggested that a similar accounting effort could be done for other countries and by following up the suggestions done in the review it could be done better for the Russian economy. Popov’s publication did not include references to data sources; Leontief listed in his review four categories of data he assumed had been used.<sup>91</sup> In the review of the Popov volume Leontief made the point that a proper methodological discussion required detailed documentation of sources and methods, as he meticulously adhered to himself in Leontief (1936, 1941).

The legend of the *Bilanz* article is that it was “lifted” from *Weltwirtschaftliches Archiv*, October 1925 issue, by the Soviet journal *Planovoe Khoziaistvo* where it appeared in December 1925, with correct name of author but with no indication of the original source. In 1964, nearly 40 years later, the article was issued in English in a volume of selected

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<sup>88</sup> Leontief (1977[1925], p.3). An explicit reference to Quesnay may not have occurred in Leontief’s work until Leontief (1936).

<sup>89</sup> In fact this was a research area in which he maintained a lifelong interest, although he published little. It was an interest shared with his father. Leontief was highly concerned with the Russian economy during WWII for OSS and after the war at Harvard’s prestigious Russian Center. He lectured at Harvard on the Russian economy, see e.g. <http://www.irwincollier.com/harvard-economy-of-russia-leontief-1947-48/>

<sup>90</sup> Adam Smith’s strict view on the distinction between productive and unproductive activity influenced via Marx the conventions of the Soviet national accounts which differed from those outside the Soviet sphere of influence. The schism lasted until the end of the Soviet Union. Smith’s authority was long-lasting and made it virtually impossible to discuss national income calculations internationally as late as the 1870s.

<sup>91</sup> They were (1) censuses; (2) current statistics; (3) budget statistics; and (4) state and trade organization, cooperatives, etc. Less than ten years later it would be Leontief who had to think of non-census sources helpful in the construction of the first input-output table for USA!



Soviet essays from the 1920s edited by N. Spulber , translated from *Planovoe Khoziaistvo* without reference to the original German version.<sup>92</sup>

There are, however, some residual doubts about this account. From the publication dates of German and Russian version there was hardly time to undertake translation and editorial processing. Secondly, in the article Leontief's domicile was stated as Leningrad, thus suggesting that the German article might have been submitted from Leningrad.<sup>93</sup> Although there is no evidence one way or the other, the Russian version may possibly have come not from a translation but instead from an original draft by Leontief in Russian. Leontief may inadvertently have glossed over the true fact.

The bibliographic confusion is not interesting by itself but it may have impacted on confusion in the wake of the article. Oskar Lange who came to USA in 1937 and spent time at Harvard with substantial contact with Leontief before he moved to the University of Chicago. He soon became well known for theoretical contributions and for his stance in some controversies.<sup>94</sup> Lange and Leontief knew each other also at the end of Lange's career in USA when he was Poland's ambassador and UN representative.<sup>95</sup> Lange (1963) cited the Russian version of Leontief's article and claimed that it contained "the first outline of the basic concepts of [input-output analysis]".<sup>96</sup> In the wake of Lange's assertions there were further articles by Soviet economists discussing whether input-output economics had been invented in the Soviet Union and usually citing Leontief's *Planovoe Khoziaistvo* article.<sup>97</sup>

There is, however, no foreshadowing of Leontief's input-output tables of the mid-1930s in the *Bilanz* article but there is an insightful discussion of the relation between gross

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<sup>92</sup> When the Spulber volume version was re-issued in Leontief's *Essays* in 1977 again no information was given about the German 1925 original.

<sup>93</sup> The *Weltwirtschaftliches Archiv* issue with the article stated Leontief's affiliation mysteriously as "Mitglied des Ökonomischen Forschungsinstituts in Moskau".

<sup>94</sup> Lange was naturalized in 1943 and "denaturalized" in 1945 when he became Polish ambassador to USA. Lange was acting editor of *Econometrica* 1943-1945, while Ragnar Frisch was isolated and incarcerated in Norway.

<sup>95</sup> Leontief (1938) on the significance of Marxian economics for modern economic theory was perhaps a delayed response to Lange (1935).

<sup>96</sup> Lange further asserted that Leontief (1925) was written "in connection with the discussion on the preparation of the first Soviet five-year plan. At the time Leontief was an employee in the State Economic Planning Commission of the USSR (Gosplan); he continued his work on balances of national economy in the United States." (Lange 1963, p.185, fn.39, based on the Polish edition of 1959). These assertions seem to be unfounded and therefore puzzling. See also Leontief (1960).

<sup>97</sup> The discussion continued to the end of the Soviet era, see e.g. Jasny (1962), Levine (1964), Spulber and Dadkah (1975), and Belykh (1989).

product and gross production, of double accounting, and other related topics of a kind that would have had to precede the development of empirical input-output tables.

In 1927 Leontief published the *Konzentration* paper in *Jahrbücher für Nationalökonomie und Statistik* (Leontief 1927a) titled *The theory and statistical description of concentration*. Like the *Bilanz* paper it took off from a Russian statistical publication but it was a theoretical paper, not a review. The opening paragraph is worth quoting because it stated the essential characteristics idea of Leontief's research method:

“Any purposeful statistical investigation of a phenomenon requires a special conceptual apparatus, a theory that will enable the investigator to select from among the numberless multitude of facts those that prospectively fit into some pattern and hence are susceptible to systematization. Even such a relatively simple event as a shift in population must first be placed within a rather complicated conceptual framework before it can be dealt with directly in a statistical investigation. The more complicated the object of inquiry, the more important its theoretical ‘preparation’, so to speak. Accordingly, we too are obliged to erect the requisite theoretical framework before we proceed to the purely statistical aspect of our topic.” (Leontief 1977, p.10).

Leontief stated in retrospect that his intention with the *Konzentration* paper had indeed been to include an empirical analysis but time ran out for him.<sup>98</sup> The research method expounded in the quote from the article as erecting a theoretical framework before proceeding to empirical study is a version of the approach of pre-eminence of theory, adhering by several of the early econometricians, e.g. Ragnar Frisch.

### **The *Wirtschaft als Kreislauf* dissertation<sup>99</sup>**

Leontief's main achievement in Berlin was the dissertation titled *Wirtschaft als Kreislauf*. Nothing much is known about how the idea for the dissertation originated, nor how much contact there was between Leontief and his supervisor Bortkiewicz in the process. Apparently, Sombart had been prepared to serve as dissertation adviser (*Doktorvater*) but at an early stage it became clear that even with a limited use of mathematics in the dissertation it would be beyond what Sombart was comfortable with. Sombart arranged for the *Doktorvater* responsibility to be taken over by Bortkiewicz but retained a formal responsibility.

There are indications that the ideas pursued the dissertation to some extent built on ideas Leontief had worked on in Russia, not least that one of the overall ideas had been rooted

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<sup>98</sup> Rosier (1985, p.80).

<sup>99</sup> All source documents related to the dissertation are from the archive of Berlin University, identified as *Bestand: Phil.Fak. 678*.

in Leontief's view of the economy as a one big, integrated unit ("I always viewed economy as a big unit."), a very broad general characterization embracing both the theoretical system of the dissertation, the input-output approach and much else.<sup>100</sup> His idea and plan for the dissertation may have been discussed with Bortkiewicz but the latter's evaluation of the completed thesis suggests that the submitted dissertation had not been read by Bortkiewicz before it was submitted.<sup>101</sup>

Towards the end of 1927 Leontief told Sombart he would soon be prepared to ask to be allowed to take the doctoral exam. Around the same time he sought Sombart's advice about possible positions he could apply for after completing the doctoral study. Leontief may have been aware of the *Institut für Weltwirtschaft* in Kiel as being a research institute of particular interest for him. As it turned out Sombart knew Bernhard Harms, director of the Kiel Institute, and his recommendation of Leontief to Harms resulting in that Leontief was offered a position at the Kiel Institute from January 1928. It was made clear that the starting date could not be postponed.

On 9 November 1927 Leontief wrote to the Dean Bieberbach of the philosophical faculty and requested to be allowed to take the doctoral exam in the winter term 1927/28. He summarized that he had by then three semesters as *Gasthörer* and two more after *Ergänzungsprüfung*, altogether five semesters on top of the seven he had studied in Leningrad. He added that it meant very much to him to be allowed to sit for the doctoral exam in the coming term as he had the offered from the Kiel Institute. His request was in full understanding with his venerated teachers, Professor Bortkiewicz and Sombart. The letter was returned after two days with a handwritten annotation by the Dean that the request had been denied, followed by an official letter of 12 November 1927 in which Dean Bieberbach regretted that Leontief's request to be allowed to take doctoral exam had been denied as Leontief had only studied five semesters, instead of the required minimum of six.

Professor Karl Stählin who had been fully informed by Leontief (or perhaps more likely by Leontief Sr.) that the request had been rejected, wrote to the Dean on 14 November, recommending Leontief warmly, arguing that he ought to be granted one semester for the seven or eight he had studied in Petrograd. Stählin also used the offer from Harms for what it was worth and referred to his close friendship with Leontief's parents. He appealed to the Dean: "If anything can be done in his favor in this matter, I should be extraordinary happy for this highly gifted and industrious young man."

Also Professor Kurt Breysig wrote in support of Leontief's request one week later and begged to the Dean to find a way help Leontief as Breysig's judgment – as that of Sombart

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<sup>100</sup> Alpers (1989, p.21).

<sup>101</sup> The title of the dissertation may seem to have close similarity with that of chapter one in Schumpeter's major theoretical work (Schumpeter 1926), but *Wirtschaft als Kreislauf* and *Kreislauf der Wirtschaft* are expressions with quite different meanings.

– was that Leontief was an extraordinarily capable person. Also Breysig mentioned the Russian exam and the offer from Harms as reasons for granting the request. Breysig even added that with the delay imposed on Leontief by denying the request put at risk not only the post in Kiel but in part also his future. Dean Bieberbach reacted to these appeals and, indeed quickly, by forwarding Leontief’s request to the Ministry on 3 December and ask for it to be granted.

As the deadline for registration for the doctoral exam expired on 10 December Leontief could not afford to wait for the Ministry decision as that might take a month or so. He decided to submit his formal request to be allowed admission to the *Doktorprüfung* by the deadline. In the letter of 9 December he asked to be examined in economics as major and in history and philosophy as minors, enclosing the dissertation *Wirtschaft als Kreislauf*, and a brief *cv* (*Lebenslauf*).

Leontief’s dissertation which was published in a well-known German economic journal. In our view the dissertation was of little importance in determining the specific route along which Leontief arrived at the input-output formulation, without denying its significance in the formation of Leontief’s overall conception of economic science. Insightful comments on the dissertation can be found e.g. in Lager (2009) and Kurtz and Salvadori (1995, 2006).

The Ministry responded to the Dean on 14 December that it was not inclined to approve Leontief’s request, pointing out that Leningrad studies could not be considered equivalent to German university studies and hence could not be counted. Hence, in order to avoid establishing a precedent the Ministry requested from the Faculty as soon as possible its opinion as to whether the accomplishments of the applicant (i.e. Leontief) were so superior that his admission to the doctoral exam would not permit doubts. The Ministry also asked for the evaluation reports of Leontief’s dissertation. Dean Bieberbach accordingly requested statements about Leontief as well as evaluation of the dissertation from Sombart and Bortkiewicz, while Leontief prepared to leave for before New Year.

Bortkiewicz wrote to the Dean that he unconditionally supported Leontief’s request for admission to the exams, even though he had some critical observations on the dissertation: “The candidate entered upon the path of his constructions (in my opinion contestable as a matter of principle) without being pointed there by any of his academic teachers (including the Petersburg economist Solntzeff, whom I know well as he had attended this university as auditor 20 years ago), but rather reached them fully independently, even in defiance to them. ... In all probability he will also in the future adhere to the same scientific positions. His dissertation should therefore in no way be considered cause to doubt his maturity for the doctoral promotion.”<sup>102</sup> He enclosed the five-page evaluation of the dissertation.

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<sup>102</sup> Bortkiewicz to Dean Bieberbach, 11 January 1928, see Wittich (2010).

Sombart sent a separate letter to Dean Bieberbach in which he stated that he agreed essentially with the judgement of Borkiewicz but “would perhaps to a higher degree than him consider the formulation of the problem dealt with as misconceived. But I also reach the conclusion that we are dealing with an extraordinary accomplishment of a very bright (*scharfsinnig*) head, and above all I am of the opinion, on the basis of my acquaintance with the author, that there can be no doubt of the maturity of the applicant to undergo the doctoral examination.”<sup>103</sup>

The philosophical faculty wrote anew to the Ministry on 21 January 1928 submitting the evaluation of the dissertation and requesting the admission of Leontief to the doctoral exam, on the basis of his special scientific qualifications. It was also emphasized that he had published two scientific papers during his studies in Berlin. Two weeks later the Ministry approved the admission of Leontief to the doctoral exam. Leontief was informed by Dean Bieberbach who subsequently on 10 February 1928 approved the request for the doctoral exam.

The evaluation of the dissertation had been written by Borkiewicz and was as follows:<sup>104</sup>

“The first part of the dissertation has more the character of a confession than an argument, as the candidate himself seems to admit to judge from the concluding sentence of that part. One misses particularly a scrutiny of the question whether the social sciences (*Sozialwissenschaft*) have overreached themselves and entered alien territory by moving to such most general insights which would deserve to be called laws. In other words the candidate does not at all touch upon the problem of such specifically social principles.

His critique of “teleological theories” is unconvincing simply because he lumps together so differently oriented thinkers as Stammler, Rickert and Sergius Hessen, the latter being a student of Rickert, but with largely independent views. The attempt to show that, contrary to *communis opinio doctorum*, the individual was in general not harmed, depends on some kind of misunderstanding.

The candidate opposes the common assertion of the relativity of social laws by a far too convenient argument. When he in this connection, of all things presents Gresham’s Law as an example of a social law in a strict sense, his position is not strengthened. Because, even when disregarding that the strictness in this case is rather problematic, this “Law” stems from the pre-historic period of economic science, in fact, as Gide on some occasions has pointed out, from the Periclean age.

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<sup>103</sup> Sombart to Dean Bieberbach, 12 January 1928, see Wittich (2010).

<sup>104</sup> The evaluation is on file at Humboldt University. It is rendered here, slightly edited, transl. by ob.

Although the candidate in his remarks has not contributed much towards clarifying the issue that the first part of dissertation is dedicated to, it is nevertheless certain that he seriously has struggled with this difficult topic and reached his own position on it. Otherwise, the first part does not need to be part of the printed version, especially as it is only loosely connected to the main object of the dissertation.”

Apparently, there was not much Bortkiewicz found meritorious in the first part of the dissertation. Although the original draft has been lost, it seems that Bortkiewicz’s advice of leaving out the introductory part was heeded.

“There is an intention behind the candidate’s title for the second part of the dissertation, rather than “The economic circulation” (*Der wirtschaftlichen Kreislauf*), he has called it “The economy as circular flow” (*Die Wirtschaft als Kreislauf*). The aim was to express that the “circulation concept” (*Kreislaufvorstellung*) is the only adequate one for the nature of the economy. (...)

The stated postulate of a “cyclic” point of view, as one might call it, is by Leontief elevated to become the constituting principle of economics (*Nationalökonomie*) as a subject]. Phenomena which do not have the kindness to fit into this point of view of view are for him excluded from the realm of economics. He shows no modesty in opposing his “circulation principle” (*Kreislaufprinzip*) to the “concept of economic man” and letting the latter virtually be supplanted by the former. Then he dissolves people in individual activities (“*Dienste*”) and perceptions (“*Empfindungen*”), which with him ranks as elements of the economic processes along with material goods and do not differ principally from these.

In his fundamental scheme of economic circulation according to pure physical relationships between costs and benefits, Leontief lets these essentially quite distinct elements combine with each other and mutually dissolve. On the basis of this construction production and consumption are identified in a consequent and conscious way. This sounds like well-known trains of thought, such as the notion of reproduction of the commodity of labor power through the consumption of means of subsistence by the laborer. Also more recent theoreticians, the Marxists (e.g. Karl Renner) need hardly be mentioned, have tried to let the processes of consumption and production merge into one flow (e.g. Seligman in his “Economics of instalment selling”). But so far hardly anyone who has held the view in question here has pursued it as seriously as the candidate. He in particular rejects any distinction between so-called productive and non-productive consumption and heaps irony on those of a different opinion, of course without showing how one can transform a ball dress back into the required production goods and work effort

needed to produce from the viewpoint of the causal relation between cost and benefit. According to ... those who reject any sharp distinction between production and consumption, the alternative is to insert money links (*Geldgliedern*) in the relevant place in the “chain”. This alternative is however unacceptable for the candidate, as he in his fundamental scheme allows neither money nor barter. After all, this scheme should apply to any economy, even the isolated economy, to the same extent. Barter trade and money circulation appear only later as complications of the plan, and have even been dealt with in an excursus.

It mustn't surprise that a consideration of the economic circulation abstracted from any social conditions, yes, from any relations between economic agents and from the motives of economic transactions also reveals itself as fruitless, when it is about to investigate how certain changes of some or other data will impact the problem. In this way the candidate e.g. deals with the question of what consequences it will have when a(n) (unintended) productivity increase occurs at any point in the circuit. For simplicity it is assumed that there are only three elements in play. These are produced in inalterable quantities in each period, and in such a way that for production of each of the three goods the other are required in fixed proportions. For the case now that the yield of the first good increases by a certain percentage, it is shown that because this excess must be find usage in the production of the other two goods, in the next period correspondingly more of these goods, and therefore less of the first good will be produced, which again leads to that in the following period, the yield of the first good increases while that of the other two decreases, and that such a “pendular movement” continues until in the end a further use of the good with increase productivity becomes impossible.

As the candidate remarks, this impossibility would happen the earlier the stronger the productivity increase would have been. Yes, it can easily be shown that in his numerical example a doubling of the productivity of the first good will already at the end of the following period lead to a standstill of the entire production process. This would mean that when an economic resource starts to flow in sufficient magnitude it must lead to paralysis of the economy. The abstrusity of such a conception is close at hand. It is obviously a flight of fancy and the Leontievan “pendular movement” is no less so: it is admittedly correctly derived logically and arithmetically, however, under assumption that strikes reality in the face: in the derivation it is assumed that the economy rather collapses than leaving a good unutilized, and that the productivity coefficients are under all circumstances maintained.

Unrealistic is also the treatment the candidate offers of the problem of exchange. His criticism of Irving Fisher's contraposition of capital and income is largely misguided. His “third” concept of the velocity of money is untenable. In

general it must be stated that his remarks are in part incomprehensible. The notation he makes use of is sometimes unusual and inappropriate.”

Bortkiewicz described and commented the content of the dissertation with a slight ironic distance. He was hardly been convinced about the benefits of the system developed by Leontief. He acknowledged the effort and the ingenuity demonstrated by Leontief in the concluding paragraph.

Although the second part of the work may have some sensitive formal shortcomings and materially little to offer as the results of the theoretical investigation at best stand in a remote relation to reality, but as a specimen of scientific aptitude and the erudition of the author this part must not be underestimated. He proceeds throughout in correct style, mostly correct reasoning and always independent, although not without strange ideas whether in an explicit position, or whether in mere suggestions to take into account.

In my assessment the dissertation deserves to be marked as *opus laudabile*.”  
10 January 1928                      signed L. v. Bortkiewicz

Bortkiewicz’ evaluation and marking was cosigned by Sombart with a single minimalistic remark that he accepted the mark (“*Mit dem Prädikat einverstanden*”).

At this stage only the oral exam (*Promotionsprüfung*) remained. It was decided to take place on 1 March 1928. The *Promotionsprüfung* was a thorough test, conducted as an oral exam over one day on 1 March 1928 with four examiners, three of them his teachers Bortkiewicz, Sombart, and Breysig, The fourth examiner was Köhler, a philosopher. All four examiners wrote their separate evaluations of the candidate, bases on their respective oral examinations on the same form. The evaluations are, unfortunately, were found hard to decipher and are therefore highly incompletely rendered below:<sup>105</sup>

Breysig (history): “Mr. Leontieff displayed in response to my questions mainly concerning the philosophy of history very well reasoned thinking and the substantive knowledge [*Kenntnisse*] ... of difficult subjects for which he is known to me. As he answered all questions in a perfect manner, I attribute the predicate: *summa cum laude*”.

Köhler (philosophy): “Apriorism in the works of Cassirer. The notions of substance and function – classical definition, implicit definition; Theories of ???. Theory of induction. Modus ??? (illegible) in the works of Wertheimer; ???istik and determination in ???; Recognition as classification [*Erkennen als Zuordnen*]. Rating: *Good to Very good*.

Sombart (economics): “Subjects: Methods of establishing facts: Statistics: induction, data-gathering methods of German statistics, profession and trade censuses, size classes in

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<sup>105</sup> Based on Wittich (2010).



the trades and in agriculture. The structure of German agriculture – its history – village and estate [*Gut*] economies. The ‘agrarian reform’ of 1807ff. Domestic [*innere*] colonization. Legislation {?} as K... {?} in agriculture. Regions {Landschaften}. The origin of mortgage deeds. The combination of the mortgage deed with ... {?}: credit foncier. Structure of a mortgage bank – is it a bank? Since I am acquainted with the theoretical gifts [*Begabung*] of the candidate, I questioned him exclusively about facts [*Tatsachen*]. In this respect his knowledge [*Kenntnisse*] proved ultimately to be still rather limited [*begrenzt?*]. Yet, after all, I can assign him the rating of: *satisfactory*.”

Bortkiewicz (economics): “Distinctions between abstract and concrete value. ‘Senior’s last hour’ (Marx). Dependence of the profit rate on production relations in such branches of production which do not determine the wages of labour (contrast between Ricardo and Marx). Stock markets, especially dated transactions [*Termingeschäfte*] and their impact on the economy. Formal theories of population. The second thesis of Malthus and cultural progress. Engel’s law and the theory of marginal utility. – Similar to colleague Sombart, I gained the impression that the positive informations [*Kenntnisse*, plural] of the candidate are not quite up to the required (?) level [*nicht ganz auf der Höhe sind*]. Rapid thinking [*Auffassungsgabe*] and giftedness [*Begabung*] remain. Result: *Good*.”

Sombart (concluding): “As an overall evaluation I would consider a rating of *cum laude* to be sufficient.”

After the *Promotionsprüfung* Leontief hurried back to Kiel. Only the promotion ceremony and the awarding of the diploma remained. It should normally have taken place in September 1928. It was required that the dissertation was printed and made available in a sufficient number of offprints, at the time set at 150 copies. Leontief had submitted to *Archiv für Sozialwissenschaft und Sozialpolitik*, which accepted it. But the printing process was delayed. In September 1928 Leontief wrote to the Dean of the Philosophical Faculty to ask for extension of the deadline as the dissertation would not appear in print until 1 December 1928.

The Philosophical Faculty granted the extension. But two months later, in November 1928, Leontief was forced to ask for an additional postponement of one month until 1 January 1929. The dissertation had been published as Leontief (1928) but the required number of offprints could not be provided by the publisher until the latter date. The Faculty accepted, exceptionally, to proceed with Leontief’s promotion on 19 December 1928 even though printed copies might not arrive at the Dean’s office until 1 January 1929.

At the end of 1928 Leontief had worked for one year at the Kiel Institute. He became pressured for time for reasons which will be apparent below and decided at a rather late stage not to go to Berlin for the *Promotion*. He sent on 18 December 1928 a telegram to the Faculty: “Please proceed with promotion in absence – I am prevented from coming. Leontief.” Adolph Löwe, Leontief’s head of department, corroborated: “...for urgent

reasons of completion of a scientific research project he cannot be given leave until the end of the year.”

The Philosophical Faculty accepted the “urgent reasons” and sent the diploma in the mail after Leontief had submitted in writing the solemn vow of *Promotion*: “I engage myself to guard the honor the Faculty today awards me from any blemish and henceforth ... to pursue truth alone, without respect for any external concerns. Kiel, 24. Jan. 1929, sgn. Wassily Leontief”.

## The Kiel years 1928-1931

Bernhard Harms had been director of the *Institut für Seeverkehr und Weltwirtschaft* in Kiel since it was founded in 1914.<sup>106</sup> The Kiel Institute was affiliated with Kiel University but not part of it. Its mission was to study the world economy. At the time it may have been the only institute in the world with this overall specialization. The Kiel Institute early achieved a reputation for its research in international economics and had attracted a number of highly qualified scholars, some with interests in other matters than sea transport and the world economy. The Institute published *Weltwirtschaftliches Archiv*, which had become a leading German language economics journal, highly regarded internationally. The journal served as a conduit for promoting research conducted at the Institute. Bernhard Harms had established a research library which at the time when Leontief arrived reputedly was the world’s largest economics library.

Kiel was the capital of Schleswig-Holstein, a small city at the Western end of the Baltic, diametrically opposite St. Petersburg. Schleswig-Holstein had been under Danish rule for centuries when it was annexed by Prussia in the 1860s after war with Denmark. Prussia moved its naval headquarter from Danzig to Kiel. The Kiel Institute moved in 1920 to a prominent seafront building, which once had been the *Seebadeanstalt* owned by the Krupp family.

As arranged between Werner Sombart and Bernhard Harms Leontief arrived at the beginning of 1928 to work as scientific staff in the Kiel Institute’s recently established department for statistical economics and business cycle research directed by Adolf Löwe; it was nicknamed *Astwik* from the acronym of the German name.<sup>107</sup> Business cycle institutes emerged in many countries after World War I, in the wake of the business cycle forecasting service established around Warren Persons at Harvard University from 1917. The *Institut für Konjunkturforschung* in Berlin was founded in 1925, and the *Astwik* department was established in 1926. In the Kiel Institute lore this had happened after Adolf Löwe had

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<sup>106</sup> *Seeverkehr* was dropped from the Institute’s name in 1934. Today the official name is the Kiel Institute of World Economy.

<sup>107</sup> Abteilung für statistische Weltwirtschaftskunde und internationale Konjunkturforschung.

visited the Institute and met with Harms who offered him a professorship if he would take charge of a new department for studying international business cycles! Among other staff of the department were Gerhard Colm and Hans Neisser who both joined in 1927. Later arrived Jakob Marschak, Alfred Kähler and Fritz Burchardt, the latter shortly after Leontief had arrived in 1928. Leontief with his broad background from Petrograd and Berlin may have appreciated to find at the Kiel Institute in addition to the bright Astwik economists the sociologist Ferdinand Tönnies.<sup>108</sup>

Leontief was the Benjamin in the *Astwik* group of scholars, roughly ten years younger than his above-mentioned colleagues. His title was *Assistant* and in the hierarchical order of the Institute Leontief was assigned to Colm as his superior.

Löwe and the *Astwik* group at the Kiel Institute were more theoretically oriented and more diverse than the Harvard group of business cycle forecasters around Warren Persons. They are referred to in German as the *Kieler Schule der Wachstums- und Konjunkturtheorie*.<sup>109</sup> In Kiel Leontief was for the first time in a research institution more permeated by modern economics than he had met with in his study years. In this setting he became increasingly familiar with “demand analysis”, “business cycle theories”, and the somewhat ambiguous concept of “dynamic economics.” Increasingly he also came to consider himself as a mathematical economist.

Leontief was employed in two separate sub-periods at the Kiel Institute. The first period was through 1928 during which Leontief developed his innovative theoretical framework for applied demand and supply analysis, published as *Ein Versuch zur statistischen Analyse von Angebot und Nachfrage* (Leontief 1929), which got distributed widely.

Leontief thrived in the research environment and the intellectual company which the Kiel Institute offered. Perhaps he found Kiel small and provincial. He would surely have appreciated the glimpse of the Baltic he could see every day only a few meters from the institute building. Kiel allowed him to enjoy favorite pastimes such as sailing, tennis, and hiking.

At the end of 1928 Leontief interrupted his employment with the Kiel Institute to take up an assignment in China. He came back to Germany one year later and contracted with the Kiel Institute for a new employment period of one year from April 1930. In this period Leontief worked on a follow-up study of Leontief (1929), which went under the nickname of the *Eisenaufsatz* (“Iron study”) and was eventually published as Leontief (1932). It was the second of two papers Leontief published with the Kiel Institute.

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<sup>108</sup> Ferdinand Tönnies (b.1855) had been banned from a professorship by the Prussian government in late 19th century and stayed on long enough in Kiel to be ousted by the Nazis in 1933.

<sup>109</sup> Hagemann and Kurz (1984).

Germany changed while Leontief was in China. The political stability of the Weimar Republic had started to crumble, the financial situation in Germany was on the verge of a crisis, and the onset of depression was near. The mood of Germany had changed. In retrospect he had vivid memories of the street propaganda of the Nazis, the proprietor of one of the houses in which he had rented a room had been a Hitler sympathizer. After his return from China he found that, “Life conditions were hard and Hitler promised a paradise.”<sup>110</sup>

Towards the end of the second employment period Leontief found that the prospects for further employment at the Kiel Institute had eroded, with gloomy prospects also elsewhere in Germany. Leontief had not in any way given up a career in Germany when he was lucky enough to receive advice that was helpful towards winning a competition for a research associate fellowship for 1931/32 with the National Bureau of Economic Research in New York.

### **The *Versuch* paradigm: Leontief’s simultaneous demand and supply analysis**

Demand analysis was an emerging field in the wake of the pioneering work of Henry Moore initiated at the beginning of the century. Moore was a rare case of a mathematical economist in USA. His Columbia student Henry Schultz continued in Moore’s tradition at the University of Chicago after World War I. Schultz published in German and English and became in the 1920s a leading name in the field. His works were studied and influenced researchers in several countries.

It was only natural for *Astwik* to engage in this field. Perhaps Leontief was encouraged to look into demand analysis or suggested it on his own. Leontief approached the topic from his acquired view on how economic research ought to be conducted. It would hardly have satisfied Leontief to conduct singular demand studies of various markets like Schultz did. He was not that kind of empirical researcher. His empiricism was to develop an appropriate theoretical framework that could be applied to suitable economic statistics with the purpose of extracting structural information. Leontief’s theoretical framework for analyzing supply and demand of commodity markets as part of the same investigation was based on Marshallian static partial equilibrium and the use of elasticities to characterize supply and demand curve of markets. He developed statistical procedures for estimating supply and demand elasticities. Leontief’s plan encompassed the ambitious but far from unproblematic idea of simultaneous determination of the supply and demand schedules for a given market from a single set of data.

Leontief’s ambitions thus went beyond those of Moore and Schultz. Leontief’s parsimoniously formulated framework considered supply and demand schedules as the key

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<sup>110</sup> Rosier (1986, p.81, transl. by ob).

structural characteristic of constant elasticities. He divided the changes affecting supply and demand schedules into two kinds: (1) structural changes, i.e. changes in the elasticity of supply and demand, which graphically would be represented as changes in the slope; and (2) shifts, i.e. all other changes in demand and supply schedules.

Moore, Schultz and those who followed in their tracks used a battery of methods for eliminating trends before estimating demand and supply relations. The estimated schedules would often be of a hybrid character. Sometimes it was entirely clear whether a demand or supply curve had been estimated.

Leontief wanted to bring this kind of analysis to a higher and more systematic level and with applications to a wider range of markets than the agricultural commodities and consumption goods studied by his predecessors in the field. He wanted to develop tools of analysis suitable for the industrial economy.

Under Leontief's stylized assumptions the elasticity was thus assumed constant throughout the whole length of the respective supply and demand schedules. In double logarithmic transformation the demand and supply schedules would then be depicted as straight lines. Each observation (market point) would be an intersection of a pair of straight lines representing supply and demand schedules. Both the demand and the supply schedule would shift. The demand schedule was considered as shifting along the supply schedule and vice versa. Leontief stated and argued on theoretical grounds for the key assumption that the supply and demand shifts were independent. To be able to determine the elasticities of demand and supply in a setting that in later terminology would be denoted as "underidentified" Leontief had introduced a congenial and crucial device, namely that the data material was divided into two parts on the assumption that the elasticities of supply and demand were the same in both parts, while the distributions of the observations would be sufficiently different to allow identification. Leontief had enlisted the help of a mathematician of Kiel University, Robert Schmidt, to work on the formulae to be used in the estimation. Schmidt wrote an appendix to Leontief (1929) to set out the mathematical derivation of the key formulae and later also published a separate paper, Schmidt (1930).

Leontief's method would then supposedly determine the two structurally invariant elasticities but also the shifts of the supply and demand curves. From this information various parameters could be calculated such as the average elasticity over a certain period, etc. Leontief (1929) comprised a number of examples to demonstrate the feasibility of the method and acknowledged that the method would only work under conditions that were not always fulfilled.

No other paper made Leontief's name internationally known so widely and quickly as this paper. This was not least due to the efforts of Bernhard Harms, the publisher of *Weltwirtschaftliches Archiv*, in distributing offprints together with a personal letter to a

large number of scholars in economics and statistics in several countries. The article itself had an initial *Vorbemerkung* by Bernhard Harms, as follows:

“The current dissertation comprises an attempt at developing a new method for statistical market analysis. A continuation of the statistical-methodological work and the market analyses conducted by means of this method is intended. The publication of this treatise, which still has a number of open problems, is due to our wish to make the statistical method available for scientific discussion.” (Leontief 1929, p.1\*, transl. by ob).

There is a discernible element of publisher’s pride in the programmatic announcement of a “new method” but also a guarded allusion to “a number of open problems,” perhaps a cautious admonition that had come from Leontief?

After completing the 50-page article, submitting it for approval by the Institute, and undertaking the first round of proof reading it, Leontief left Kiel abruptly on a ship bound for Shanghai months before the article was published. The further proofreading was taken care of by one of his *Astwik* colleagues, Gerhard Meyer, who served as the relay between Leontief and the Institute during his absence.

## The Chinese interlude

In 1929 there was thus an odd interlude in Leontief’s career as he interrupted his work at the Kiel Institute to spend most of a year in China. It is merely a parenthesis in Leontief’s scholarly career. But it was a detour he obviously appreciated, after being a hardworking student for so many years. It gave him a glimpse of the orient, and, more important, an opportunity to practice as an economic advisor in the field. Many of Leontief’s students and friends would have heard him tell about his Chinese adventure.

As Leontief recounted the story of how it came about, it started with an accidental meeting in Kiel in 1928. In a restaurant with colleagues Leontief got involved in conversation with a group of Chinese at a neighboring table. The group turned out to be a Kuomintang government delegation sent to Europe to find experts who could help with them with development plans. After the initial contact further meetings between Leontief and Chinese government representatives followed. Soon after a telegram from the Chinese Minister of Railroads in Nanjing arrived, inviting Leontief to come to China for a year as an advisor on economic issues, foremost on planning new railway lines. Leontief jumped at the chance. This happened just after the split between Kuomintang and the Communist Party of China. Leontief believed the Minister of Railways was a son of Sun Yat-Sen who had chosen to remain with Chiang Kai-Chek.

Leontief left from Marseille on a ship that took him through the Suez Canal and all the way to Shanghai. It was a slow boat to China and Leontief took the opportunities offered to get ashore for brief glimpses of the countries along the route. It was his first chance to

see any part of the world outside Europe, and as he reminisced in an interview: “It was the first time I had a chance to see underdeveloped countries.”<sup>111</sup> The first impressions were from Egypt and the Arab peninsula.

Leontief’s assignment was directed from Nanjing, the seat of Kuomintang government. The Ministry of Railroads was planning a new railroad system and Leontief got involved with a range of economic and technical problems connected with the plans. Leontief spent much of the time between field work sessions in Shanghai where he had an apartment in the Burlington House Hotel. The language used by Leontief in China was English. He had learned English in school but his fluency level was rudimentary in comparison with his German and French. The mission to China was really his first opportunity to use English.<sup>112</sup>

Leontief elaborated on occasions of how he and the team he worked with used small aircraft to survey the agricultural use of land and population settlements to use as information for planning purposes.

Gerhard Meyer kept in touch with Leontief during the China trip, as a relay between Leontief and the Institute, communicating smaller and bigger events of the Institute, including office gossip while Leontief was away.<sup>113</sup> Letters took about one month; hence communication was not very expedient. Leontief sent some photos of his experiences in the field. Letters in both directions were often relayed via Leontief Sr. in Berlin.

As soon as the July 1929 issue of *Weltwirtschaftliches Archiv* with Leontief’s supply and demand study had been published some copies were sent to Leontief in Shanghai together with a fairly long list of names of economists to whom offprints had been sent. As the journal and the offprints were distributed around the world letters of the acknowledgement were returned in a steady flow to Harms, some of them addressed to Leontief. It was an immediate and largely positive response, although superficial. Proper comments and journal reviews took longer time.

One of the first scholarly comments came from Arthur Bowley at London School of Economics and arrived early, already at the end of July 1929. Bowley wrote that he was “greatly interested in Dr. Leontief’s brilliant analysis” and had reworked the mathematics from another point of view and arrived at a resulting equation he claimed to be simpler but equivalent to that of Leontief. He had prepared a note about his analysis. As Bowley was

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<sup>111</sup> Rosier (1986, p.80, transl. by ob).

<sup>112</sup> Ellsberg (1952).

<sup>113</sup> Gerhard Meyer left Germany in 1933 and arrived eventually in USA where he got a teaching position in Chicago. He is known to have been very close to Hyman Minsky in Chicago and exerted pivotal influence on Minsky to pursue graduate studies in economics, see Jones (2001, p.1024).

going on vacation somewhere in Holstein, he suggested a meeting at the Kiel Institute to discuss the issues.<sup>114</sup>

Bowley came to the Institute on Monday 19 August 1929. Colm was not present as he was vacationing in Switzerland. In a letter on the following day Meyer gave Colm an account of the proceedings with copy sent to Leontief in Shanghai. Meyer's impression was that Bowley had substantial points to make but would rather have enjoyed his vacation than having a drawn-out confrontation with the Institute staff when Leontief was not present.

The discussion was conducted in English and it was mostly left to Robert Schmidt to speak for the Institute. Schmidt was (partly) responsible for the mathematics that Bowley had objected to and had to admit that Bowley's mathematical reasoning achieved to a great extent what Schmidt had tried to do through more complicated and less transparent reasoning.

A key point in Leontief's paper was the splitting the time series into two periods. Bowley argued that splitting the material into two equal periods had no advantage over practically any other way of splitting the material; any way of splitting it was equally useful. He even argued that it could be just as useful to ignore the time dimension altogether. Schmidt insisted against Bowley that splitting the data in two equal sub-periods had advantages over all other alternatives and elaborated on his criterion of *Prägnanzmass* developed for this problem but not yet completely worked through, see Schmidt 1930.

In the evening there was a social gathering at Löwe's. During the evening Bowley had said that Keynes didn't understand mathematics. Meyer passed this on to Colm with the remark that Roberts Schmidt had said pretty much the same about Bowley's mathematics.<sup>115</sup> But this was an unfair remark as Bowley, unlike Schmidt, was up to the task.

Meyer also mentioned to Colm that George F. Warren, an agricultural economist at Cornell, had thanked for the "distinct contribution to the theory of elasticity" and added a somewhat more ambiguous, or even spiteful, compliment that he was happy to note that Germany was catching up on the mistakes of recent years. W.L. Crum, the current editor of Harvard's *Review of Economic Statistics*, answered that a study of supply and demand curves was in progress at Harvard and "we shall be greatly interested in this particular work which you have brought our attention." Crum was the chairman of Harvard University's Committee on Research in the Social Sciences which had on its small staff Elizabeth Waterman Gilboy who soon would become the secretary of the Committee. Gilboy and the Committee would come to play an important role in supporting Leontief's

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<sup>114</sup> Bowley to Harms, 20 July 1929.

<sup>115</sup> Meyer to Colm, 20 August 1929.



research after his arrival at Harvard. She took an early interest in Leontief's work and later wrote a comparative review of the methods used by Henry Schultz and Leontief (Gilboy 1931). The methods used for analyzing economic data were often inadequate at the time. In fact this was among the reasons for Ragnar Frisch and other European economists and statisticians to join forces with Irving Fisher and others on the other side of the Atlantic to establish the Econometric Society.

Little is known about Leontief's adventure in China, although his memory of it was very vivid. Documentary records about Leontief's activities in China have not been found.

In the autumn of 1929 Leontief wrote a letter to his colleagues addressed to the coffee shop in Kiel they usually frequented. In somewhat cryptic terms he commented upon pictures received and enclosed:<sup>116</sup>

“Gentlemen,

With satisfaction I see that our main business blooms and prospers, in spite of the apparently not yet quite overcome burden of writing and calculation work. From the pictures enclosed I conclude that the summer business developed altogether satisfactorily.

Our local branch enterprise suffered under unfavorable summer conditions, but the winter promises good employment/activities.

In the photos enclosed you see me on a delicately configured gazelle chasing after my work. The other photo shows the environment in which my activity proceeds (compare it with a normal man who stands alongside!). The last photo shows that in spite of all I remain true to our proven business principles.”

With faithful respect,

W. Leontief 4 Oct. 1929

Leontief returned from Shanghai along the same route to Marseille as on his outward voyage, enjoying a second opportunity to have brief glimpses of countries he didn't know. He returned to Kiel but then left for Berlin where he stayed with his parents in Berlin-Schöneberg for two months or so.

The Chinese adventure had been a very interesting and pleasant experience. He had worked hard in China but also been rewarded well in pecuniary terms, much better than at the Kiel Institute. His first post-war visit to China in 1973 revived the memory of the adventure he had experienced there. Leontief found that railroad lines had indeed been built according to plans he had helped to prepare in 1929!

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<sup>116</sup> Based on draft in Leontief Archive, translated by Claus Wittich.

## In Kiel again

It seems unlikely that Leontief at this time nurtured any idea of leaving Germany. He had chosen Germany as a place to live and work and had high admiration for German science and culture. He was always curious about the world and would surely have confirmed interest in visiting USA, and England perhaps even more, and hoped he would get a chance to do it. But Germany was his chosen country. He had two important concerns at this time, namely his future career in Germany, and his continued research after Leontief (1929).

For a university career in German the doctoral exam was of limited value without the *Habilitation*, required to qualify for a professorial chair at a German university. Sometimes referred to as a second doctoral degree, it was conferred on the basis of a scientific thesis (dissertation) with a level of scholarship higher than that required for the doctoral degree, and conducted independently, i.e. not guided by a supervisor. The habilitation could be done at any university and the thesis submitted had to be defended before an academic committee and approved by the entire faculty. To be appointed to a chair after habilitation often implied waiting in the wings for years as *Privatdozent*, a title conferred by a university and implying permission to teach and supervise doctoral students at the conferring university without holding a professorial chair. As *Privatdozent* was not necessarily a salaried appointment, the holder might have to seek his source of income elsewhere.

At his return from China Leontief was hardly in any doubt that he in his further research wanted to build on his *Versuch* treatise and extend and enhance the demand and supply studies on the basis of the experience he had gained and new ideas he had considered as worth looking into. He was not tied to the Kiel Institute; he could take his research with him to another institution and hope to find scholars with similar interests. He may have considered only one other research institution of great potential interest for himself, namely the *Institut für Konjunkturforschung*, directed by Ernst Wasserman. He knew quite a bit much about the *Konjunkturstitut* and had contacts there. Perhaps he also preferred Berlin over small and sleepy Kiel.

Leontief appreciated the positive feedback he got on the *Versuch* treatise. Among the letters from economists who had seen the article was one from Joseph Schumpeter. The letter initiated correspondence which developed into an increasingly close friendship between Leontief and Schumpeter which would last to the end of Schumpeter's life. They exchanged letters from just after Leontief's return from China until they were both well established at Harvard in the fall term of 1932. Schumpeter from the very beginning looked at Leontief as a very promising talent who could benefit from the wisdom he could offer.

In Berlin Leontief went to see his mentor, Werner Sombart, about the possibility of doing the habilitation in Berlin. He gave Schumpeter an account of what Sombart had said

but Schumpeter took a dim view of what Sombart had to offer. “I am very sad” [*Ich bin sehr betrübt*], he said and emphasized very firmly that the next step was for Leontief to conquer some kind of acceptable position, that was inseparably connected with his habilitation. Sombart’s conditional response was a disguised no [*verhülltes Nein*], he could (or would) not promise a position. Under the given circumstances Leontief would in Schumpeter’s view have to reckon with quite a long time as *Privatdozent*. To have a position was indispensable, without it the habilitation could be nearly worthless.<sup>117</sup>

Schumpeter advised Leontief to accept an offer from the Kiel Institute a renewed contract if he got one, and try to hold on to it. He could then do the habilitation in Berlin but had to be very careful to avoid that someone was set on blocking it because a negative decision could not easily be reversed. Schumpeter feared that might happen. He advised Leontief to talk to Bortkiewicz about it. If he was of same opinion, it would be better to abandon the attempt to avoid a formal defeat. If Leontief got an offer of a new contract with the Kiel Institute, he could continue to work, Schumpeter argued, and gain recognition improving his position until some opportunity for habilitation would open up. Schumpeter elaborated further on his view of the situation:

“When I ask myself what I can do for you, the answer is depressingly bad. A habilitation in Bonn I regard as unachievable for the moment. And with regard to an intervention on your behalf at other universities, the situation was as follows: It has become common to offer me compliments but my advice or wish is much less listened to than that of the first and best faculty politician. In our scientific business we can expect not only ‘structural changes’ which will benefit you, but also that many people by these structural changes will get a bad conscience with regard to me and therefore will be prepared to fulfill which will be in your interest. I cannot express myself clearer than that.” (Schumpeter to Leontief, 21 February 1930, transl. by ob).

The statement perhaps said more about Schumpeter than about Leontief. Schumpeter had his own worries and didn’t share them all with Leontief. His years in Vienna had given him many foes, particularly due to Schumpeter’s brief and unsuccessful career as banker. He was misjudged on untrue rumors.<sup>118</sup> But he seemed to think that the situation would soon change and then he would be better able to exert influence. In Bonn he knew that any protégé of his would be blocked from achieving habilitation. One of his foes was indeed Werner Sombart who eventually would block Schumpeter from being offered a chair at Berlin University. That would also impact on Leontief in an unforeseen way, as we shall see.

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<sup>117</sup> Schumpeter to Leontief, 21 February 1930.

<sup>118</sup> See Stolper (1994).

Schumpeter added that what he just stated had nothing to do with the question of whether Germany was the right place for Leontief in the future. Schumpeter would make no judgment on that, especially as it did not fall under his duties to scare a promising talent away from Germany. In Schumpeter's view Leontief's prospects in Germany were not too bad, if the next two-three years could be bridged over in a way which did not expose him or tie him up in any direction.<sup>119</sup>

Schumpeter's advice was carefully set out but Leontief may not have been able to see fully through it. Of course, Schumpeter was not able to see better than others the ominous turn events would take in Germany within three years. Schumpeter at this time considered his own future to be in Germany and that that would be the best choice for Leontief too. (That they would be colleagues at Harvard within three years was hardly imaginable for any of them at this time.)

In line with Schumpeter's advice and his own inclinations Leontief re-joined the staff of the Kiel Institute from 1 April 1930. The project he had got approved in his contract was a closer investigation of the structure and development of the iron market. It had been studied along with other markets in *Versuch* but not with very satisfactory results. His project was in the Institute jargon from the very beginning spoken of as the *Eisenaufsatz* ("Iron study"). To make his demand and supply analysis work for iron and other industrial raw materials had become of major importance for Leontief. The problems were all on the supply side.

Leontief wrote to Schumpeter in May 1930 to tell him what he was working on after rejoining the Kiel Institute, intimating that his choice of looking closer at the iron market had not been done without some doubts. He regarded the risk of failure as far from negligible but on the not too demanding field of agricultural products there was hardly anything more of theoretical relevance to do. Furthermore, in that field the Leontief's method for determining elasticities had already been taken into use in several places, e.g. by the US Bureau of Agricultural Economics in USA. He had also heard from Bresciani-Turroni in Cairo, that an analysis of cotton market had just been initiated by the Egyptian government.<sup>120</sup>

Leontief argued that the "resistance" of the iron market to analysis would yield through a sober theoretical analysis. When the principal problems had been sorted out the adequate methods of calculation could surely be derived with logical stringency. Systematic experiments since March had shown that the undertaking was not hopeless.

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<sup>119</sup> Schumpeter to Leontief, 21 February 1930, transl. by ob.

<sup>120</sup> Costantino Bresciani-Turroni (1882-1963) was one of the most highly regarded Italian economists and as an outspoken opponent of fascism he lived in exile in Cairo. Bresciani-Turroni became a leading Italian member of the Econometric Society in the early 1930s and was offered (but declined) the position as research director of the Cowles Commission in the late 1930s.

The difficulties related mainly to the cyclical fluctuations. He enclosed a note titled *Problemstellung* in the letter to Schumpeter. He had prepared it the basis of sampled experiments – for his own use – and these would in the continuation serve as connecting threads [*Leitfaden*]. Leontief appealed to Schumpeter that it would be of the utmost interest for him to know Schumpeter’s opinion about his approach.<sup>121</sup> But Schumpeter had too much on his plate at the time to study Leontief’s problem.

Leontief also passed on to Schumpeter that the latest paper by Hans Staehle about demand and supply analysis and published in *Giornale degli economisti* (Staehle 1930) had been so loaded with superlatives that it had made him blush. He also received a letter from Elizabeth Gilboy at Harvard who praised the *Versuch* treatise; she is likely to have been the first to mention Leontief’s work in an English journal (Gilboy 1930). Gilboy’s letter opened up a relationship with Leontief that would last for decades, first through Gilboy’s role in the Harvard Committee on Research in the Social Sciences and from 1948 as administrator of the Harvard Economic Research Project.

But the responses to *Versuch* were not all positive. Henry Schultz was in the final stage preparing a follow-up volume of Schultz (1928) just as Leontief (1929) appeared. Schultz hastily added an appendix with a critical review of Leontief’s work (Schultz 1930). As Schultz was so influential in the field of empirical demand analysis Leontief felt that Schultz’s criticism was had put him in a vulnerable position and considered how to address it.<sup>122</sup>

Schumpeter did not respond to Leontief until the end of August 1930. He had been stuck with work to prepare for a period of absence. He told Leontief that he would leave on 9 September for Harvard and remain there until Christmas. He would then leave for Japan and Tahiti and not to return to Bonn until the end of April 1931.<sup>123</sup>

Schumpeter had talked with Emil Lederer, professor at Heidelberg, about his high appreciation of Leontief’s talent and the habilitation issue and.<sup>124</sup> Marschak, Leontief’s colleague at the Kiel Institute would leave in the autumn of 1930 to do his habilitation with Lederer at Heidelberg and remain there as *Privatdozent*.

Schumpeter had little to say about Leontief’s plan for the *Eisenaufsatz*. He found Leontief’s letter of 31 May, even with enclosed *Problemstellung*, too brief to fully comprehend Leontief’s approach. He agreed that with iron the problem at stake was

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<sup>121</sup> Leontief to Schumpeter, 31 May 1930. The *Problemstellung* note has not been found.

<sup>122</sup> Schultz (1930) was published in a well-known series edited by Eugen Altschul. Schultz made his paper with appendix also available in English as a mimeograph.

<sup>123</sup> Schumpeter revised his itinerary slightly to stay in USA beyond Christmas so that he could attend, and indeed chair, the founding of the Econometric Society in Cleveland, Ohio on 29 December 1930.

<sup>124</sup> Lederer and Schumpeter had both been members of the German Socialization Commission, chaired by Karl Kautsky after World War I.

completely different from that of consumption goods and agricultural products which both were simple to deal with. The material seemed to Schumpeter to ask for exponential curves which could reflect the cyclical oscillations after corrections for population, price level and income but Leontief's approach did not comply with that. As the iron price would be suitable for a theoretically really well underpinned investigation, Schumpeter expressed deep regrets that they could not remain in contact in the near future due to his forthcoming trip to America and Japan.<sup>125</sup>

In October 1930 the roof started to tumble, the prospects for a brighter employment possibilities in the Weimar republic vanished. Leontief wrote to Schumpeter that the budget cuts (*Sparmassnahmen*) had reached the Kiel Institute. The Prussian and the *Reich* financial support had been merged. The immediate consequence was the dismantling of the *Astwik* department. Leontief was told that he was allowed to finish the *Eisenaufsatz* but from 1 April 1931 he would have to find something else. As Leontief considered it virtually impossible for him to find another position in Germany, somewhat in despair he conveyed to Schumpeter that it would have to be either America or England. As Gerhard Colm had just returned from England and told Leontief that his paper there had evoked great interest, Leontief added that he preferred England if he had to leave. He asked Schumpeter for advice about a position, making it clear that a "productive" position would be much preferred over a stipend.<sup>126</sup>

Schumpeter responded to Leontief from Harvard at the end of November 1930. He regarded it as an unfortunate mishap that the austerity measures had hit before habilitation had been possible. Schumpeter recommended to enlist the help of Lederer to try to get something at Wagemann's *Institut für Konjunkturforschung*. Schumpeter, still convinced that Leontief's future ought to be in Germany. He was worried that if Leontief left Germany it would be even more difficult to arrange the habilitation. On the other hand, if Leontief insisted that he couldn't find anything in Germany, Schumpeter would primarily recommend USA. In view of Leontief's stated preference for England, Schumpeter would put out feelers, hoping to receive responses before he left for Japan, as he after that would be out of reach until the end of April 1931. He promised to let Leontief know if he heard anything.<sup>127</sup>

Schumpeter also offered advice regarding Henry Schultz's sharp attack on the *Versuch* treatise: "The attack of professor Schultz does not hit at the essentials. However, in your place I would respond, calmly and politely." Then he added another piece of advice he also

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<sup>125</sup> Schumpeter to Leontief, 25 August 1930.

<sup>126</sup> Leontief to Schumpeter, 22 October 1930.

<sup>127</sup> Schumpeter to Leontief, 28 November 1930. The only "feeler" put out in was to Theodore Gregory at London University, formerly at LSE. Gregory, a member of the Macmillan committee, did not respond.

would reiterate later: “How about an English paper, I would be happy to help to get it published.”<sup>128</sup>

Leontief was Schumpeter’s soothing advice and realized he had overreacted. He responded to Schumpeter’s letter on 10 December 1930 that he had for a while been aware that if he had to leave Germany, it might become extraordinarily difficult to return. But he was worried, he felt he could not risk to be without a position. With regard to the criticism from Henry Schultz, he informed Schumpeter that Eugen Altschul had indeed contacted him earlier in the autumn with a proposal that Leontief responded to the Schultz criticism and had it issued in the Frankfurter series edited by Altschul. Leontief had liked the idea and aired it with the Institute leadership, only to be told that the Institute would exercise its right to prevent “independent” publications by its scientific staff. Leontief thought he might at least be allowed to issue a short note in English but put it aside for the time being.<sup>129</sup>

### **Ezekiel’s intervention**

In November 1930 Mordecai Ezekiel turned up at the Kiel Institute. Ezekiel, born 1899, was an experienced agricultural economist who had worked for the US Department of Agriculture since 1922. His Ph.D. was from the Brookings Graduate School of Economics and Government in 1926. In 1930, Ezekiel published a brick of a book, *Methods of Correlation Analysis* (Ezekiel 1930), which soon became a standard reference volume on statistical methods. In that same year he began work as Assistant Chief Economist with the Federal Farm Board. From September 1930 to September 1931, Ezekiel had a Guggenheim fellowship for studying methods used by leading European governments for directing and controlling economic activity, he also intended to visit the Soviet Union.<sup>130</sup>

In Kiel Ezekiel met Leontief and took an immediate liking to him. Ezekiel was already familiar with Leontief (1929) and quite impressed with it. Ezekiel intervened in Leontief’s life in a way that turned out to be very fortunate, by offering to help Leontief to investigate possibilities for visiting the United States. Ezekiel suggested four institutions which might possibly be willing to offer employment for a limited period and could offer suitable working conditions for such empirical studies Leontief was interested in. And then he carefully selected the person to address in each of the four institutions. They were Edwin G. Nourse in the Brookings Institution, Simon Kuznets in the National Bureau of Economic Research, Howard Tolley at the Giannini Foundation at the University of

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<sup>128</sup> Schumpeter to Leontief, 28 November 1930. The only “feeler” put out in England seems to have been to Theodore Gregory at London University, formerly at LSE. Gregory was also a member of the Macmillan committee at the time, but did not respond.

<sup>129</sup> Leontief to Schumpeter, 10 December 1930.

<sup>130</sup> Ezekiel later became an influential policy adviser in the New Deal period.

California at Berkeley, and Col. Leonard Ayres at the Cleveland Trust Company, Cleveland, Ohio, which had a research department of high reputation.

Leontief was delighted about Ezekiel's suggestions and decided right away to apply to all four institutions. He wrote identical application letters to all four institutions, while Ezekiel wrote in recommendation letters accompanying the applications that Leontief would like spend one year in USA, to work on statistical problems in an institution where he would have opportunities of applying his methods in practical research.

In his application letters Leontief set out his cv, and listed the four publications he had published so far. He mentioned his current work, describing it as an investigation of the iron markets and prices, to be finished by the end of March, 1931. As motivation for his interest in visiting the USA he stated: "Being particularly interested in statistical price analysis, I am seeking an opportunity to become acquainted with the American experience in this field, and I would like a chance to apply my recent research on this subject in practical business analysis." Hence Leontief expressed his research interest along the lines of the 1929 article and his ongoing *Eisenaufsatz*. He was firmly within his *Versuch* paradigm, there was no indication of anything pointing in the direction of input-output.

Ezekiel knew Brookings Institution quite well. He addressed Edwin Griswold Nourse who at the time was director of the Institute of Economics at Brookings. Ezekiel spoke of Leontief as "one of the keenest young men now working in the fields of statistical-economic price analysis, particularly in the case of industrial products, where the supply as well as the consumption can be continuously adjusted to price changes." He made Nourse aware of the other three institutions he addressed on Leontief's behalf and expressed the hope that it would be possible for one of them to arrange for Leontief to visit USA. He added: "The arrangement would be as advantageous for you as for him, I feel quite sure, for he seems to me to be one of those intuitive geniuses in the field who occur so rarely."<sup>131</sup>

Colonel Leonard Ayres was Vice President of The Cleveland Trust Company. Ezekiel knew as well and in his letter to Ayres he commented upon Leontief *Versuch* treatise:

"You may have seen his article...in the *Weltwirtschaftliches Archiv*, in which he puts forth an entirely new method of price analysis. This method does away with the old assumption that a given analysis would give either a supply curve or a demand curve, and instead enables the two curves to be determined simultaneously. I have spent a good deal of time going over his method with him here, and am convinced that it will considerably extend the possible scope of price analysis,

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<sup>131</sup> Ezekiel to Nourse, 10 Novemembr 1930. Edwin G. Nourse became in 1945 the first chairman of the Council of Economic Advisors.



particularly in industrial products, where the supply as well as the consumption can be continually adjusted as prices change.”<sup>132</sup>

Ezekiel thus had a very positive assessment of Leontief’s method unlike Schultz and some of those who later reviewed Leontief (1929). Ayres had in his employ the statistician Bradford B. Smith who Ezekiel held in high regard. Ezekiel remarked if Leontief came to work at Cleveland Trust he would have Ayres and Smith to confer with and an interest in much the same industries as those Ayres had been concerned with. Cleveland Trust would therefore be an ideal place for Leontief to spend the time. At the end of the letter Ezekiel made the same remark as in the letter to Nourse that Leontief was “one of those intuitive geniuses” but in this case he added, “and one of whom you have already acquired in the person of B.B. Smith.”<sup>133</sup>

In the preparation of the founding of the Econometric Schumpeter had put forth Leontief’s name as one who ought to be invited to the organization meeting to found the Society. Leontief received at the beginning of December 1930 the invitation to attend the organization meeting in Cleveland, together with the list of all those invited.<sup>134</sup> It is easy to understand Leontief’s enthusiasm about the Econometric Society, as the key sentence in the draft constitution that the Society’s aim was to “*promote studies that aim at a unification of the theoretical-quantitative and the empirical-quantitative approach to economic problems and that are penetrated by constructive and rigorous thinking similar to that which has come to dominate in the natural sciences*” must have appealed to Leontief. He wrote to Irving Fisher to express his interest in becoming a member and convey his regrets that he was unable to attend. Leontief noted that Ezekiel was also invited and unable to attend.

At the end of 1930 economic prospects had changed also in USA, including for the institutions Leontief had applied to. He never got any response from the Giannini Foundation. Ayres responded that their research work was more likely to contract than expand; no further commitments could be made. Ayres at Cleveland Trust went out of his way, however, to offer constructive advice, explaining that owing to the severe business depression the USA was experiencing a considerable number of industrial and financial institutions had been releasing research workers of unquestioned ability: “Their difficulty in making new connections leads me to believe that you would find your efforts more fruitful if they were directed toward making a connection with some of what he called the

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<sup>132</sup> Ezekiel to Ayres, 10 Nov. 1930.

<sup>133</sup> The remark suggests Ezekiel’s high appreciation of Smith whose deep insight in econometric analysis seems to have been unacknowledged at the time, see Mills (2011).

<sup>134</sup> Bjerkholt (2017).

“subsidized” research institutions in this country.”<sup>135</sup> Ayres pointed particularly to NBER and Brookings Institution but mentioned also the Bureau of Agricultural Economics of the US Department of Agriculture and the Federal Farm Board. The prices with which the latter two were concerned were naturally agricultural prices. He passed on to Leontief also that there had been a recent reorganization in the Department of Commerce which contemplated more thoroughgoing analyses of industrial commodity prices, although Ayres didn’t really know much about it. In Ayres’ view expressed his belief that Leontief’s best opportunity lay with the subsidized institutions he had mentioned.

Both Brookings Institution and NBER were well endowed institutions, both benefitting from Rockefeller Foundation grants in addition to other funds. And Ayres was right, they had something to offer. Nourse of Brookings Institutions responded to Leontief that, “it would be a cause of great gratification to me if we could be the means of facilitating your spending some time in research work in the United States.”<sup>136</sup>

Nourse offered two options. One was a regular staff appointment at the Institute of Economics, directed by Nourse; the other a fellowship under the Training Division. Fellowships were granted for an academic year of nine months, offered complete freedom to pursue whatever line of work the applicant was most interested in, and came with a small stipend “just about sufficient to defray your expenses”. For a staff appointment the key issue was the project, as it had to fit into the research program of the Institute of Economics. Nourse thus invited more information from Leontief as to what he was interested in, including the range of salary he would consider appropriate in connection with a staff appointment. The fellowship stipend was small but better than nothing. It would provide a chance to visit USA, get acquainted with institutions and people. Leontief decided to apply for the fellowship and pursue possibilities at NBER as well.

Leontief checked out with Schumpeter a rumor he had heard that it was the intention of the new international economic association to publish its journal in Europe; perhaps that could be an opportunity for him for a position in Europe? The “association” was the Econometric Society, to be founded two weeks later. But a journal for the Society was not even on the agenda for the organization meeting. But what Leontief had heard was not taken out of thin air. Irving Fisher had in fact written to Bernhard Harms to inquire about the possibility of the Kiel Institute actually publishing the journal on behalf of the (not yet founded ) Econometric Society. Leontief had hardly seen the letter from Fisher only heard about it from one of his senior Astwik colleagues. Leontief knew that Schumpeter was in the inner circle among those involved in preparing the Society and took the opportunity to express his interest.<sup>137</sup>

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<sup>135</sup> Ayres to Leontief, 2 December 1930.

<sup>136</sup> Nourse to Leontief, 28 November 1930.

<sup>137</sup> Schumpeter to Leontief, 28 November 1930.

Leontief didn't seem to have had very much belief that the applications would come to anything. He had not even mentioned Ezekiel's initiative to Schumpeter but enclosed a copy of the application letters about two weeks after he sent them. He had barely sent off the copy to Schumpeter when he received from Simon Kuznets a response on the letter he had sent to NBER. Kuznets was at the time one of ten to twelve persons on the research staff of NBER, including the two directors of research, Wesley C. Mitchell and Edwin F. Gay. Kuznets was in fact quite upbeat about the prospect of having Leontief as a colleague at NBER for a year. He told Leontief that NBER had introduced one year earlier a new recruitment system. Every year three one-year research associate positions would be offered for competition.

Before he answered to Leontief Kuznets had discussed the matter with Mitchell and with Frederick C. Mills and Pierce Williams on the research staff to make sure that there was no problem about a foreign citizen taking part in the competition and possibly holding a position with the Bureau: "I wanted to find out from them whether there will be a good chance of your obtaining a position as a Research Associate of the Bureau for the next year. They seem to think that there was no reason why your candidacy should not be given very respectful consideration."<sup>138</sup> Kuznets further stated as his personal opinion that Leontief had a very good chance of being a successful candidate and that it decidedly would be worthwhile for him to apply. That turned out to be correct assessment.

With his response letter Kuznets sent the official announcement of the NBER research associate fellowships and then proceeded to give Leontief most valuable advice. The application form asked for references, Kuznets conveyed that the Committee deciding the selection of research associates relied much on the opinion of references. As it in practice it was cumbersome to get in touch with European references, Kuznets advised to obtain an adequate number of references from American scholars. He further suggested that it would be advisable to submit copies of all the publications listed in the application. The application required an attached note about the project to be undertaken. Kuznets advised that a quantitative study of an economic problem with application to the United States would receive greater consideration, for the simple reason that while working in the United States Leontief could most advantageously study an American problem.

Leontief's hope was boosted by Kuznets' message. He hurried to update Schumpeter about the promising news from Kuznets, and asked his advice about whom to ask for recommendation letters. But possibilities in Germany were also on his mind. He told Schumpeter that he had heard that many staff changes had taken place in the *Konjunkturinstitut*, which he took to imply that there was still hope for him to remain in Germany.<sup>139</sup>

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<sup>138</sup> Kuznets to Leontief, 2 December 1930.

<sup>139</sup> Leontief to Schumpeter, 13 December 1930.

The NBER application form asked for comprehensive information about education, experience and publications which Leontief provided together with list of references which comprised from USA W.L. Crum (Harvard), M. Ezekiel (USDA), H. Hotelling (Columbia), Henry Schultz (Chicago), G.F. Warren (Cornell) and Frederick Waugh (USDA) and in addition Luigi Amoroso (Rome), Joseph Schumpeter Bonn), Jean Lescure (Paris), Arthur Bowley (LSE) and L. Bortkiewicz (Berlin). Leontief's two-page note about the project to be pursued at NBER was follows:

Memorandum on proposed analyses of supply and demand relationships prevailing in the markets of non-agricultural commodities.

For the last two years I have been mainly concerned with developing refined statistical methods as how to measure elasticity of demand and supply within industrial markets. The objective of this research was to overcome the difficulties which arise from the application of the usual method of multiple correlation to the intricate conditions prevailing in non-agricultural markets. The new method which I was able to work out will solve some of the problems involved by calculating demand and supply curves from one identic set of data by one process. I beg to enclose a brief of this "simultaneous" method.<sup>140</sup>

After having completed this method I chose to apply it to the history of the American iron markets in order to prove – or disprove – its workability. The results hitherto arrived at encourage me to believe that the application of this method on a large scale will benefit the quantitative approach to the problems of economics. I venture to say that this research would fit in with the general program of the Bureau as it has been indicated by your publications.

I beg to develop somewhat further my program as outlined above.

I. Scope of the investigation:

Some markets of non-agricultural products for which sufficient data on prices, production, consumption and stocks are available; such as:

- a. in the sphere of consumer goods: textiles, automobiles, boots and shoes etc.
- b. in the sphere of producers goods: metals, coal, building materials etc.

II. Objective of the investigation:

- a. Analysis of the different markets as to kind and magnitude of their elasticity. Combining Marshall's distinction between long and short period elasticities with the usual timing of statistical series. A fourfold approach would be advisable: 1) long-run

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<sup>140</sup> The brief was a two-page summary of the method of Leontief (1929), not included here.

elasticity; 2) elasticity within the business cycle; 3) seasonal elasticity; 4) elasticity of minor fluctuations.

- b. Interdependence of elasticities. This investigation would be of special importance for some industrial markets, the commodities of which bear a close relationship to different stages of the industrial production (f.ex. raw materials, half-finished goods, consumer goods).

### III. Technicalities:

In order to carry through a research of this scope I would need the constant help of a statistically well-trained person; I assume that the data and sources which will be needed will be easily provided by your Bureau.

The project described was straight out of the *Eisenaufsatz* and can hardly be said to be about input-output analysis. Point IIb on the interdependence of elasticities is also worth noting, as it suggested explaining supply through vertical linkages. It is the first inkling of moving away from Marshallian partial equilibrium and towards a general equilibrium approach. One might suggest that Leontief was trying to cope with more aspects of an industrial economy that he was able to confine into a single analytical scheme.

### **Exploring options**

After the feedback he had got on the applications Leontief had become hopeful of the possibility of visiting USA in the coming year, either at NBER or Brookings. Leontief hardly gave any thought at all at this point to the possibility for finding permanent employment in USA. He was instead concerned with his possibilities in Germany, not least the *Konjunkturinstitut* in Berlin. Although the Kiel Institute had been a good place to work with interesting colleagues, *die Kieler* as Schumpeter referred to them, Berlin had greater attraction.<sup>141</sup>

In Schumpeter's last letter to Leontief before he left USA for Japan, he was less hopeful than Leontief: "I do not believe that there are much chances in the *Konjunkturinstitut*. Nor do I think that the research fellowship at the National Bureau of Economic Research will be easy to conquer as I know of a great many candidates. ... Of course, this would mean leaving Germany for good. Your best chance seems to me to be Colonel Ayres."<sup>142</sup> But Schumpeter was wrong on both counts: Colonel Ayres had nothing

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<sup>141</sup> Schumpeter referred to Leontief's colleagues as «die Kieler» when he asked Leontief what they were up to.

<sup>142</sup> Schumpeter to Leontief, 27 December 1930.

to offer, while NBER welcomed Leontief as it would turn out. Schumpeter had heard nothing from England.

In December 1930 Ragnar Frisch wrote to Schumpeter about the *Versuch* treatise. Frisch had been at Yale since the beginning of 1930 and had heard about Leontief's paper Charles Roos whose offprint he borrowed and studied thoroughly. Frisch was very critical of the statistical procedures in Leontief's treatise: "I hate to be too critical of other people's work, but it seems to me that Leontief also has been fooling himself."<sup>143</sup> He continued with setting out an interesting line of thought inspired by Leontief's problem but not addressing it directly:

"I got a little excited ever these, as I see it, misleading methods, so I looked up my own not yet finished notes on the subject and gave some new thought to the matter. It seems quite surprising to me that the problem has not yet stated been stated in the following simple and rather natural form.

Let  $x_1, x_2, \dots, x_n$  be a set of economic magnitudes (price, quantities consumed, produced etc.) for which we have a certain static theory, in the sense that we postulate for a priori reasons a number of structural relations:

$$\begin{aligned} F_1(x_1, \dots, x_n; a_{11}, a_{12} \dots) &= 0 \\ F_2(x_1, \dots, x_n; a_{21}, a_{22} \dots) &= 0 \\ \dots \dots \dots \\ F_n(x_1, x_2, \dots, x_n; a_{n1}, a_{n2} \dots) &= 0 \end{aligned}$$

The relation  $F_1 = 0$  may represent a certain demand relation,  $F_2 = 0$  a certain supply relation, etc. Each of the functions  $F_1, F_2 \dots$  contain a number of constant parameters  $a_{ij}$  that characterize the shape of the function. The problem of determining such a set of parameters for actual data is an interesting example of an econometric problem.

Now we have the curious situation that if the material at hand fulfills our assumptions it is impossible to determine these constants  $a_{ij}$  that express the nature of our assumptions, because in this case we would only have a single observation, namely, the one corresponding to the solution of the system. But if our assumptions are not fulfilled, then it may be possible to determine what they constants  $a_{ij}$ .

Suppose, for example, that the functions  $F_1, F_2 \dots$  contained also another set of variables,  $\xi_1, \xi_2 \dots, \xi_m$ ,  $m$  being at least equal to 1. Our set of structural relations then will take the form

$$\begin{aligned} F_1(x_1, x_2, \dots, x_n, a_{11}, a_{12} \dots, \xi_1, \dots, \xi_m) &= 0 \\ F_2(x_1, x_2, \dots, x_n, a_{21}, a_{22} \dots, \xi_1, \dots, \xi_m) &= 0 \\ \dots \dots \dots \end{aligned}$$

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<sup>143</sup> Frisch to Schumpeter, 13 Dec. 1930.

$$F_n(x_1, x_2, \dots, x_n, a_{n1}, a_{n2}, \dots, \xi_1, \dots, \xi_m) = 0$$

Furthermore, let  $\Omega(\xi_1, \xi_2 \dots \xi_m)$  be the frequency distribution of the set  $(\xi_1, \xi_2 \dots \xi_m)$ . Then to this frequency distribution of the set  $\xi$  there corresponds a certain frequency distribution  $\Pi(x_1, \dots, x_n)$  known from observation. We see that now we really do get variations in the set  $(x_1, \dots, x_n)$ .

Now it is quite clear that the relation between the distribution  $\Omega$  and the distribution  $\Pi$  depends in a characteristic way on the magnitude of the parameters  $a_{ij}$ . Further, by expressing the fact that the  $x$  distribution deduced from the  $\xi$  distribution (that is, from the function  $\Omega$ ) is identical with the actually observed  $x$  distribution (that is, with the function  $\Pi$ ), we obtain a system of equations in  $a_{ij}$  which will furnish a solution of the set  $a_{ij}$  provided the equations in question are theoretically and practically solvable.

Thus in point of principle, the constant  $a_{ij}$  may be determined if we know  $\Omega$ . Actually, of course, we do not know  $\Omega$  but we may make some more or less plausible assumptions about it. To every such assumption corresponds a set  $a_{ij}$ . In an actual case, the formulae connecting  $\Pi$  and  $\Omega$  would give an exact expression for the effect of a change in the assumptions regarding  $\Omega$ . Such a formula, it seems to me will be of some value as a check on the perfectly gratuitous assumptions which are sometimes made in this field.” (Frisch to Schumpeter, 13 December 1930).

Frisch’s letter thus foreshadowed an ingenious kind of probability approach to Leontief’s problem. Leontief never got to see the letter from Frisch. When Frisch in 1933 published his *Pitfalls* essay with a sharp criticism of Leontief’s work it came without this line of thought.

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After advising Leontief with the applications Mordecai Ezekiel left for Berlin where he took lessons to improve his German language. He visited Leontief’s parents several times and received suggestions for his trip to Russia. Leontief’s mother taught him the Russian alphabet. Leontief’s father updated Ezekiel about the positive response letter from Kuznets and Ezekiel felt reassured Leontief would get one of the fellowships. Ezekiel wrote immediately to Leontief to dismiss any fear Leontief might have had that the competition would be hard: “I don’t think competition from others should affect your opportunities, as your own method is unique, and Kuznets would be a top notch man to help you develop and apply it.”<sup>144</sup>

Leontief visited his parents in Berlin for a week around Christmas and New Year. Ezekiel was travelling in Germany and he and Leontief did not get to see each other again

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<sup>144</sup> Ezekiel to Leontief, 15 January 1931.

in Europe before Ezekiel in March left for two months in Russia. Ezekiel was, however, bent on staying in contact with Leontief to continue to give advice. On his return from Russia on a steamer across the Black Sea in cold weather and rough sea Ezekiel picked up a letter from Leontief in Constantinople and got to know that the response from Brookings had been positive, although it disappointingly in financial terms. Ezekiel wrote from Thessaloniki, advising Leontief to write to Brookings, accept tentatively but state that he had another pending application and might resign if the other offer worked out. He suggested that Leontief specifically to stated that this way of keeping the options open was Ezekiel's suggestion so that if they found it irregular they would put the blame on him. Ezekiel thus tried to use the leverage he had with Brookings to Leontief's advantage. Obviously, he would very much personally like to have Leontief in Washington and introduce him at USDA.

But it was a lost case and Ezekiel had surmised that, although he didn't know exactly what NBER would offer. In New York it would cost Leontief about \$100 a month for room, board, carfare, and other incidentals. These expenses would with Brookings have been covered by the fellowship which in addition had an allowance of \$100 a month. Hence the offer from NBER would have to be at least \$200 a month to make Leontief better off. It was in fact \$300 a month plus \$400 for travel, while Brookings offered nothing for travel. Ezekiel concluded out that Leontief wouldn't be able to see much of USA on the terms Brookings offered.

But it wasn't only about money. At NBER Leontief would be able draw on the support of clerks and other assistance in his work while in Washington he would have to do all the work by himself. Ezekiel had nothing more to offer but reiterated that if Leontief chose Brookings as a very pleasant place to work he could mingle with the economists of the U.S. Dept. of Agriculture who "would probably be as stimulating as the people in New York."<sup>145</sup>

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Schumpeter had told Leontief that he expected to be back in Bonn at the end of April. But already on 3 April 1931 Leontief wrote in a somber mood to Schumpeter's address in Bonn to tell him that he had been fired [*ausgeschieden*] from the Kiel Institute from 1 April. His contract had expired with no offer of renewal. This had been clearly foreshadowed by the Institute but Leontief might have nurtured a faint hope that he would be offered some additional months. His work on the *Eisenaufsatz* was nearly completed. Leontief hoped to see it published in the July issue of *Weltwirtschaftliches Archiv*, but that did not work out. Leontief gave Schumpeter a brief summary of what the *Eisenaufsatz* was about it.

The work was a largely theoretical study supported by empirical illustrations. Leontief developed an analytic structure for closer scrutiny of the total supply elasticity for

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<sup>145</sup> Ezekiel to Leontief, 13 April 1931.



individual production branches. The supply elasticity was influenced by the scale property, for which Leontief introduced the *transformation elasticity*, and by the supply elasticity of the total input which could be structured as successive stages of a vertical production chain. He developed appropriate theoretical relations and also reached empirical results about the iron market as an improvement over what was found in the *Versuch* treatise. Monopoly pricing had been dealt with by means of a concept of “monopolistic supply curve”. Analytically this was achieved as a general result of theoretical interest beyond the specific empirical application.

The statistical procedure had been improved and extended to a “double precision measure”. It was now possible to assess numerically the relative precision of each of the determined curves. Empirical results such as some of those in *Versuch* which some critics had declared meaningless were now excluded because of the lack of precision.

Leontief had earlier, as mentioned above, been forbidden to spend time on an article to defend his *Versuch* treatise against the criticism that had been directed against it by Schultz and others. The situation had now changed. Colm had indeed asked Leontief to write a “defense paper” (*Verteidigungs-aufsatz*) in *Weltwirtschaftliches Archiv*. When Leontief hesitated in complying, it was made clear that to him that such refusal would be considered as a break with the Kiel Institute and would not be in Leontief’s best interest. It was an unpleasant situation, ultimately caused by the financial hardship. Leontief’s situation was not very comfortable.

Leontief kept informed about the *Konjunkturinstitut*. He had an application pending there and knew that Wagemann kept one position vacant despite knowing about Leontief’s interest in it. But Wagemann had not given him a final rejection. Leontief believed for right or wrong that influence was exerted on Wassermann not to hire Leontief.<sup>146</sup>

Then it seemed that the NBER option was within reach. Mitchell had written to Leontief and mentioned him as a “most promising candidate.” The decision was expected very soon, before the end of April. But if he got it, he would still be in a fix on his return to Germany after one year.

Adolf Löwe had been appointed professor at Frankfurt University and would leave in the autumn. He had suggested to Leontief that he could do his habilitation in Frankfurt with Zizek who was professor Frankfurt University. Leontief didn’t know what to think about this proposal. It seemed to him that the habilitation question was the most important just now for his future possibilities. Again he requested Schumpeter’s opinion.<sup>147</sup>

Later in April 1931 there were new developments on the applications to visit USA. Leontief had adhered to Ezekiel’s advice but Brookings had insisted on an immediate answer. Luckily, a letter from NBER arrived on the same day with the message that

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<sup>146</sup> Leontief to Schumpeter, 3 April 1931. Leontief suspected that Lorenz exerted influence.

<sup>147</sup> Leontief to Schumpeter, 3 April 1931.

Leontief had been appointed as a research associate in NBER. The Brookings fellowship was no longer of interest.

Leontief had been appointed as number one of the three NBER research associates for 1931/32. According to the NBER Annual Report 1931 there had been a large number of well-qualified applicants.<sup>148</sup> Leontief was described as a student of the conditions of demand and supply in selected commodity markets and his project as an “attempt toward an analysis of the mechanism of economic, especially cyclical, changes.”

Leontief wrote immediately to Schumpeter in Bonn that he had been appointed research associate of NBER and had to take up the position by 1 October. Schumpeter responded immediately to Leontief to congratulate him with the appointment. But more important he wrote to say that whether he returned to Germany after one year or would try to make a career in America, it would be of the greatest importance that he could do the habilitation during the summer semester before he left for America. Schumpeter had no doubt that Leontief’s published papers plus the *Eisenaufsatz* would be more than sufficient. Although time was short Schumpeter figured it could be done with the help of Löwe and the two professors in Frankfurt, Zizak and Pribram.<sup>149</sup> In his view, Leontief should absolutely go for it: “You may discuss my advice with Löwe and Lederer. I am very curious about your new work. I hope I shall see before you leave.”<sup>150</sup> Leontief answered immediately that habilitation before departure would undoubtedly have been the best but time had run out for it. Löwe would not be in Frankfurt until the winter semester. Leontief very much wanted to meet with Schumpeter and discuss both the habilitation and the problems of his current work (*Eisenaufsatz*), which was not yet finished.<sup>151</sup>

Schumpeter got in touch with Löwe and concurred in that it would be better to do the habilitation after Leontief’s year in New York. By that the habilitation issue was on the shelf until Leontief returned from the United States 1932 (which didn’t happen). Schumpeter again deplored the unfortunate fact that he was not able to put it through in Bonn. In Frankfurt the ground was better as he knew both Pribram and Zizek, and even more so when Löwe also was in place. He thus gave up the idea of habilitation before departure with a remark that it would be foolish to venture a *Husarenritt* which even if might work, would be risky.

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<sup>148</sup> The other two appointed research associates were Carl T. Schmidt and A. G. Silverman.

<sup>149</sup> Karl E. Pribram (1877-1973) was Austrian, had been professor at Vienna University, and worked for ILO in the 1920s. He was professor in Frankfurt am Main from 1928 and lost his chair in 1933 because of his Jewish extraction. Emigrated to USA in 1934. Member of the Econometric Society.

<sup>150</sup> Schumpeter to Leontief, 20 April 1931, transl. by ob.

<sup>151</sup> Leontief to Schumpeter, 21 April 1931

Schumpeter then extended an invitation to Leontief for dinner in his Bonn home on Friday 13 May 1931.<sup>152</sup> But Leontief declined the invitation to finish the *Eisenaufsatz*. He had been waiting day after day for 3-4 weeks for statistical calculations to be done in Kiel. They had finally arrived and then he wanted to finish and submit the manuscript to *Weltwirtschaftliches* as soon as possible, hoping to see the article in print before he left for America. Leontief suggested that perhaps he might instead visit Bonn in first week of June. Schumpeter who was as keen to meet with Leontief as he was to see Schumpeter, invited him for Wednesday 3 June at 13:30 *zum Mittagessen*.<sup>153</sup>

Leontief left Berlin on 3 June and met with Schumpeter in his home at the agreed time. After another day in Bonn he then visited in succession Frankfurt and Heidelberg and returned to Berlin on 7 June. He wrote to Schumpeter that the trip had come off according to plan. Schumpeter's prior letters to Frankfurt had resulted in a very friendly welcome for him in Frankfurt. The purpose of visiting Frankfurt was and met with Zizak and Pribram was of course to prepare the ground for habilitation but this could absolutely not be stated. Zizak had very thoroughly presented the statistical seminar for him, also at some length elaborated on advanced topics. Leontief got less time with Pribram as he was about to leave for America. Pribram had asked whether Leontief's visit was due to any specific purpose, which Leontief denied. He left with the impression that neither Pribram nor Zizak had any clue about Leontief's real motivation. But the secrecy also meant that Leontief had no way of assessing the attitude they might take to his habilitation sometime in the future. Mannheim, whom Leontief knew from earlier, was ill and but Leontief talked with on the telephone. Leontief also spent time with Eugen Altschul and was impressed with how energetic he was.

In Heidelberg Leontief had stayed with Marschak. He had an interesting conversation with Carl Brinkmann and visited Lederer twice discussing his recent book on technical progress. He then had a meeting with the mathematician Emil Gumbel who helped him with some mathematical questions. Gumbel had mentioned Robert Remak whose work in Gumbel's view was in mathematical terms similar to what Leontief was concerned with. The visit as whole had overall gone very well. The habilitation question was as open as earlier but that was to be expected.<sup>154</sup>

On his return to Berlin Leontief wrote immediately to Remak about Gumbel's impression that they both had come to be concerned with similar problems, essentially "determinant analysis of economic equation systems", Remak from the side of abstract mathematical economics and Leontief from the side of economic theory.<sup>155</sup> There is no

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<sup>152</sup> Schumpeter to Leontief, 29 April 1931.

<sup>153</sup> Leontief to Schumpeter, 26 May 1931. Schumpeter to Leontief, 29 May 1931.

<sup>154</sup> Leontief to Schumpeter, 7 June 1931.

<sup>155</sup> Leontief to Remak, 11 June 1931. About Remak, see the account in Parys (2013, 2014).

record of a response from Remak or a meeting between them. Remak's mathematical formulations had in fact striking similarities with input-output. But they were on different tracks: Leontief's theoretical work was derived from the empirical problems he studied, while Remak was a mathematician.

After the trip Leontief treasured the memory of the day spent in Bonn and the friendly welcome Schumpeter had given him. Leontief also came back to the Wagemann matter, i.e. whether there were any prospects for a position at the *Konjunkturforschungsinstitut*. He realized that there could be no question of getting a proper position when he in two months would leave for America. Leontief had got to know from an inside source that Wagemann was prepared to accept Leontief as an intern (*Volontär*) and also some kind of official relationship for the time Leontief would be in America, which might make the return to Germany easier. But Leontief whether it would be worthwhile for me to accept the awkward position of an intern and the loss of time it would involve.<sup>156</sup> Obviously, he was quite interested in Wagemann's institute.

Schumpeter and Leontief continued to stay in steady contact. At the end of June Schumpeter asked Leontief for guidance as to how to apply the demand and supply analysis. Leontief responded by page references to his paper, enclosing graphs, tables and explanations. Schumpeter wanted to know how things were going and whether he used his free time for something "beautiful and new", or had given himself away to Wagemann?" On the Wagemann matter Leontief was still undecided and apparently not very inclined to accept the conditions he had been offered. He was furthermore dismayed by the fact that the *Eisenaufsatz* would not appear in the July issue of *Weltwirtschaftliches*. It had been submitted one month ago but nothing had been done with it with regard to the technical editing. And to get a copy of the article (for Schumpeter) was impossible.<sup>157</sup> It appeared eventually in January, 1932.

What concerned Leontief at this time was the "*antikritischen*" essay he wanted to write as response to his critics. Leontief now looked at it in the following way:

"The main task consisted of putting all polemics aside and find a basis for the discussion on which one could build further. For this purpose I might try to exhibit the connecting threads between the partial equilibrium – as used in empirical market analysis – and the general equilibrium, to show when and under what conditions general interdependence relates the supply and demand curves of individual commodities, just as if a multidimensional body was projected on to a plane. The real meaning of such a way of looking at it appears in the analysis of

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<sup>156</sup> Leontief to Schumpeter, 7 June 1931.

<sup>157</sup> Schumpeter to Leontief, 20 June 1931; Leontief to Schumpeter, 23 June 1931.

economic changes, the data variations. Unfortunately, I must struggle rather much with determinant theory.” (Leontief to Schumpeter, 23 June 1931, transl. by ob).

The passage confirmed that the *Eisenaufsatz* had caused the confinement to partial equilibrium had been given up. What happened in one market was related to what happened in other markets. This brought the problems into a general equilibrium context. But how to structure and represent the general equilibrium relationships. Leontief was searching for graphic or conceptual ways of representing such multidimensional entities. His modeling structure was not linear.

### **The *Eisenaufsatz***

The *Eisenaufsatz* (Leontief 1932) built on and modified the *Versuch* paradigm. It had been the intention in the *Versuch* treatise to apply the analysis to commodity markets over a longer period. It was attempted mainly the American iron market but had not worked out well. Hence a motivation for the *Eisenaufsatz* was to resolve these problems. Of the 50 pages of the *Eisenaufsatz* the last 30 pages were devoted to show that the new conceptual tools developed in the paper made sense in the analysis of the American pig iron and steel markets from 1879 to 1915. The empirical analysis was limited to that. The motivation thus came from the poor results achieved for iron in the *Versuch*. An aim was to adapt and develop the elasticity method to make it work well for intermediate industrial goods, not only for consumption and agricultural goods. Iron was in this context a particularly important commodity. The remaining part of the paper was an attempt to construct a more general approach the analysis of supply, or more precisely as the title indicates to the supply elasticity, and thereby transcend the narrow partial equilibrium approach of the *Versuch*.

The new approach to supply analysis emphasized the vertical relation (p.67) between the product markets and the markets of the major inputs or cost components (*Kostenelemente*). The idea was that the supply curve for pig iron would be closely related (*in einem gesetzmässigen Zusammenhang*) to the supply curves of iron ore and coke. Another emphasis in the article on the assumption of a given technical structure of productive capacity (p.68). These features suggests a similarity with input-output but the differences were substantial. The verticality is of course a recursive feature. A major input in one branch of production will be produced in another branch of production with other major inputs and so. Leontief called it stages of production (*Stufen*). This is of course also a core idea of input-output.

Leontief introduced two new elasticity concepts: transformation elasticity and cost elasticity. Transformation elasticity expressed the scale property in the relationship between input and output, hence the deviation from proportionality was emphasized. Cost elasticity could be defined as the supply elasticity of the aggregate input into the production

of a commodity (p.69). Leontief's notation was not particularly elegant and often cumbersome. To apply the two new elasticity concepts all inputs of a production branch were aggregated into an input aggregate. This by itself was a serious index problem. The object was then to find the total supply elasticity as a combined result of the transformation and cost elasticity applied to the input aggregate. A key point was to separate in the empirical measurement the transformation elasticity and the cost elasticity from each other. Leontief argued that an immediate insight into the transformation elasticity could be acquired through direct observations of the cost structure of existing establishments at a given point of time. (p73).<sup>158</sup> In view of the monopolistic characteristics of price formation of the American iron market Leontief made an effort at defining the "elasticity of monopolistic supply" (pp.84-87). The cumbersome statistical procedures are ignored here.

When the *Eisenaufsatz* was largely completed but not yet published Leontief summarized the key content of the *Eisenaufsatz* to Schumpeter as follows:

"The paper is largely theoretical, as was the intention from the beginning. I have succeeded in decomposing "total elasticities" for production branches into cost elasticities and transformation elasticities by production stage, develop corresponding theoretical relations, and also achieving interesting empirical results for the iron market. I deal with monopoly prices by means of a concept of "monopolistic supply curve" and supply elasticity. Analytically, this has been set out in a general way allowing arbitrary functional forms, thereby achieving results, which apart from empirical applications, also are of a certain theoretical importance." (Leontief to Schumpeter, 3 April 1931).<sup>159</sup>

The basis for the *Eisenaufsatz* was thus the *Versuch* paradigm, it had not been abandoned, only modified to provide a somewhat better theoretical approach. In the very first sentence of *Eisenaufsatz* Leontief cited the *Versuch* treatise and its *Simultan-Methode* for statistical analysis of supply and demand curves and stated in a footnote his intention of dealing with the criticism raised against it in an "anti-critical essay" (Leontief 1932, p.66, fn.1).

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<sup>158</sup> This is another early example of his emphasis on direct empirical observations.

<sup>159</sup> Leontief enclosed graphs, tables and text excerpts and added that the statistical procedures had been enhanced – over those in *Versuch* – with regard to the control of accuracy, implying that most of the empirical results which the critics had denoted as meaningless (such as the sugar supply curve!) would be discarded as insufficiently precise. This was a reference to the sharp criticism of Schultz (1930), which was concerned with Leontief's results for the sugar market.

## Farewell to Europe

At the end of July 1931, Leontief received the galley proofs and wrote to Schumpeter to say that he would send him the pages which covered the theoretical core of the treatise. It was only one month left until Leontief's scheduled departure for New York on 29 August. He regretted deeply that it had been beyond reach to have a chance to speak with Schumpeter before the departure. He still hoped that Schumpeter would have an opportunity to glance through the extract he had made while it was still possible to make last minute changes. The treatise was above all about changes in economic equilibrium such as they had discussed in Bonn. To finish up the manuscript of the *Eisenaufsatz* had been strenuous and time consuming and the only piece of work he had got done in addition to a review of Marschak's book based on his habilitation thesis (Leontief 1931). Marschak's paper for the forthcoming first Econometric Society meeting at the end of September was based on the book, it was a meeting Leontief would have loved to attend but he was committed to be at work in NBER at that time.<sup>160</sup>

At their meeting in Bonn Schumpeter had elaborated on his own work and plans as it is well known that he did in the circle of younger economists around him at Harvard. In his letter to Schumpeter at the end of July 1931 Leontief expressed delight and excitement that Schumpeter's book on monetary issues (*Geldbuch*) would soon be finished. He also expressed the hope that Schumpeter would soon issue his 1908 book *Das Wesen unter der Hauptinhalt ...* in a new edition, "*Es ist so dringend notwendig*". As students of Schumpeter know, the *Geld-Buch* was never finished and *Das Wesen* never re-issued.

In Schumpeter's last letter before Leontief's departure was no longer so hopeful about the *Geldbuch*, as he had felt nervous and unwell. But in the winter he would devote himself to the new edition of *Das Wesen*, or rather the new book that would come out of that process, adding, "A pity we cannot cooperate on that".<sup>161</sup> Schumpeter regretted that he wouldn't see Leontief again before his departure but hoped he would hear from in on a current basis and expressed his willingness to help with whatever he could. He gave Leontief's his last piece of advice before the arrival in America:

"Whatever your inclination is, look around on everyone and everything in America from the viewpoint of a possible lasting relationship. Maybe it will be for you as for others no other possibility. And if you don't want it, you can turn it down."<sup>162</sup>

Leontief wrote again before departure and enclosed one set of proof sheets for the *Eisenaufsatz* with apologies for sending his own paper when Schumpeter was so

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<sup>160</sup> Leontief to Schumpeter, 28 July 1931.

<sup>161</sup> Schumpeter to Leontief, 5 August 1931, transl. by ob.

<sup>162</sup> Schumpeter to Leontief, 5 August 1931.

overburdened with his own work but he was so curious about what Schumpeter would think of it. He thanked Schumpeter for his advice and guidance and told him that he was totally determined that before the end of the first year in America he had to find a position which would allow him without great sacrifices to continue his scientific work. Leontief felt vulnerable after seeing other prospects crumble and with a touch of despair begged Schumpeter to write to him if any kind of possibility for something that could provide an income should accidentally turn up while he was away.<sup>163</sup>

In August Leontief received from Divisia invitation for the first Econometric Society meeting to be held in Lausanne at the end of September 1931. Much as he wanted to attend he was not able to as his voyage to USA was three weeks before the meeting and couldn't be postponed without risking losing his NBER fellowship.

The departure from Europe also meant separation from the parents for an unknown period. Leontief Sr. stopped working for the Soviet Ministry of Finance at this time, possibly already in 1930. This happened probably as the consequence of an order to return to Moscow. Leontief Sr. had at this time lost his earlier beliefs in more a positive political development in Russia. His change of opinion came through in the reviews he wrote of books about Russia economics and politics. The parents continued to live in Berlin, Leontief Sr. secured teaching assignments at the Berlin University, possibly with the help of Karl Stählin.

Leontief left Europe from Bremen aboard Norddeutscher Lloyd's *S.S. Sierra Cordoba*, arriving in New York on 9 September, 1931. He had asked Ezekiel to book a room for him at the International House. Simon Kuznets met him on Ellis Island.

## **NBER stepping stone to America**

Wesley Mitchell (1874-948) had been director of research of the National Bureau of Economic Research (NBER) in New York since it was founded in 1920 with Mitchell as one of the founders.<sup>164</sup> He was also professor of economics at Columbia University where he was extremely well regarded as he indeed was in the profession at large at the time when Leontief arrived.<sup>165</sup> Mitchell had been a student of Thorstein Veblen, who many American economists considered as the greatest social scientist and economist who had lived on American soil and with whom Mitchell retained a close relationship until Veblen died in 1929. Mitchell succeeded in turning NBER into a well-known and prestigious institution. It had been well endowed financially with generous support from Rockefeller, Carnegie, and other foundations and private sources.

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<sup>163</sup> Leontief to Schumpeter, 10 August 1931.

<sup>164</sup> Mitchell was one of the founders also of the New School of Social Research in 1919.

<sup>165</sup> Rutherford (2004, p.38),



Leontief arrived at the NBER offices on 51 Madison Avenue in central Manhattan on 10 September 1931. As noted above Mitchell was in England during most of Leontief's year at NBER and they didn't get to see much of each other. But Mitchell was well aware of Leontief's arrival. He had taken keen interest in the research associate recruitment program and indeed in the selection of Leontief as the best qualified applicant.

Mitchell had made business cycles the main topic of NBER's research, a field in which Mitchell and NBER promised more than actually achieved. A cornerstone of the NBER research program in business cycles was *Business Cycles: The Problem and Its Setting* (Mitchell 1927) published as the first of two volumes.<sup>166</sup> The second volume was meant to exhibit systematically the cyclical behavior characteristic of different factors in business through analysis of a massive compilation of economic data, mainly time series.<sup>167</sup> In 1931/32 Mitchell was Visiting Professor at Oxford University to work on a second volume for which he hoped to have a manuscript ready for publication before the end of 1933. That turned out to be too optimistic; it was published eventually in 1946 (Burns and Mitchell 1946). A third volume was promised to provide a theoretical underpinning of the NBER business cycles analysis; it was foreshadowed in 1932 as an "attempt... to develop a synthesis which should further our comprehension of this phenomenon of incessantly evolving change."<sup>168</sup> It had not appeared when Wesley Mitchell died in 1948.

Just as Leontief arrived NBER plunged into crisis; the evolving financial and economic crisis in USA had reached NBER.<sup>169</sup> The NBER 1932 report drew a gloomy picture:

"During the past year the National Bureau of Economic Research, in common with the entire economy of the nation and of the world, has suffered seriously. Its budget has been drastically curtailed, the number and remuneration of its working staff have been reduced, and some of its activities and projects suspended. But despite the necessity of cutting the budget by almost two-thirds, in order to bring

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<sup>166</sup> Mitchell's interest in business cycle predated NBER, he had earlier published *Business Cycles* (Mitchell 1913), "a book that cemented his reputation as a scholar of first rank" (Rutherford (2004, p.38). Mitchell (1927) was an update of the earlier book.

<sup>167</sup> The title of the second volume had been pre-announced as *The Rhythm of Business Activity*, but when it eventually appeared it had a different title (*Measuring Business Cycles*) and was coauthored by Arthur F. Burns who was one of the three research associates recruited one year ahead of Leontief.

<sup>168</sup> NBER 1932 report, p.17. For all references to NBER reports, see <http://www.nber.org/nberhistory/annualreports.html>

<sup>169</sup> The Bureau moved shortly afterwards to somewhat more modest office facilities at 1819 Broadway, New York.

expenditures strictly within the available resources, care has been taken to preserve intact essential continuing work; branches have been lopped but the living trunk of this tree of knowledge remains unimpaired.” (NBER 1932 report, p.12).

For a while it seemed as if no one was really sure NBER could make it beyond 1933! The research staff was in 1932 reduced by 40-50 percent. Edwin F. Gay, Mitchell’s co-director of research since 1920, stepped down and went back to Harvard where he was professor of economic history.

Under these circumstances NBER suspended the research associate recruitment program. It had lasted only two years before it was closed but was nevertheless declared a success, as by offering facilities to those well qualified, “NBER research had been stimulated.” Although NBER managed to extricate itself from the severe financial constraints the program was never reopened. Leontief might in retrospect have considered himself rather lucky for this last chance to use NBER as a stepping stone to get to the United States, and it is unlikely that he would have got this chance without the commitment and counseling of Ezekiel and Kuznets.

As NBER had been founded on the idea of studying business cycles and it may seem almost paradoxical that when the depression really hit the studies were suspended rather than stepped up or at least maintained. NBER set it out in the 1932 report:

Before the end of 1931 it became evident, ... that the survey of the depression, which the National Bureau had been prosecuting actively ... should be temporarily suspended. Although much material had been gathered and discussed, and several chapters drafted, it was clearly inexpedient to issue even a preliminary description of the downward movement until it had run a full course that could be measured; still less was it justifiable to attempt an objective analysis when not only were the factors shifting in their relative perspective and meaning in the sight of the observers, but also these observers were themselves liable to be unconsciously swayed by the changing environmental attitudes. The prolongation of the depression through 1932 has obviously made necessary the further suspension of this work, for similar reasons. It is understood ... that when the upturn of the business trend has been definitely established, some of the preliminary reports already in hand are to be revised and issued in separate monographs as Studies of the Depression. (NBER 1932 report, p.22).

After four weeks in New York Leontief reported to Schumpeter in Bonn that NBER had provided him with good research assistance and he was in a situation in which he could devote most of his time to what was most important, namely the theoretical issues. He conveyed his observations that at NBER everything was done properly but terribly

empiristic [*schrecklich empiristisch*]. Leontief had felt an immediate urge to act as His Majesty's Opposition [*Opposition S. M.*]. He told Schumpeter that he had tried to convert the office chat once a week into a small group for discussing theory, inviting also the research assistants to take an active part: "But it was almost subversive and didn't last for very long."<sup>170</sup> Leontief found enjoyment in showing NBER staff that time series, of which NBER had compiled such a large number, could be considered also from a theoretical point of view.<sup>171</sup>

NBER thus was to Leontief a good place to work but of limited scientific interest relative to his own research. He didn't seem take much interest in Mitchell's work, but he did learn more about business cycles at NBER. He may also have agreed with Ezekiel that the economists of US Dept. of Agriculture were a more inspiring and lively crowd than what he found at NBER. But Simon Kuznets was certainly an interesting person for Leontief to get to know, as indeed Ezekiel had predicted. Kuznets' work became indeed the most important achievement at NBER in the 1930s. Leontief had read Kuznets' monograph on "secular movements in production and prices" (Kuznets 1930) in the spring of 1930 and learned a lot from it. In 1934 he reviewed Kuznets (1933) on seasonal variations (Leontief 1934c). Kuznets most important publication in the 1930s was, however, the monograph on national income 1929-1932 (Kuznets 1934), on which he worked during Leontief's visit. Leontief would make extensive use of it for *The Structure of American Economy* (Leontief 1941) but not for the first input-output table he compiled (Leontief 1936a), as the table was for 1919, a year not covered in Kuznets national income monograph. For that table Leontief benefited instead from the work of another NBER researcher, Willford I. King's *The National Income and its Purchasing Power* (King 1930). King had been on the original research staff of NBER but had left before Leontief's arrival.

Leontief enjoyed New York. At Leontief's request before he left Kiel, Schumpeter had sent him suggestions for people to meet in New York. In addition to Mitchell and Kuznets at NBER, Schumpeter had suggested John Maurice Clark and Ralph Souter at Columbia.<sup>172</sup> He enclosed with the letter to Leontief a greeting card for Henry Moore whom Schumpeter described as suffering and withdrawn. Moore had retired in 1929 and Leontief didn't get to see him.

At Columbia Leontief instead met Harold Hotelling who was an outlier at Columbia at that time as the only non-institutionalist. Hotelling had indeed been hired to take over

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<sup>170</sup> Rosier (1986, p.83, transl. by ob).

<sup>171</sup> Leontief to Schumpeter, 7 Oct. 1931.

<sup>172</sup> J.M. Clark was a leading institutionalist but carried with him also parts of the neoclassical legacy of his father, J.B. Clark. Both had been invited charter members of the Econometric Society. R. Souter was from New Zealand and at Columbia 1930-35.

Henry Moore's courses in mathematical statistics and mathematical economics.<sup>173</sup>

Schumpeter had furthermore mentioned Carl Snyder at the Federal Reserve Bank of New York and also the blind Mr. Thomas, whom Schumpeter held to be one of the most gifted economists of the day. Leontief visited the Fed and met Thomas and several other young economists.

In the beginning of December 1931 Leontief explained to Schumpeter what his ongoing work was:

“The basic idea of my current work is the following. The ‘empirical’ explanation of economic changes must start with the isolation of primary changes from secondary ones, which can be regarded as the response of the total system. It can theoretically (mathematically) be shown that when an essential change, a ‘new combination’ i.e. an elasticity shift, occurs in any point of the economic circulation, then this will call forth in all other and particularly in the ‘neighboring’ parts of the system quite specific shifts (both in size and direction) of the supply and demand curves.

If the elasticities of the individual markets are known, then the level shifts can be measured. And if the latter are determined for the ‘strategically’ important points of the circular flow, then deductions can be drawn from their configuration about probable location of the primary change at stake. The results I have achieved so far show that the task is not hopeless.” (Leontief to Schumpeter, 15 Dec. 1931, transl. by ob).

This is an informative passage in view of Leontief's earlier work and our knowledge of where he was heading, although disappointingly brief. The keyword in the description was “economic changes.” Leontief was clearly struggling with a theoretical framework of general equilibrium character, comprising several industrial markets, not necessarily covering the entire economy. The commodities were dealt with in terms of supply and demand curves, carried over from the *Versuch* and *Eisenaufsatz* papers, characterized by “structure”, conceived as an (autonomous) elasticity, and “shifts” or “level movements”, induced by changes in “neighboring” markets (or in later terminology by forward or backward linkages). It is hardly correct to denote Leontief's analytical structure in 1931 as an early version of the input-output framework. Possibly it could be denoted as a precursor; it is any case a significant indication of the direction in which he was heading.<sup>174</sup>

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<sup>173</sup> Rutherford (2004, p.50).

<sup>174</sup> But notice the opening of the preface of Leontief (1941): “Preliminary research for this investigation was concluded in 1931 while I was holding the position of Research Associate in the National Bureau of Economic Research.”

Leontief had joined Econometric Society while still in Europe. In New York he joined the American Economic Association (AEA) and at the end of December 1931 took part in the joint meetings of the social science associations in Washington D.C. He registered as an AEA member and in a letter to Schumpeter shortly afterwards he referred to the meeting, perhaps with tongue-in-cheek, as a trade union congress [*Gewerkschaftskongress*]. It was his first economist meeting in USA and he was positively impressed, not suppressing that it would have been a better meeting if the poorer papers had been rejected. The joint meetings comprised also an Econometric Society meeting, the first ever on American soil. Irving Fisher, President of the Society, was present and had taken upon himself to write the official report from the meeting.

Leontief had not submitted any paper but had accepted to open the discussion of two papers in AEA sessions. One was by Ezekiel on the relationship between price analysis and mathematical economics, the other by Henry Schultz “on shifting demand.” It was the first meeting with Schultz for Leontief who had his sharp criticism of the *Versuch* paper fresh in mind. Leontief had not heeded Schumpeter’s advice of responding to Schultz, because the Kiel Institute leadership discouraged him. He reported to Schumpeter about the meeting with Schultz that “I was most happy thoroughly to settle accounts with Schultz. Now we have made eternal peace.”<sup>175</sup>

He added a miniscule remark about how his work was going: “My work is like a Hydra. In the place of one resolved problem, two new ones immediately emerge. But it is fun.” He had also looked into the capital accounts of NBER and drawn the conclusion that the capital funding had shrank to an extent that seemed to rule out any hope of extended employment with NBER.

Leontief’s year at NBER became much affected by Harvard’s interest in him. During the year with NBER he received from Harvard University an offer of a teaching position from the next academic year, with prospects for a professorship, as discussed below. The offer from Harvard may well have distracted Leontief’s attention away from NBER. He worked on his own and sat out his time.

After meeting Arthur Marget at the AEA meeting in Washington Leontief was invited to give a lecture or two at the University of Minnesota. Leontief proposed a lecture with the same title as the one he prepared for Harvard, “The Positive and Normative Approach in Economic Theory”. Marget involved Alvin Hansen who proposed also a second lecture on Leontief’s current research, suggesting the title of “Dynamic Economics”. Leontief went to Minneapolis in February 1932 and gave the two lectures. As he was pressed for

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<sup>175</sup> Leontief to Schumpeter, 6 Jan. 1932.

time he travelled between New York and Chicago by air. It earned him at NBER the nickname “the flying economist”!<sup>176</sup>

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Leontief’s only obligation at NBER was really to submit a manuscript at the end of his sojourn for publication by NBER. It seems to be an indubitable fact, however, that such a manuscript, was never submitted. At the end of 1932 he submitted from Harvard a brief one-page report on the work done outlining the content of the manuscript he promised as forthcoming:

“The project is devoted to a statistical analysis of shifts in demand and supply during the business cycle. The first part of the study contains a discussion of fundamental concepts, especially the relation between the (Marshallian) supply and demand curves and changes in the general equilibrium of the whole system. The following chapter will contain a detailed description of the statistical technique which is subsequently – in the third part – applied to the empirical analysis of some twenty-five American commodity markets over a period of seven to thirteen cyclical waves. In the concluding section, I will sum up the results in a comparative study of cyclical shifts.” (Leontief to Martha Anderson, NBER, 9 January 1933.)

This was an ambitious outline with no suggestion of input-output tables. The project included topics not included in Leontief (1936, 1937). It stated an ambition of covering an impressive range of American commodity markets over a considerable time span with supply and demand curves embedded in as general equilibrium structure. The content of the outline can be read as related to the account he gave Schumpeter in December 1931 (see above). Not mentioned to Schumpeter was, however, that cyclical waves would be studied with an emphasis on cyclical shifts. Does this informal report lend support to the assertion that Leontief had developed the essentials of input-output analysis at NBER (Carter 1976, p.57)? It seems rather as if Leontief was working on something more intricate, both theoretically and empirically. Perhaps it appeared differently in Leontief’s retrospective view.

The NBER 1932 report stated that Leontief’s statistical analysis of elasticities of demand and supply in selected American commodity markets had been delayed but was expected before the mid-year 1933. Mitchell wrote to Leontief that he had heard from Edwin Gay that Leontief’s manuscript would be submitted by 1 August 1933: “May we

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<sup>176</sup> Leontief to A.W. Marget, 14 Jan., 20 Jan., 5 Feb., and 19 Feb. 1932; A.W. Marget to Leontief, 18 Jan. and 25 Jan. 1932.

count upon receiving it?” Leontief confirmed that he expected to be able to submit a complete manuscript by the first of August but that doesn’t seem to have happened.<sup>177</sup>

One year later the NBER 1933 report reiterated that the manuscript had not been submitted but without any indication of when it could be expected. The difficult situation in NBER led to some changes in the annual reporting. At the end of 1934 a report for 1934-1935 comprised a list of papers expected to be ready for publication in 1935, one of which was “Dr. Wassily Leontief’s analysis of the shifts in the demand for and supply of commodities during business cycles.”<sup>178</sup> In the annual report at the end of 1935 it was mentioned that among publications from former research associates in prospect was *Cyclical Shifts in Demand*, by Wassily Leontief.<sup>179</sup>

NBER was at that time a somewhat monolithic institution. The publications appeared under the authors’ names but they were above all NBER publications, corroborated and approved by the director of research, i.e. by Mitchell. In 1935 this stringent system of approving publications was reconsidered with the specific aim of reducing some of the burden on Mitchell. A memorandum was issued outlining the new procedures. Leontief was told by letter that a committee consisting of Frederick Mills and Solomon Fabricant had been named to cooperate with him on the preparation of the report on laws of demand and supply. There was no formalized procedure for the work of these committees, “but you may now be sure that there are two people here at the Bureau who will be particularly interested in seeing any results of your study as a manuscript becomes available.”<sup>180</sup>

The 1936 annual report comprised the following paragraph:

*Shifts in Demand and Supply*

Wassily Leontief is carrying out certain analyses of theoretical equations reflecting price-quantity relations for comparison with the empirical results obtained when he was a Research Associate at the National Bureau. It is thought that his manuscript on shifts in demand and supply during business cycles will greatly benefit from his extended work on the subject. (NBER 1936 Report, p.16).

That was the last mention of Leontief’s unsubmitted manuscript in the annual reports of NBER. Mills and Fabricant must have drafted the paragraph after contact with Leontief. The legendary aftermath of Leontief’s brief sojourn in NBER is that NBER had brought

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<sup>177</sup> Mitchell to Leontief, 27 April 1933.

<sup>178</sup> NBER 1934 Report for 1934-1935, p.26.

<sup>179</sup> NBER Retrospect and Prospect 1920- 1936, pp.15-16. Leontief published at this time an article with a similar title in *Review of Economic Studies* (Leontief 1935), but it was not submitted to NBER and escaped the scrutiny of its director of research.

<sup>180</sup> C.A. Bliss, exec.secr. of NBER to W. Leontief, 17 January 1936.

Leontief to USA and that the year at NBER had been important in re-orienting his project to a more promising path of inquiry.

As such it was celebrated at the fiftieth anniversary dinner of NBER on 27 February 1970 at the Starlight Roof of the Waldorf-Astoria Hotel in New York with Leontief was a guest of honor as the incumbent president of the American Economic Association. Walter Heller was emcee and spoke of NBER's "most significant contributions of all, to American economics, namely Wassily Leontief, whom the Bureau brought to these shores as a Research Associate in 1931. Indeed, it was Simon Kuznets who went to Ellis Island to greet him."<sup>181</sup>

Leontief paid homage to NBER in his speech on this occasion, ending by a note on how his personal life had been influenced by his stay at NBER in New York:

"Thirty-eight years ago when I was escorted off Ellis Island by Professor Kuznets, and brought directly to the National Bureau, it was, of course, located like all the good businesses and important institutions downtown. I remember very well that it was Madison Square. However, there were two insurance companies on Madison Square; one was the Metropolitan Life and the other the New York Life. Was the National Bureau located in the Metropolitan Life or New York Life building? You see, this proved to be a more portentous question for me than you might think. Soon after my arrival I met a girl I liked very much. We agreed to meet again in the lobby of *my* building, but I forgot which of the two insurance companies this was. We didn't meet that evening since the girl was waiting for me in the New York Life when I was in the Metropolitan. But an economist cannot be put off by a small factual error; the girl now sits a few tables from me among you."<sup>182</sup>

The "girl" was Estelle Marks Leontief, married to Wassily Leontief since 26 December 1932.

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<sup>181</sup> Some 40 years later Simon Kuznets and Wassily Leontief celebrated as Harvard colleagues each other's Nobel Prizes in 1971 and 1973, respectively.

<sup>182</sup> NBER Fiftieth Anniversary Dinner, transcript of proceedings, p.10.



## Enchanted at Harvard

Leontief's passage from being research associate at NBER to becoming professor at Harvard happened quickly and smoothly. It may also seem surprising that it went so smoothly as Leontief after all had never held a university position and as he lacked *Habilitation* he was not even qualified for one in Germany, and he had never published anything in English. Leontief accepted an offer from Harvard in March 1932, less than half a year into his one-year contractual period with NBER. The offer was for an instructor position from the coming academic year 1932/33 but it came with extra benefits, a promise of an assistant professorship within a year, and prospects for even more. Even at Harvard, there was as in Kiel and at NBER uncertainty due to the financial crisis, but only a touch. The offer Leontief accepted initiated a period of more than forty years as a professor at Harvard. But even after accepting Harvard's offer Leontief he had not burnt the bridges for a return to Germany.

But how and why did he come to Harvard's attention so soon after arriving in USA? Did Schumpeter play a role behind the scene? Why was Harvard so eager? In general there was a premium at leading American universities on talented economists from Europe. But were there more specific reasons for Harvard's interest. Who provided the president of Harvard with an assessment of Leontief as a good prospect to buy into?

With regard to how Leontief came to Harvard's attention, a pertinent fact is that after two-three weeks in America Leontief traveled from New York to Cambridge and spent three days at Harvard University. He was certainly not invited by Harvard University and he hardly had any acquaintances among the economists there. He had been in correspondence (barely) with W.L. Crum, who impressed with *Versuch* and was one of his references for the position at NBER, and with Elizabeth Gilboy who had written a review paper about the methods of the *Versuch* treatise. But it seems remote that either of them could have been involved. More likely does it seem that Edwin Gay, co-director of research with Mitchell at NBER when Leontief arrived and also professor of economic history at Harvard, invited the bright new research associate for a brief visit to Harvard.

From Leontief's account to Schumpeter of his meeting with Harvard in the beginning of October 1931 he was almost overwhelmed by the welcoming gestures he received from the new tribe he had become acquainted with. He seemed quite intrigued by the cordiality he was met with. Leontief's observations of and reactions to Harvard were conveyed in letters to Schumpeter in Bonn from October 1931 to April 1932. On his return to New York from Harvard he wrote to Schumpeter: "All the economists are exceptionally kind to

me. That is really a place where one can do real work.”<sup>183</sup> He was asked informally whether he after Christmas would give a lecture on methodological issues.<sup>184</sup>

Two weeks later Schumpeter responded with delight about Leontief’s success in establishing a position for himself in USA: “The beauty of America, is that a talent and new ideas are not met with obstinate hostility [*verbissener Feindschaft*] but on the contrary with positive interest.” Of course Leontief should accept to give a lecture at Harvard and if a further offer should follow, it should not be turned down, only postponed to allow you to spend a year in Germany first, as “it is after all your wish to work in Germany.”<sup>185</sup> In Schumpeter’s view Leontief’s prospects in Germany had been a little improved. If it nevertheless didn’t work out in Germany, Leontief could go back to Amerika. Schumpeter was thus rather insistent on exploring opportunities in Germany. Schumpeter also argued that if Leontief were to choose USA, New York, i.e. NBER, would be a better choice than Harvard, at least if Leontief succeeded in convincing the NBER staff to pay more attention to theory. Although we cannot rule out that Schumpeter had mentioned Leontief to someone at Harvard it seems unlikely that he was involved in any plot to recruit him.

A further remark here about Schumpeter’s situation may be in place. Schumpeter had hardly explained to Leontief his intricate maneuvering for a suitable professorial chair to move to after Bonn, where Schumpeter had held a chair in public finance since 1925 but had his reasons for leaving.<sup>186</sup> More than anything else he wanted a chair at Berlin University but was, actively opposed by some members of the Berlin faculty, mainly for non-scientific reasons. His main foe was Werner Sombart who had retired in 1930 but actively campaigned to prevent Schumpeter getting a chair in Berlin.<sup>187</sup> It ended with defeat and frustration for Schumpeter in 1932.

But Schumpeter had a fallback option. He had spent one year as a visiting professor at Harvard in 1927/28. This happened after the Harvard Economics Department had wanted to invite for a visiting year an “outstanding man,” which tacitly meant an “outstanding

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<sup>183</sup> Leontief to Schumpeter, 7 Oct. 1931, transl. by ob.

<sup>184</sup> Another possible lead to why Leontief turned up at Harvard so soon after arriving in USA is Pitirim Sorokin, his Petrograd teacher of sociology. Sorokin had continued to work for the SR at its head quarter in Prague after the Bolshevik coup and had become professor at Harvard just a year or two earlier. But there is no evidence that Sorokin played any role and according to Svetlana Alpers the relationship between Sorokin and Leontief (Sr. and Jr.) was not close enough to make this likely.

<sup>185</sup> Schumpeter to Leontief, 24 Oct. 1931, transl. by ob.

<sup>186</sup> See Swedberg (1991, pp.19-21).

<sup>187</sup> Stolper (1994, pp.319-320) discusses this event and points out that Emil Lederer, who had recently taken over Sombart’s chair, was the only faculty member in Berlin who supported Schumpeter’s honor and scientific importance, but to no avail. Swedberg (1991, pp.21-22) asserts that the chair Schumpeter wanted was the one given to Emil Lederer, but Stolper’s version seems to be correct here.

European man.” A vote was taken over proposals put forward and Schumpeter won handsomely over Gustav Cassel and Edwin Cannan.<sup>188</sup>

Schumpeter’s erudition, charisma and social skills made an impact on Harvard leaders, not least President Lowell and the new economics department chairman Harold H. Burbank. They were concerned about finding a way of luring Schumpeter back to Harvard. The sudden death of Allyn Young at 53 years of age in 1929 led President Lowell to offer Schumpeter a chair at Harvard. Schumpeter knew how to play his hand and suggested to President Lowell that he could meet Harvard halfway, enabling “...me to take some share in Harvard’s great work whilst keeping up part of my duties here”. Burbank then took over and offered Schumpeter to have a visiting position for half a year every year, an offer which Schumpeter agreed to. Under mild pressure from the Prussian Ministry of Education which supervised the Prussian universities Schumpeter did not go to Harvard in 1929 but came for the fall term 1930. After Schumpeter’s failure in getting a chair in Berlin he approached Harvard about a permanent position. After some negotiation the outcome was that Schumpeter moved permanently to Harvard.<sup>189</sup>

Thus at the time of Schumpeter’s letter to Leontief in October 1931 quoted above, Schumpeter was still hoping to get the chair in Berlin and hoped that Leontief was heading for a career in Germany after his fling with Harvard.

When Leontief wrote to Schumpeter again on the middle of December, he was at Harvard on his second visit. He could tell Schumpeter that it had been decided that he would address the Harvard Economic Seminary in the spring term. He had decided on the title, “Positive and Normative Approaches in Economic Theory,” and worked on it in a history of economic thought perspective. It had been suggested to him that the lecture could be published in the *Quarterly Journal of Economics*.<sup>190</sup> He would speak about his current research in a separate lecture, he referred to it as “elasticity matters” (*Elastizitätsangelegenheit*),

Leontief emphasized again that he liked to be at Harvard, comparing it with his impression of Columbia University which he had visited a couple of times. At Columbia he had found the ambience strange and unsympathetic while at Cambridge he had felt comfortable and secure.<sup>191</sup> But Leontief also vaguely felt Harvard that nurtured some far-reaching intensions involving him. At the visit in December 1931 he heard rumors that

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<sup>188</sup> Swedberg (1991, p.23).

<sup>189</sup> See McCraw (2007, pp.193ff).

<sup>190</sup> Leontief to Schumpeter, 15 December 1931, transl by ob. The seminar lecture was given (in March 1932) but it does not seem to have been published.

<sup>191</sup> Leontief to Schumpeter, 15 December 1931, transl. by ob. The difference in mood at Harvard and Columbia was well known: “...Columbia was impersonal and not very collegial in character. Faculty and students did not socialize much. One does not get the feeling ... that creating a band of faithful followers was ever part of the Columbia ideal.” (Rutherford 2004, p.70).

Schumpeter possibly would move to Harvard but the negotiations between Harvard and Schumpeter were hardly finished at the time. Schumpeter had various concerns in Germany which needed to be settled before he was ready to leave Europe for good. It is likely that Schumpeter at this stage told Burbank that Leontief had talents that Harvard needed and perhaps also that Schumpeter would much appreciate to have him as a colleague if the outcome became that he moved to Harvard. Leontief added to Schumpeter that if the rumors of Schumpeter's transfer to Harvard were true, "then my plans would fundamentally change ...".<sup>192</sup> Germany would then no longer be an option worth pursuing.

Leontief met Burbank at his second visit in December. Three weeks into 1932 Burbank wrote to Leontief in New York:

"There are a number of matters I had intended to discuss with you at the time of your prospective visit to Cambridge, but I find that it may be advantageous to consider some of them immediately.

Very frankly, we are much interested in the possibility of having you become associated with us. I had expected that in March I could talk these matters over with you leisurely and definitely, but I now find that it seems necessary to begin abruptly and indefinitely." (Burbank to Leontief, 21 January 1931).

Thus Burbank let out of the bag what Leontief had suspected for a while, namely that the Harvard people were out to recruit him. This was good news, given the uncertainty of employment prospects in Germany. But there was a contingency as the American universities were also feeling the depression acutely:

"Harvard is affected less than most, but affected nevertheless. Exactly what we shall be able to do during the forthcoming year is still a question and will remain so for some weeks. In the meantime I wish very much to proceed with the attempt to make arrangements if you are interested." (Burbank to Leontief, 21 January 1932).

Burbank's message was clear. He wanted to push on with the recruitment of Leontief but the financial uncertainties caused by the depression constrained his room for maneuver. Burbank continued by asking Leontief straight out whether he would care to become associated with the economists of Harvard. If so, he should to send his cv to Burbank as soon as possible. Burbank assured Leontief that Harvard was familiar with his work and interest in economic theory. And he added a surprisingly specific question, namely whether Leontief was sufficiently interested in International Trade to undertake instruction, for graduates in that subject.

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<sup>192</sup> Leontief to Schumpeter, 15 December 1931, transl. by ob.

Leontief had not yet published a single paper in an English language journal. The “familiarity” with Leontief’s interest in economic theory must have come from what Schumpeter had told Burbank, or from the few Harvard people who had studied the *Versuch* treatise, the only paper of importance Leontief had published so far. Had Burbank acquired over the grapevine impressions of Leontief from NBER, from Kiel or even from the AEA meeting in December 1931. Burbank was in any case convinced that Leontief was a brilliant candidate and had qualifications that Harvard particularly needed.<sup>193</sup>

Burbank’s question about Leontief’s interest in teaching international trade may have reflected an acute lacuna in the teaching plan. Leontief confirmed his interest in international trade and was soon after working on his first paper in English, on indifference curves in the analysis of foreign trade (Leontief 1933). The impulse for embarking on it is very likely to have originated in Burbank’s question.

Burbank’s letter of 21 Jan. 1931, in which he apologized for having been so “indefinite” when they met in December, was followed by a flurry of telegrams to Leontief about details of Harvard’s offer. Leontief continued to keep Schumpeter updated about his talks with Harvard and the situation soon became more definite. Harvard offered a lecturer position for 1932/33, possibly to be converted into a professorship one year later. The teaching obligations were been stated as comprising “Modern Economic Theory” and “Price Analysis” to be given as full courses and an additional half-course on selected topics in international economics. Leontief commented to Schumpeter: “A bit much, but quite enticing!” [*Etwas viel, aber recht verlockend*]. He was promised an assistant for his own research and the salary would be \$3600, more than the average instructor.

Harvard wanted an immediate commitment from Leontief but he wanted Schumpeter’s opinion before responding. He asked Schumpeter: “Would my prospects in Germany (if they still exist) deteriorate?” But was this tongue-in-cheek or an indication that Leontief had not yet finally given up Germany? At this point Leontief had become convinced that

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<sup>193</sup> This may as good place as any other for a brief remark Leontief’s Jewishness (or lack of the same). One may wonder whether the extraordinary attention given to Leontief by Burbank would have been different if he had known that Leontief was the son of a Jewish mother. Burbank’s anti-Semitism is a well-established fact, not least from Paul Samuelson’s labeling of Burbank as “anti-Semite no. one in Harvard’s department of economics” (Backhouse (2014, pp. 61-62). McCraw (2007, p.202) writes that “Harvard was making every effort to entice Wassily Leontief, a brilliant young Jewish economist from Russia.” But it was hardly the case that Leontief was ever spoken (or thought) of as a “Jewish economist” or unimaginable that he thought of himself in such terms. Leontief’s status wrt. Jewishness was that from an orthodox point of view he was and would always remain a Jew. But as an immigrant to the United States he was not Jewish. I thank E. Roy Weintraub for this clarification.

the offer from Harvard was largely due to Schumpeter's influence and thanked him sincerely (but still unaware whether Schumpeter would move to Harvard).<sup>194</sup>

The hiring of Leontief was dealt with also by the President Lowell. This was done without sufficient coordination with Burbank. Edwin F. Gay who held a high opinion of Leontief, had advised and Lowell accepted to appoint Leontief as Assistant Professor. Burbank adhered to administrative guidelines that due to financial difficulties additions to staff should be avoided wherever possible, and hired Leontief as instructor. Burbank later apologized to Leontief for his confused handling of the matter. He made amends by telling Leontief that

“...there is an unusual opportunity in the department for young men of high ability. We have a group of distinguished senior professors who are now well advanced in years. As time takes its toll the opportunities for advancement in this department will be many.”<sup>195</sup>

In retrospect it is obvious that Harvard at the time had great need for gifted economists, not least of the rare breed of mathematical economist with strong empirical inclination, as Leontief indeed was. Leontief's arrival was indeed as fortunate for Harvard as manna from heaven.

In the middle of March 1932 Leontief went to Harvard for the third time. He confirmed that he accepted the offer. The official appointment did not arrive until two months later. Leontief made preliminary preparations for his forthcoming relocation from New York to Cambridge.

Soon after his return to New York he provided more details to Schumpeter. The contract he had signed was for three years. He would have his own full-time assistant, a proper office in Holyoke House and an additional work room in Widener Library. After one year he would start tutoring gradually and also got the right to choose his tutees. Burbank had given enlightening remarks on the courses assigned to him. Leontief sent Schumpeter the notes he had taken and repeated: “The goodwill of the Harvard people toward me seems to have no limit.”<sup>196</sup>

The main purpose of the third visit was to give the lecture to the Harvard Economic Seminar.<sup>197</sup> The Economic Seminar on Monday, 14 March 1932 was the first time

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<sup>194</sup> Leontief to Schumpeter, 2 February 1932.

<sup>195</sup> Burbank to Leontief, 1 March 1932.

<sup>196</sup> Leontief to Schumpeter, 17 March 1932, transl. by ob. He ended his letter as follows: “*In Harvard werden Sie mit grosser Ungeduld erwartet ...*”

<sup>197</sup> The Economic Seminary met every second and fourth Monday each month, see <http://www.irwincollier.com/1961-2/>

Leontief spoke to an audience at Harvard and it became a busy day. In addition to the seminar he also gave two lectures in an undergraduate course on demand and supply elasticities, and a dinner speech at the end of the day.

During the dinner Leontief got to know that Schumpeter would move permanently to Harvard. One month later Schumpeter confirmed it and also that he would arrive in September. He was delighted at the prospects of having Leontief as a colleague. Leontief was even more delighted: “The possibilities are so open and the prospects so great that I cannot yet fully believe that this is really so.”<sup>198</sup>

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In the summer of 1932 Leontief left for Europe on 29 June and returned after one month after having spent much of the time in Austria with his parents. In 1931 Leontief had met Estelle Helen Marks in New York. She worked as a piano teacher at the time and was raised in a family with socialist leanings. Leontief once referred to his father-in-law, a lawyer, as “an old-time Debs Socialist.” Leontief surely wanted to tell his parents that they intended to marry later that year, which they did in December 1932.<sup>199</sup> Shortly after his return from Europe he was a faculty member at Harvard.

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An early initiative by Schumpeter at Harvard was to propose a course in mathematical economics. Schumpeter offered to teach such a course himself, at least to get it started. He channeled it through Crum for approval. Crum took it up with Burbank who was very responsive to Schumpeter’s suggestion. A committee was appointed to provide a concrete proposal for a course; it comprised H. H. Burbank, E. H. Chamberlin, W. L. Crum, E. S. Mason, J. A. Schumpeter, and F. W. Taussig.

The committee agreed that the course was not meant for the advanced student who was interested in the mathematical aspects of economics and already had some mathematical training, but rather for the wider circle of less advanced students and their need to master the fundamental concepts of mathematics necessary to understand, e.g. Marshall’s Appendix and important parts of mathematical economics, such as the works of

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<sup>198</sup> Leontief to Schumpeter, 15 April 1932, transl. by ob.

<sup>199</sup> A birth certificate was issued by on October 5, 1932 by Standesamt der Landshauptstadt München showing that Leontief was born on 5 August 1905 in Munich. It must have been requested by Leontief or Leontief Sr., probably due to the forthcoming marriage but whether certificate was presented is not known.

Cournot, Walras, Edgeworth, and a few others. Thus the course should not be mathematics taught to economists but rather on the “mathematical theory of economics.”<sup>200</sup>

But why did Schumpeter take this initiative? The reason was undoubtedly Schumpeter’s deep involvement in the founding of the Econometric Society, a purpose of which was to promote mathematics within economics. Some would even say it was the purpose.<sup>201</sup> The Econometric Society was founded in December 1930; during 1931 the Council of the Society invited a list of about 175 names as charter members of the Society. Most of the careful approval of names on the list was done by the unofficial troika of the Council, namely Irving Fisher, Schumpeter and Ragnar Frisch. The Harvard economics department was not known for having any person with mathematical skills. When the troika in the summer of 1931 discussed by letter possible charter members from Harvard each of them had found one but only one, name to propose from the economics department. Fisher’s candidate was Thomas N. Carver, Schumpeter proposed Frank W. Taussig, while Frisch suggested John D. Black. After noting the outcome the two younger men yielded in deference to Fisher, who was exasperated at the thought of having no charter member from the Harvard economics department, and they put both Carver and Taussig on the list.<sup>202</sup> Leontief was, as mentioned above, informed shortly before he left Europe in August 1931 that had been elected as charter member.

Hence, Schumpeter’s initiative was meant to remedy the situation with regard to mathematics in the teaching of economics. The Committee proposed to start already from the fall term 1933. A course titled “Introduction to the Mathematical Treatment of Economic Theory” was offered as half-course to be given by Schumpeter “and other members of the department”. The same course was offered again in 1934 but accompanied by another half-course titled “Mathematical Economics” to be given by E.B. Wilson. From the third year Schumpeter’s course was taken over by Leontief, who had assisted Schumpeter the previous years.<sup>203</sup> Schumpeter thus saw the need to promote mathematics in economics at Harvard. Did it work? Solow observed the terrain in 1941 and asserted in a retrospective article that, “[t]he learning of mathematics was frowned upon by the Harvard economics department.”<sup>204</sup>

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<sup>200</sup> <http://www.irwincollier.com/harvard-mathematical-economics-1933-1937/> and Swedberg (1991, p.23).

<sup>201</sup> See Bjerkholt (2017 forthcoming).

<sup>202</sup> See Bjerkholt (1998, p.41). Harvard had, however, an outstanding charter member in Edwin Bidwell Wilson, professor at Harvard but not in the economics department.

<sup>203</sup> See <http://www.irwincollier.com/harvard-mathematical-economics-1933-1937/>

<sup>204</sup> Solow (1998, p.300). Solow added: “In 1945, Wassily was teaching the first-year graduate course in economic theory, as well as a more advanced graduate course in mathematical economics. These were...the only courses at Harvard in which economics was treated rigorously.”



Another Schumpeter initiative soon after arrival at Harvard and much appreciated by Leontief was an informal group which dined once a month and called themselves “The Seven Wise Men.” In addition to Schumpeter and Leontief the members of the original group were Edward Mason, Edward Chamberlain, Overton Hume Taylor, Seymour Harris, and Douglas Brown. It was Leontief’s idea to make a joint effort to compile funds to establish a wine cellar. The group wrote an “anti-New Deal” book, published in 1934; Leontief’s contribution was a chapter in agricultural economics.

### **The Harvard Committee on Research in the Social Sciences**

Harvard had a Committee on Research in the Social Sciences. Harvard faculty in the social sciences could apply for grants for research assistance, primarily for conducting empirical investigations. Not many at Harvard Faculty exploited the opportunity, perhaps not more than five or six each year and often the same persons in successive years. During Leontief’s third visit to Harvard in March 1932 he was told about the Committee, and as he would be Harvard faculty from the forthcoming academic year, he was invited to apply.

The secretary of the Committee was Elizabeth Waterman Gilboy whom Leontief knew from his last years in Germany as a critical, but positively inclined, reviewer of his work on demand and supply analysis. He would be in frequent touch with her as secretary of the Committee for the next ten years, as he applied for and received grants from the Committee. Gilboy went out of her way to smooth out problems that arose in this process.<sup>205</sup>

The Committee practiced a simple system for doling out grants for research assistance to social science faculty. Application for grants from the Harvard Committee had to be submitted by February for the forthcoming academic year. Applications were submitted on a two-page form with of a small number of entries; no further project documentation was normally required. It was a semi-formal procedure. A brief report on the outcome of the project was expected at the end of the academic year.

The key entries on the form were the name of the project (usually given as descriptive phrase) and an outline of what it was about. Other entries on the form were about the kind and cost of assistance required, data needs, publication plans, etc.

The applications and reports submitted by Leontief to the Harvard Committee make up an interesting source about his research in the 1930s. The early applications were formulated within the paradigm he had developed in Germany. But at some points the input-output paradigm emerged out of this process. There are limitations as to what can be read out of the applications as the information is brief. Applications might have been discussed informally with the Committee being recorded.

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<sup>205</sup> Gilboy later became the administrative director of Leontief’s Harvard Economic Research Project (HERP) from 1948.

In the *Eisenaufsatz* Leontief had become increasingly concerned with “vertical” linkages between individual commodities, such as the dependence of the supply of iron upon the supply conditions for the major inputs of iron production, such as iron ore and coke. By pursuing such linkages further back the analytic framework was extended to comprise interrelations between an increasing set of industries producing and consuming commodities. The partial equilibrium approach of the *Versuch* was broadened to encompass the joint equilibrium of several markets and industries. The general equilibrium structure that emerged was pursued it on the assumption that the equations supporting the structure could be solved mathematically.

Thinking about how to resolve the general equilibrium Leontief conceptualized it as a simultaneous system in which primary changes took place. He spoke of “the isolation of primary changes from secondary ones, ... as the response of the total system.” The primary changes would through the interdependencies of the system cause economic changes in the entire system. Problems that could be posed comprised to study how primary changes spread through the system, or conversely, from a disturbed system identify the primary change(s). The archetypical kind of primary changes considered was a change in a supply or demand elasticity.

Leontief’s conceptual system thus comprised demand and supply curves with linkages between markets. The relationships were not linear, the comfortable linearity of the *Versuch* could not be preserved. Business cycles were attempted taken into consideration. Nothing is really known about the mathematical structures Leontief must have played around with in this period. A remark about the need to study *Determinantentheorie* in one of the letters to Schumpeter suggested structured simultaneous equations systems. These could, however, easily grow out of hand with regard to numerical solvability with the available computational equipment. At the same time it was vital for Leontief to have his work firmly anchored in empirically. He kept his feet firmly anchored so to say in economic statistics.

In the following we proceed step by step through the application from 1932 to 1937.

### **The 1932 and 1933 applications: the emergence of the first input-output table**

As soon as Leontief got back to New York from Harvard in March 1932 he submitted the first application to the Harvard Committee. The title was stated as “Statistical analysis in economic dynamics.” As background information the application stated that the prospective study was “... based on my theoretical and statistical analysis of demand and supply” which had been initiated at the Kiel Institute and “which I am now carrying further as a research associate of the NBER.”

The outline was as follows: “The study is intended to be conducted on two parallel lines: (a) Theoretical analysis of the mechanism of economic changes; (b) an empirical (statistical) research based on the foregoing theoretical analysis.”

The logic expressed here about empirical research in relation to theoretical analysis was entirely in line with Leontief’s idea of empirical research. It was noted above how this was set out in the introduction of the *Konzentration* paper, and as demonstrated by the *Versuch* treatise with empirical analysis conducted within a strictly defined theoretical framework. Or as stated in the application, the empirical research would be “based on the foregoing theoretical analysis.” The theoretical analysis was about “the mechanism of economic changes,” an expression Leontief had used in his communication with Schumpeter as changes caused by disturbance of a state of general equilibrium. But the written application offered no information about the intended empirical research.

Leontief requested in the application an assistant with a good theoretical and practical statistical training and who “... must be interested in theoretical economics.” He left the estimated cost of assistance blank; Elizabeth Gilboy penned in “Probably \$1500” which is what he got. It was stated that more detailed information about the prospective study had been given in “my conversations with Mrs. Gilboy and Professor Crum”. The Harvard Committee, after approving Leontief’s application, recruited for the project a research assistant named Maynard Heins. He was a bright and hard-working guy, suitable for the job and apparently much appreciated by Leontief.

In March 1932 Leontief announces his first theoretical research topic at Harvard as the “theoretical analysis” of part (a)? But what was it about? The application gave no outline of the theoretical structure, only that it was about the “mechanism of economic changes” which was a term used in the exchange with Schumpeter. Can we consider this announced project as the prototype of input-output? Hardly, Leontief’s theoretical paradigm for a general equilibrium system anno 1932 was different and more intricate than the what would become known as the *closed Leontief model* as set out in Leontief (1937). By the summary descriptions Leontief conveyed to Schumpeter the general equilibrium structure Leontief had in his mind were individual markets represented by supply and demand curves as in *Versuch* but interacting because they were neighboring markets influencing each other, e.g. through a vertical chain. The prime example of an economic change was a shift in a supply or demand elasticity.

But the key idea and ambition of calculating the reconstituted general equilibrium after an “economic change” was retained. Leontief (1937) showed that the impact of a change in any structural coefficient on the overall equilibrium could be calculated given the input-output equations.

It is known from archival information that Leontief completed the theoretical analysis paper he proposed in the application and submitted it to *Econometrica* as “Economic

changes and general equilibrium.” It did not lead to publication, unfortunately, and is further discussed below.

As noted the application left the empirical research rather undefined. But what was initiated on the basis of the grant approved for the application can in retrospect nothing less than the first leg of compiling an input-output table for 1919, as eventually published in Leontief (1936). The application might thus be viewed as the beginning of input-output economics.

In June 1933 Leontief reported that the analysis of “cost, output and distribution of products” had been extended since the beginning of the investigation to cover the following industries:

“Iron Ore, Iron and Steel, Machinery, Non-ferrous Metals, Coal and Coke, Petroleum and Natural Gas, Textiles, Wearing Apparel, Leather, Chemicals, Paper, Forest Products, Agricultural Products, Foods and Kindred Products, Non-metallic Mineral and products, and Rubber.”

The 1919 census material provided only a starting point for the compilation, “the larger part of the work was devoted to the gathering of additional data ... the available statistical information falls short of our ultimate requirements and consequently frequent recourse to indirect computation and evaluation becomes necessary.” The report’s summary up of what had been achieved was as follows:

“The analysis of separate industries is leading toward the final goal of this investigation, a quantitative description of the interrelations between the different branches of the national economy of this country. The production data are linked after an adjustment for imports, exports, and other industries and the final distribution statistics of the finished good.”

The report clearly described an input-output table in the making. There is a notable confident tone in the very brief report from June 1933, reflecting perhaps a belief that the “quantitative description” of which the size and specifications had not been stated, was achievable.

Leontief’s research philosophy was that the empirical research should be based on a prior developed theoretical structure. That didn’t work out here. In the case of input-output it may seem as if the empirical research, i.e. the input-output table, took off and preceded the theoretical analysis!<sup>206</sup>

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<sup>206</sup> Leontief recapitulated his research philosophy in the first paragraph of Leontief (1937), describing an investigation as comprising three distinct tasks: (1) formulation of an appropriate

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One year later, in February 1933, a new application was submitted titled: “Statistical analysis of the interrelation of industries and the circuit flow of goods.”<sup>207</sup>

The outline of the project was the continuation and completion of the empirical project for the previous year, which we can identify with hindsight as an input-output table for 1919 but this was not apparent from the application. There was in the application a striking absence of any notion of what was being prepared.

The application stated that the project would be completed in the spring of 1934. That turned out to be too optimistic but the compilation and organization of data was largely nevertheless an impressive performance.

Leontief, clearly satisfied with the work of his research assistant, expressed in the new application that the further participation of Maynard Heins “in the completion of the work would be very desirable.”

The 1933 application stated that the major part of work in 1933/34, would be “...devoted to co-ordinating all the different accounts into a single balance of production and consumption.” This remark in the application is the only indication that what was prepared was table fulfilling balancing requirements.

The terminology of “single balance” was reminiscent of that used in Popov’s work as reviewed in the Leontief (1925). Clearly, Leontief’s thorough study of it must have been very inspirational, and from which Leontief surely learned something. Note the linguistically awkward expression of “circuit flow” in the title. This was obviously an evocation of *Kreislauf* as used in the title of the 1928 dissertation.

The report from June 1934 was disappointingly minimalistic. It just stated that the project was “completed in its main part.” Nothing was stated about the dimensions of the table and the industrial specifications.

It seems that the empirical work in Leontief’s first two years at Harvard was driven by its own dynamics, rather than by an overall theoretical design, towards a table giving a so complete picture as possible of the interrelations of the economy within the limits set by available data? It may seem as if it was the completion of the empirical table which

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theoretical scheme; (2) gathering necessary empirical material; (3) applying the “theoretical devices” to analysis of the factual data. But in the case of input-output it seems that (2) preceded (1).

<sup>207</sup> Leontief tried out names for the table. In 1932 he had no name for it, in 1933 he used “interrelation of industries”, amended the following year to “mutual interrelation of American industries.” In Leontief (1936) it had become “quantitative input and output relations.” The “input-output” term emerged later, presumably from the title of Leontief (1936). “Input-output analysis” was coined during WWII at the Bureau of Labor Statistics.

spurred Leontief on to conjure up the theoretical structure he imposed on the table rather than the other way around.

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Schumpeter had impressed upon Leontief while he was still in Germany the importance of publishing in an English-language journal. After Leontief moved to Harvard Schumpeter's advice became even more urgent. Leontief submitted in June 1933 as a paper titled "Economic Changes and General Equilibrium" to *Economic Journal*. There is every reason to assume that the paper submitted had been prepared as the theoretical part (a) of the 1932 application. But it was to no avail, the manuscript was summarily rejected by the editor J. M. Keynes after 5-6 weeks.<sup>208</sup>

He had in fact mentioned this paper to Wesley Mitchell two months earlier. Mitchell had asked Leontief in April 1933 about the work NBER was waiting for. Leontief promised the statistical analysis to be forthcoming very soon and added for Mitchell's information that he was just finishing a paper for the *Economic Journal* about the theoretical scheme of his work for NBER.<sup>209</sup>

Some months later Leontief submitted the paper to *Econometrica*. As the cover letter stated that Schumpeter had read the manuscript and advised him to submit it the assistant editor took that as equivalent to refereeing and sent the manuscript to Frisch.<sup>210</sup> Unlike Keynes, Frisch liked the paper and returned it with extensive comments and Leontief resubmitted a revised version. Frisch reviewed it again, found Leontief's revision unsatisfactory and rewrote a several pages long section of Leontief's paper before he returned it. The paper seems to be lost, which is great pity. The exchange between Leontief with Frisch is the only documentary trace of the paper with Leontief's attempt to write out the general equilibrium ideas he struggled with after the *Eisenaufsatz*. Below follows an abbreviated version of the exchange:

Frisch to Leontief, 4 January 1934.

"Dear Professor Leontief,

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<sup>208</sup> Leontief to Keynes, 2 June 1933 & 25 July 1933. I am grateful towards Wilfried Parys for making me aware of these letters.

<sup>209</sup> Mitchell to Leontief, 28 April 1933; Leontief to Mitchell, 29 April 1933. Mitchell had also inquired about an article on the theory of international trade he had heard that Leontief was preparing. Leontief responded curiously that the international trade was just an "unimportant by-product of his lectures" not deserving Mitchell's attention. This was an odd self-deprecating comment as the paper on indifference curves in the analysis of foreign trade (Leontief 1933) brought Leontief quite a bit of attention.

<sup>210</sup> W.F.C. Nelson to Frisch, 25 November 1933.

I have just read the manuscript of your paper submitted to *Econometrica*. I am very much interested in your general idea of investigating the repercussions from one system to another which may be caused by changes in any one system in a closed circuit. So far as I understand, this is the essence of your approach. This idea, I think, is very promising, and I believe definitely worthwhile to follow up.

The general way in which you have set up the problem is very good, but I am under a strong impression that the whole exposition could in places be very much improved upon, if it were condensed and perhaps accentuated a little more by indicating sharply the object followed within each of the sections. I should therefore recommend that you go over your manuscript once more with this in view.

While reading the manuscript I have taken the liberty to suggest – with pencil marks – certain pages, which I think could be omitted without weakening your whole chain of argument. As a matter of fact, I think your main idea would stand out more clearly if this were done. In places I have also suggested the re-phrasing of certain sentences. (...)For the last part of the manuscript I have only made very few and very general suggestions.”

So far it was sheer praise. Leontief’s paper clearly was appreciated by Frisch. The more technical comments that followed cannot really be appreciated without a conception of the model of Leontief’s paper, which apparently is lost. But an idea of Leontief’s model can be gleaned from the section of Leontief’s paper which was rewritten by Frisch after the paper was re-submitted. The model set out in that section was a highly structured general equilibrium system of  $n$  production functions, representing Leontief’s idea of vertical stages of production as a causal chain with. It was set out elegantly by Frisch as a circular causal chain (but the circularity was Frisch’s idea and Leontief didn’t like it). The Frisch version of (part of) Leontief’s system was as follows:

$$\begin{aligned}
 x_n &= f_n(x_{n-1}, b_n) \\
 x_{n-1} &= f_{n-1}(x_{n-1}, b_{n-1}) \\
 &\text{-----} \\
 x_2 &= f_2(x_1, b_2) \\
 x_1 &= f_1(x_n, b_1)
 \end{aligned}$$

The amount supplied of  $x_i$  by sector  $i$  was a function  $f_i(x_{i-1}, b_i)$  of the input  $x_{i-1}$  provided by sector  $i-1$  and a parameter  $b_i$  and so on in a circular chain. The problem posed by Leontief was to find the equilibrium solution in the  $x$ 's when one or more of the  $b$ 's shifted. Frisch’s letter continued as follows:

“There is another point I would like to mention. You say that the transformation processes can be lumped together in such a way that all the  $f_i'$  become equal. Of course from the purely logical viewpoint this true, but if such a lumping together is done, we may get results that have very little economic meaning. If the  $f_i'$  shall have economic significance they must be interpreted as marginal costs or something of that sort referring to concrete processes of the type we know from actual economic life, and if they are defined in this way, they may of course turn out to have definite magnitudes.

But this is not all. Even from a purely logical viewpoint, there is something which makes it perfectly artificial to assume all the derivatives equal; these derivatives depend of course on the magnitudes of the various variables involved so that for one set of values of these variables one would have to adopt one system of sub-division for the process, and for another set of variables another kind of sub-division would have to be adopted. The whole system of division would simply change continually as the general system of general parameter (prices, quantities, etc.) changes in the market. This leads to so much arbitrariness that I do not see how the idea of equality of derivatives can help to shed light on the normal happenings.

There is, however, no need to assume these various derivatives equal. It seems that your whole argument could be developed without making such an assumption. (...)

If you would like to work through your paper once more with these various things in your mind, I should be very much interested if you would submit a new version of it for consideration with a view to its being published in *Econometrica*. But some such revision appears to me to be essential as I do not think we could accept the paper in its present shape. Your manuscript is returned herewith.”

Three weeks later Leontief sent a revised version to Frisch with the title changed to *Vertical repercussions in a chain of production processes*:

Leontief to Frisch, 26 January 1934.

“I am sending you a new version of my article on “Vertical Repercussions” adjusted and brought in line with your requirements.

The assumption  $f_1' = f_2' = \dots = f_n'$  is dropped and the mathematical treatment changed accordingly. The problem of scale of measurements to [be] discussed in an additional footnote on page 17 (addendum).

It would be rather difficult for me to make new computations and to redraft the graphs, but one explanatory sentence in the text will, I hope, preclude any misunderstanding in this respect.”

Leontief’s revision of the paper did not satisfy Frisch:



Frisch to Leontief, 8 February 1934.

“Dear Professor Leontief,

Thank you for yours of Jan. 26, with which was enclosed the revision of your ms. I am sorry to say that I find that your corrections do not adequately take account of the objections I formulated in my last letter. I found therefore that if the material should be utilized at all I had to work through the details myself. This I have done. On doing so I find that your formula regarding the effect on the output  $x_i$  is actually correct, but it is developed in an all too cumbersome way. (...) In reality the whole thing can be developed rather simply by using the classical rules for the derivation of implicit functions. This I have done and changed the manner of presentation accordingly.

In the latter part of your paper, more precisely in the part discussing prices, there is a fundamental mathematical error which makes it necessary to scrap this whole part. The error is this: You assume that all the processes work under the application of the marginal remuneration principle. But this assumption is inconsistent with the very basis of all the previous formula, indeed if the marginal remuneration principle is generally applied the system of equations by which the derivatives are determined becomes singular.

Instead of the last part of the paper I have therefore written a short section pointing out the true connections between the assumptions. I have also formulated the conclusions in terms of elasticities (not only in terms of marginal productivities) in order to get a statement that is invariant under a change in the units of measurement.

If you care to have the ms. appear in *Econometrica* in this new form I shall be glad to accept it. In this case I think there should be added a footnote where you make an acknowledgement to me for instance something like this:

“The author is indebted to the editor of *Econometrica*, Professor Ragnar Frisch, for certain corrections and simplifications in the mathematical derivations as well as for many suggestions for improvements in the manner of presentation in general.”

Frisch was an inexperienced editor in 1933 and would have done better if he had left to Leontief to write the acknowledgment note. Two weeks later Leontief answered:

Leontief to Frisch, 27 February 1934.

“Dear Professor Frisch:

I appreciate very much the trouble you have taken with my ms. and I am glad to know that you have found that the formula describing the quantity changes is correct. Your remark about the impossibility – under the given assumption – of applying the marginal pricing principle universally is very pertinent, but I do not think this can

invalidate the second part of my analysis devoted to the changes of the marginal productivities. My conclusions still hold in relation to the majority of prices which are proportional to the marginal productivities. Indeed, I would be very interested to have your further opinion on this point. I think it would be interesting to drop the assumption of a purely circular arrangement of the elements and investigate the more general cases. I feel unable to accept your kind proposal to publish your version of my article.

With many thanks and best regards.

Sincerely yours, W. Leontief.”

The section of the paper rewritten by Frisch was returned.<sup>211</sup> That finished it. No paper was published and Leontief’s careful formulation suggesting an interest in a further dialogue with Frisch was not heeded. Frisch was irritated over Leontief’s refusal of adhering to his advice about notation and other matters and would have done better if had left to Leontief to decide how to acknowledge help from the editor.

Later in the year Frisch brought up the incident in correspondence with Schumpeter:<sup>212</sup>

“Dear Schumpeter,

With regard to the Leontief case I know that you had recommended it for publication in the *Economic Journal*. I think I commit no indiscretion by saying the Keynes told me the whole story during my visit to Cambridge. Keynes was very uncomfortable about the matter, but felt that he had to turn the paper down. I am convinced he did the right thing. It would have been a scandal for any scientific journal to have that paper appear. I think you have judged the paper only by the introduction which was – from the point of view of style and presentation – quite well written. If you had worked all the mathematics through carefully as I did, I am sure you would have been entirely of my opinion. (...) I spent several days on the paper, suggesting to Leontief what he could do in order to bring the paper into order. ... Leontief made a few minor modifications, but not the fundamental ones which were necessary in order to put the matter straight. ... I again spent several days on the paper, this time actually indicating in more detail what had to be changed. (...) The final result was that Leontief found he could not accept to have the paper appear in this form, because, as he said, too much of it was now due to me.”

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<sup>211</sup> The handwritten manuscript was found in 2013 in the basement of Frisch’s Institute (now Department of Economics, University of Oslo) in immaculate condition after crossing the Atlantic twice in 1934 followed by eighty years in cold storage.

<sup>212</sup> Frisch to J.A. Schumpeter, 6 June 1934; 19 November 1934. Schumpeter to Frisch, 2 November 1934.

Schumpeter responded:

“Dear Frisch,

I ... do not think as you do about the paper he sent in for *Econometrica* (which in the first part, you have of course immensely improved by your superior technique, but the second part of which is hardly based on a ‘mistake’ pur et simple, while it points in a most hopeful direction). ... I like Leontief and think highly of him. (...).”

The positive impression Frisch had expressed about the idea pursued in the paper had gone sour. The exchange between Leontief and Frisch overlapped with another and more confrontational exchange between Leontief and Frisch: the *Pitfalls* incident.

Frisch’s essay *Pitfalls in the Statistical Construction of Demand and Supply Curves* (Frisch 1933) was a sharply formulated criticism of the statistical reasoning in the *Versuch* treatise. The Frisch essay figures prominently in the history of econometrics as a milestone in the development of an understanding of the identification issue.<sup>213</sup> Frisch was an expert in the field that Leontief had entered when trying to work out an adequate method for estimating supply and demand elasticities in the *Versuch* treatise. Others had also criticized Leontief’s method.

Frisch’s *Pitfalls* criticism of the *Versuch* treatise became an acrimonious affair, not least because it was played out in the open. It is of peripheral interest for Leontief’s discovery of input-output economics; hence we bypass the substantial issues here as they are well covered in the literature. The sharp criticism of Leontief’s main scholarly contribution in his second year at Harvard put him in an uncomfortable position. He responded to Frisch’s criticism in Harvard’s *Quarterly Journal Economics*.

Frisch was much surprised to see Leontief’s defiant response to the *Pitfalls* essay (Leontief 1934a) as Henry Schultz had told him at the Econometric Society meeting in Leyden 1933 had stated that that Leontief had admitted that the Frisch’s criticism was correct.<sup>214</sup> He responded with a very sharp rejoinder, ending in the following passage: “One cannot help feeling that the prestige of economics as a science must suffer when papers containing such mistakes and oversights as Dr. Leontief’s last paper, appear in a journal of high international standing.” (Frisch 1934, p.755). The statement was not well received at Harvard. Schumpeter spoke of it as “the slap in the face you gave the whole department by your remark” and told Frisch that he had come close to destroying a great talent.<sup>215</sup>

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<sup>213</sup> Morgan (1990,183-187). .

<sup>214</sup> Frisch to Schumpeter, 19 Nov. 1934.

<sup>215</sup> Frisch to Schumpeter, 19 Nov. 1934.

Leontief wrote another defiant response to Frisch (Leontief (1934b) and the editor asked Jacob Marschak for a comment in the same issue. Marschak was highly regarded by both Frisch and Leontief and his considerate and balanced comment possibly helped to soothe the feelings on both sides.<sup>216</sup> Marschak pointed out correctly that Frisch had not discussed Leontief's economic considerations. Frisch admitted this but argued that he had decided to leave the economics of the model out because it was the technique as such that was an optical illusion, regardless of the interpretation given to the parameters.<sup>217</sup>

Tinbergen looked at the matter much like Frisch and much later he commented: “[Frisch] did not lack a fascinating sense of humor when, after having published a study named *Pitfalls in the statistical construction of demand and supply curves*, he reacted to his critics in a second publication named *More pitfalls in demand and supply analysis*”<sup>218</sup> But Leontief was hardly amused.

The confrontations between Frisch and Leontief 1934 became needlessly sharp. This was paradoxical as they held very similar views as econometricians in the 1930s. Frisch was right on the substantial points in the *Pitfalls* controversy but should have been able to handle the confrontation with Leontief better.<sup>219</sup>

### 1934 and 1935: the input-output paradigm comes into shape

In 1934 Leontief's application to the Harvard Committee comprised three projects:

- a) Completion of the current statistical survey of mutual interrelation of American industries;
- b) A statistical study of price and quantity changes on important commodity markets in their mutual interrelation;
- c) Analysis of the rhythm and amplitude of cyclical fluctuations.

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<sup>216</sup> For a blow by blow account of the *Pitfalls* incident, see Hendry and Morgan (1995, pp.38-40 & 257-270).

<sup>217</sup> Frisch to Schumpeter, 19 Nov. 1934. Marschak (1934, p.766). For other views on this controversy, see e.g. Gilboy (1933), Working (1935).

<sup>218</sup> Tinbergen (1973, p.483).

<sup>219</sup> After 1934 Leontief's *Versuch* attracted little attention and was never translated. One of very few who saw merit in Leontief's method was Edward Leamer who in the 1970s expressed surprise that Leontief's contribution had been so completely ignored by the post-1940 econometrics literature. He noted that Frisch devoted the *Pitfalls* essay almost completely to debunking the method (Leamer 1981, p.321)., but more important he blamed the post-war econometrics for “excessive attention to asymptotic properties of estimators and insufficient interest in the shapes of likelihood functions” (Leamer 1981, p.322).

Project a) was about the completion of the 1919 input-output table. Leontief asked for only one month's additional research assistance. The request for support was granted but the amount turned out to be insufficient to finish the table. Leontief in 1935 applied for an additional grant to finish the table the project had become quite controversial within the Committee, as we shall see below.

Project b) in 1934 came with the following outline of study:

“This project, although independent in its set-up as a separate study, can be considered as a further development of the previous investigation (project a). In contrast with my previous studies, it will represent an attempt to abandon the partial supply and demand analysis and investigate more directly the connections between the price and quantity changes of the most important groups of commodities.”

The first sentence here is interesting stating that the project is “further development of the input-output table. Although the project is undercommunicated in the application to the committee the project is clearly the first paper about leading up to Leontief (1937). It may seem a bit odd today to call it a “statistical study” (as it was for the input-output table in project (a) to be denoted a “statistical survey”). It was about “price and quantity changes on important commodity markets in their mutual interrelation.”

But notice the second sentence in the outline of project (b) which is a striking renouncement of the *Versuch* paradigm! What caused Leontief to announce at this specific time and place that he had abandoned the partial equilibrium supply and demand analysis? Was it an announcement to himself or to the Harvard Committee? One cannot avoid noting that it happened just at the time Leontief struggled to respond to the criticism of Ragnar Frisch's *Pitfalls* essay; but that controversy was about the statistical methods not about the economic model. The renouncement was in any case so pointedly stated that we have to read it as a very deliberate statement.

Leontief had since the *Eisenaufsatz* taken an increasing interest in “general equilibrium” matters without being very precise about the equilibrium structure he had in mind. But at the basis of his reasoning was still the *Versuch* paradigm of a system of supply and demand curves with the supply and demand elasticities as the structural parameters. The paradigm had been enhanced through the reasoning initiated in the *Eisenaufsatz* that the supply curve of, say, pig iron would be influenced by supply changes in the inputs of pig iron production, such as iron ore and coke. Similar relationship would be found on the demand side. In later terminology they would be known as forward and backward linkages.) Such relations would connect individual markets, each with a supply and demand curve. A “primary change” in such a system as Leontief explained to Schumpeter could be an autonomous change in an elasticity due e.g. to tastes or technology, would generate secondary changes throughout the system.

Such was the conceptual model Leontief tried out in the “lost” paper on economic changes from the 1932 application. The *Versuch* paradigm was linear due to log-linearity and constant elasticities. When combined with intermarket relations the linearity could not be retained. His thinking along this line before and after he left Europe, would have told him that a solution of non-linear general equilibrium model would be hard to achieve even in relatively small dimensions.<sup>220</sup>

Announcing that he abandoned, or attempted to abandon, the *Versuch* paradigm analysis he would as quoted above from the 1934 application “investigate more directly the connections between the price and quantity changes of the most important groups of commodities.” Leontief doesn’t give much away through these formulations. One could be tempted conclude that there was no definite clue about an input-output paradigm yet but at this time he must have been considering the key issue: what can be derived from a singular input-output table about the structure of the economy.

Project c) in the 1934 application in analysis of rhythm and amplitude of fluctuations had the following outline of study attached to it: “A preliminary survey has shown that the rhythm (wave-length) of economic cycles is in many instances much more stable than the amplitudes of the waves. This seems to justify certain theoretical considerations concerning the nature of economic cycles. A more thorough statistical analysis along these lines can be expected to throw definite light on this problem.” The study of cyclical fluctuations had been an important issue for Leontief, closely related to the supply and demand curve analysis, also for understanding the limitations of the *Versuch* paradigm. He had worked on it during his NBER year. An outcome of this work was his paper, *Price-quantity variations in business cycles* (Leontief 1935).

In 1934 Leontief following the lead of Ezekiel, Ricci, Schultz, Tinbergen and Rosenstein-Rodan added to the literature on “cobweb dynamics of non-linear supply and demand curves” with some quite original ideas.<sup>221</sup> It was an outgrowth of the problem dealt with in *Versuch* but unrelated to the input-output work. He submitted to the Austrian journal edited by Morgenstern and it was the last paper he submitted in German (Leontief 1934d). Leontief presented a paper on it at the Econometric Society in New York, December 1935, prominently in the opening session.<sup>222</sup>

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<sup>220</sup> “The intricacies of [general equilibrium] are so great that many years will pass before it will be possible to fill, with concrete statistical data, all the hundreds of ‘empty boxes’ of the Walrasian supply and demand equations.” (Leontief (1935, p.24).

<sup>221</sup> Samuelson (2004, p.6). Samuelson added that “his topological explorations into multiple periodic motions came close to chancing on modern *chaos* theory” (p.6).

<sup>222</sup> See *Econometrica* 4, p.184. An English version of Leontief (1934d) was published in Leontief (1966).

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Wassily and Estelle went on a pleasure trip to Europe in the early part of the summer of 1934. In Paris they run into Henry Schultz in the middle of June. Later they visited the parents in Berlin and went to Spain. Leontief may have been worried about the situation of the parents in Germany after Hitler's accession to power.

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In 1935 Leontief asked for an additional appropriation for an assistant to complete the table. To compile an input-output table is a quite demanding project, especially when there is no blueprint or guidebook. Very comprehensive work on the 1919 table had been done with few resources and it should not surprise that scrutiny at a late stage indicates that an additional effort is required before publishing the table. An application for an additional appropriation was held up by the Committee, causing some aggravation both inside and outside the Committee. It seems that within the committee some members doubted both the worthwhileness of the table and whether it would ever be finished. This was a very serious matter for Leontief because without the completed table he would not have much to show for himself as an outcome of his research activity at Harvard. In the short run nothing more could be done. Leontief decided to raise the matter again by applying the following year for reopening the project.

But Leontief had also another project in 1935: "A statistical study of price-quantity changes of selected commodity markets from the point of view of general equilibrium." The title was almost identical with project (b) in 1934. Leontief took crucial steps towards basing his general equilibrium on a linear structure. Also at this point the application documents use disappointingly few words conveying the ideas he dealt with. The outline of the 1935 project described it as an investigation combining on the one hand the empirical survey of the mutual interrelation of American industries which has supplied "quantitative information concerning the circuit flow of commodities, services and incomes", i.e. the 1919 input-output table, and on the results of his study of price-quantity relations of selected commodities, i.e. project b) in 1934.

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After his confrontations with Frisch in 1934 Leontief may have felt disinclined to submit any more of his work to *Econometrica* but in 1935 he got a paper accepted. It happened more or less accidentally without a prior intention of submitting a paper.

Leontief presented in June 1935 a paper titled *Composite commodities and the problem of index numbers* to an Econometric Society meeting in Colorado Springs. It was a small meeting, only a handful of papers were presented. Charles Roos and Harold

Hotelling, two prominent members of the Econometric Society were both favorably impressed and suggested that the paper be submitted to *Econometrica*. The editorial office of *Econometrica* in USA was in fact in Colorado Springs. Leontief's manuscript arrived on the assistant manager's desk on 12 July, 1935 and on the same day he also received the latest issue of the *Review of Economic Studies* with an article by Hans Staehle on the theory of price indexes (Staehle 1935). The two articles seemed to Nelson to overlap in content and he vented his quandary to Frisch:

“In running through Staehle's article, I find no reference to Leontief's work. A rather delicate situation thus arises. In the circumstances, I am sure you will wish to go through both articles carefully yourself in order to arrive at some judgment as to the independent value of Leontief's paper. If Staehle and Leontief both have reached new conclusions, then, of course, publication of the report of the Colorado Springs meeting will give Leontief some priority. If Staehle seems to have appropriated Leontief's material without acknowledgement, an awkward situation emerges. I am, therefore, referring the whole matter to you.” (Nelson to Frisch, 7 July 1935).

Frisch responded after studying Leontief's paper:

“I find Leontief's paper very interesting. It is accepted for publication in *Econometrica*. In view of the appearance of Staehle's paper, I have as Editor appended a footnote to the MS, ... I think it is fair to give this information in order to protect Leontief's priority. ... In view of the delicate situation regarding this MS I have preferred not to do any editing at all. If there are verbal or other slight changes or condensation which you as Assistant Editor would suggest, please communicate direct with Leontief. Please also try to get this in an early issue, preferably the January issue.” (Frisch to Nelson, 9 Sept. 1935).

The footnote was shortened and revised by Leontief who had spoken with Schumpeter about it. It appeared as follows:<sup>223</sup>

“This article was completed more than a year ago. In the fall of 1933 it was communicated to professor Schumpeter's Discussion Group at Harvard and on June 24, 1935 it was presented at the meeting of the Econometric Society in Colorado Springs. The June 1935 issue of the *Review of Economic Studies* contains a paper in Index Numbers by Dr. Hans Staehle in which the treatment of

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<sup>223</sup> Leontief to Frisch, 3 Nov. 1935; Leontief to Nelson, 3 Nov. 1935.



several topics is very similar to that given in the present article.” (*Econometrica* 4 (1), p.39).

This was a rare case of priority conflict in *Econometrica* or just a touch of it. Staehle was on his way to become a renowned expert on index numbers. Leontief’s article was published as quickly as possible; it appeared as article no. 2 in the issue, after the annual survey of theory always placed first in the issue; in this case it was by Frisch on the problem of index numbers (Frisch 1936).

The editorial incident had an aftermath sixty years later. In an article by Paul Samuelson in a book published 1995, he wrote as follows:

“Ragnar Frisch was pretty much autonomous editor of the early issues of *Econometrica*. He was interested in everything. Also, he believed in the superiority of his interpretations of anything and everything. [Indeed, he was so great a mind that there was much merit in such belief.] When Wassily Leontief participated in the post-1933 revival of the economic theory of index numbers (à la Konüs, Staehle and others), Frisch held up publication of the Leontief 1936 contribution until he could publish in the same issue of *Econometrica* his own survey article on the subject. Foul play, I say.” (Samuelson 1995, p.23).

Samuelson had great admiration for Frisch but his assertion was utterly false. Frisch had not held up the publication of Leontief’s paper but on the contrary hastened it by shortcutting the refereeing process. And he was glad to have his paper published side by side with Frisch’s article.<sup>224</sup> There was no foul play.<sup>225</sup>

### 1936 and 1937: The two REStat articles

In February 1936 it was again time for submitting applications to the Committee. It was crucial for Leontief to apply again for a grant that would allow him to complete the 1919 table. The application referred to the Committee’s decision in 1935 to postpone the decision concerning the grant of an additional appropriation for the completion of the 1919 table. Leontief had solicited the support of Professors Edward Gay, John H. Williams and William L. Crum who had been briefed and supported the request for a grant to complete the table.

The application made it clear to the Committee that the postponement had resulted in the loss of Maynard Heins’ assistance: “The necessity caused by anticipated lack of funds

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<sup>224</sup> Leontief to Nelson, 20 Nov. 1935.

<sup>225</sup> Leontief’s had a longer history; it had been submitted to *Journal of Political Economy* early in 1934. The editor, Frank Knight, had after refereeing invited a revise and resubmit to which Leontief did not respond, perhaps because he found the anonymous referee too unappreciative of his ideas.

to discharge my assistant would critically impair the progress of the planned research. It is practically impossible to find another man of similar experience for this work.” Leontief was notified shortly after submitting the application that the Committee had voted to grant an “additional sum not to exceed \$300” to complete the research on the 1919 table and make the grant available immediately (rather than from the beginning of the coming academic year). The first input-output table was published only six months later.

After the release of funds for completing the 1919 table a paper with the table was submitted to the *Review of Economic Statistics* and published already in August 1936 (Leontief 1936a) as the first ever input-output table based on comprehensive national statistics.

In 1936 Leontief applied to the Committee for a grant to compile a 1929 table. As the term input-output table had not yet been coined Leontief used different denotations, e.g. “Tableau Economique of U.S.A.” The census data for 1929 had not yet been completely processed in 1932 and Leontief had then decided to use the 1919. The decision was not obviously the best one as the 1919 census data had many shortcomings compared to the later censuses. He had chosen not to delay the work on the 1919 table to do the 1929 table and publish them together. After studying the 1929 material he realized that the comparative abundance of primary statistical sources made it clear that the results for 1929 would be considerably more complete and reliable than those obtained for 1919.

The work on the 1929 table could e.g. benefit from national account figures prepared by Kuznets. The 1929 table was used exactly the same specifications as the 1919 table to facilitate comparison. The preparation of it benefitted from the experience with the 1919 table and thus proceeded faster. It and was largely completed during the academic year 1936/37.

In the 1936 application Leontief mentioned the idea of a monograph based on the results from both 1919 and 1929 but that turned out to take a lot longer time to complete than he had expected. Even with the shortcomings of the 1919 table the advantages of having tables for two years were convincing. It would allow

- 1) a complete statistical background for a systematic study of the structural changes of American economy over ten years; and
- 2) material for corroboration of the stability of the structural features of the economy.

While in 1936 Leontief published the 1919 table and supervised the work on the 1929 table he also finished up the theoretical work on how the tables could be used to determine structural features of the economy and also calculate these numerically. Leontief found out at this time (if not earlier) that it was no practical possibility for doing the numerical calculations in full format for the more than 40 industries in the table. Perhaps he had

considered to illuminate the calculability of his formula through calculation by hand some examples in very small dimension.

As is well known he found a middle way by the help of Professor John B. Wilbur of MIT and his *Simultaneous Calculator* which could solve – “nearly automatically” – systems of nine linear equations and given that limitations was very well suited for Leontief’s purpose.

Outside the normal time for submitting applications Leontief got the Committee’s permission to submit in November an application for a project called “Computation of price-quantity changes on basis of an empirical general equilibrium pattern.” The content of it was set out as follows:

“The problem is that of finding a solution for a very large system of (43) linear equations. The difficulty of accomplishing a computation task of this sort can hardly be exaggerated. Reasonable estimates indicate, however, that no more time will be required than that for the calculation of a long index number series. Should the general equilibrium theory be ever put to practical use, computations of this type and often of even far greater proportions appear to be unavoidable. I am inclined to consider the present project as a test case.”

The required assistance was “One computer” at estimated cost \$300, i.e. 600 hours at 50 cents an hour. Wilbur and his *Simultaneous Calculator* were not mentioned but it seems pretty obvious that Leontief’s contact with Wilbur must have been established before the application was sent. The Committee considered the application right away and granted the amount asked for. *Interrelation of prices, output, savings, and investment* (Leontief 1937) was published 8 months later with a lot of calculations in between.<sup>226</sup>

Hence Leontief chose, wisely one may add, to rush to publish his results by using an outdated table and computer equipment which allowed only what he called a “preliminary study” based on a much smaller table than he had compiled.

The result was – eventually – the *Interrelation* paper in 1937 (Leontief 1937) which comprised (1) the input-output structure: (2) the formulae for calculating what the “impulses of any local primary change into the remotest corners of the economic system” (p.110); and (3) the numerical calculations. It laid the foundation for the input-output analysis.

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<sup>226</sup> Readers were offered to be supplied on request all relevant minors of the two key determinants in Leontief ‘s system. (Leontief 1937, p.131n1).

In the beginning of 1936 Estelle gave birth to a daughter. Pitirim Sorokin served as godfather when she was baptized in the Russian orthodox church and given the name *Svetlana*, the same as Stalin's daughter, born ten years earlier.

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In the 1937 application Leontief asked for a grant for a comparative statistical study of the structure and interrelation of American industries in 1919-1929. He foreshadowed a monograph from the complete study. He called it at first in 1937 a "monograph on statistical study of the general equilibrium"; then in 1938 "monograph on the empirical application of economic theory of general interdependence." (Leontief was not particularly proficient neither at making titles of books and articles, nor on coining new terms.)

Leontief had held the cards close to his chest about the input-output ideas he struggled with until he had published Leontief (1937). At the Econometric Society meeting in Atlantic City in December 1937 he presented a paper about what he had achieved under the title "Empirical application of the economic theory of general interdependence." The report in *Econometrica* comprised the following passage:

"The speaker stressed the difference between his approach and the other similar attempts to empirical application of the theoretical concept of general interdependence. The majority of investigators in this field are constrained to reduce the number of the variables to a few aggregative price and quantity indices because they base their analysis on the small number of rather complicated non-linear equations. He himself, on the contrary, reduces the theoretical issue to a solution of large systems of relatively simple linear equations and thus us able to operate with a much greater number of variables." (*Econometrica* 8, pp.190-191).

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In 1938 the Leontief family moved from a rented apartment on 22 Forest Street to a bigger and more comfortable one on 16 Kirkland Road, both in Cambridge. Just after the war the Leontief family purchased half of a two-family house on 14 Ash Street to become neighbors with the Schumpeters around the corner on Acacia Street. Leontief had bought a piece of American soil already in 1937 when he purchased an estate – a *dacha* – on Lake Willoughby at Barton, Vermont. It was a retreat for the family and a place for trout fishing as well as for hiking and mushroom picking .

## The Structure of American Economy

From the beginning of 1938 it was plain sailing but a considerable work effort, not least in numerical calculations, to complete the monograph. It took much longer time than expected. The date set for completing the manuscript changed from the end of 1938, to 1939 and 1940. The monograph was eventually published at the beginning of 1941.

In 1938 Leontief was elected Fellow of the Econometric Society, effectuated from 1939.<sup>227</sup> Econometric Society had two categories of members: Fellows and ordinary members. But it was really a Society of Fellows, because the fellows decided everything, ordinary members had no say in anything within the Society. The Fellowship was thus not just an honorary bestowment it was an election to a quite exclusive club. After Leontief had been elected the number of Fellows was only 38, while the number of ordinary members was 633. The election procedure was elaborate with a nominating process preceding the election according. In the announcement of the election results in *Econometrica* the name of each elected Fellow was accompanied by a shortlist of most important works. Under Leontief's name was a list of fourteen papers, among which were two papers written by Leontief's father about Soviet financial issues, a bizarre mistake!<sup>228</sup>

The situation of Leontief's parents in Germany, not least the fact that they remained there, was a worry for Leontief. The parents remained in Berlin not only throughout 1937 but also throughout 1938 marked by ominous events such as *Anschluss* in March, *Münchener Abkommen* in September, and *Kristallnacht* in November. They renewed their *Fremdenpässe* in June 1939 and were still in Berlin at the outbreak of World War II. They booked a passage on the Italian liner *S.S. Conte di Savoia* from Genoa to New York, the ship docked at New York on Thanksgiving Day, 23 November 1939.<sup>229</sup> A year and a half before his departure Leontief Sr. could read in the German press about the fate of the Soviet ambassador of his Berlin years. Nikolai Krestinsky had been arrested in 1937, put on trial 12 March 1938, and shot three days later.<sup>230</sup>

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<sup>227</sup> See *Econometrica* 7, pp.284-287.

<sup>228</sup> The list was prepared by the Secretary. Leontief's name had been nominated or «aired» several times since the first fellowship election in 1933, see Bjerkholt (2015).

<sup>229</sup> See Alpers (2013, pp.72-74). Luggage shipped out on another ship got lost.

<sup>230</sup> While almost all defendants admitted guilt during the Moscow Trials, Krestinsky denied everything:

On 12 March he stated: "...I was never a member of the right-winger and Trotskyite bloc ...nor have I committed a single one of the crimes imputed to me personally; and I am not guilty of having maintained relations with the German Secret Service. The next day he reversed himself: "Yesterday, under the influence of a momentary keen feeling of false shame, evoked by the atmosphere of the dock and the painful impression created by the public reading of the indictment, which was aggravated by my poor health, I could not bring myself to tell the truth, I could not bring myself to say that I was guilty.... I almost mechanically answered, No, I am not guilty."

The monograph built on three parts: the 1936 and 1937 articles and the unpublished work on the 1929 table and comparing the 1919 and 1929 tables. A topic that had been ignored in the compilation of the input-output tables was the government part. Leontief realized this at a late stage and applied in 1938 again to the Harvard Committee for a project titled “Governmental receipts and expenditures in their relation to American industries” and with following outline for what it was about:

“The purpose of this statistical investigation is to analyze the receipts and expenditures of public bodies in the same way in which the relation of each separate industry to all other parts of American national economy was analyzed in my previous studies. The absence of reliable data on this subject constituted a serious gap in my previous studies. Collection and integrated presentation of such data should be of great importance for an analysis of governmental economic policies.”

But it was too late for the book. It would be for the next input-out table for 1939 to integrate the government transactions properly into the input-output table and allow analysis of governmental economic policies.

The 1929 table was actually published in another publication two years before the monograph appeared. Leontief was contacted by Gardiner Means, a New Deal economist who in 1939 was the editor of a report issued by the National Resources Committee as the “first major attempt to show the interrelation of the economic forces which determine the use of our national resources” (National Resources Committee 1939, p.iii). The report surveyed the American economy and the use of economic resources. Means had studied the published 1919 table and was aware that a 1929 table of similar format was forthcoming. He contacted Leontief who gave him permission to print the 1929 table in the forthcoming volume.<sup>231</sup> But this was done at a late stage in the preparation of the report and the 1929 table arrived too late to make any impact on the analysis of the report. The input-output table for 1929 was almost hidden away in Appendix 17, as one table in million dollars and one as two-way percentage distribution.

The title of the publication edited by Gardiner Means was *The Structure of the American Economy* (National Resources Committee 1939). Thus while Leontief let Gardiner Means publish the 1929 input-output table (presumably with the permission of the Harvard Committee) but he took in return the title of his own monograph from that the National Resources Committee report. But he didn’t use it verbatim but for unknown (and hardly very convincing) reasons changed it to *The Structure of American Economy*.

After eight years at Harvard Leontief had a sabbatical year in 1940/41. He had been awarded a Guggenheim fellowship allowing him to travel. His plan was spend the first

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<sup>231</sup> See Gardiner Means to Leontief, 6 August 1938.

term in Berkeley and the second in Mexico. When the sabbatical was planned he had possibly expected the monograph to be published before he left, and he had surely not foreseen that he would have to read proofs and make editorial decisions while he was travelling. But that is what happened.

Leontief travelled by car with his wife and his four-year old daughter. The first leg of the journey was from Cambridge to Colorado Springs (Colorado) to attend the sixth Cowles Commission research conference. It was the last such conference, the Cowles Commission had moved its office from Colorado Springs to Chicago in 1939. Leontief gave his lecture “Some results of an empirical study of the general equilibrium” on 12 July 1940. He was in good company, among those who participated were two of his students at Harvard: Paul A. Samuelson and Trygve Haavelmo.

From Colorado the Leontief family drove in a leisurely pace to Vancouver and from there due south through Washington, Oregon and Northern California to Berkeley where they arrived in the beginning of September and lived in a house on Hilgard Street throughout the semester. The World War had lasted for one year. Elizabeth Gilboy wrote on August 26 from Harvard: “Littauer Center is pretty deserted at the moment, with everyone either on vacation or in Washington running the Defense Committee. ... Do the Pacific Coast papers mention the war at all.”

At Bancroft Library at Berkeley Leontief looked up François Quesnay’s *Premier problème économique*, published 1766, perhaps for the first time since he read it in Petrograd. He extracted three sentences which he sent to Gilboy and asked for them to be placed as a motto between the Foreword and the Introduction. He left it for the publisher to decide whether an English translation should follow the French original (which didn’t happen).

In November Leontief decided to spend some days on near a beach on the Pacific Coast; he booked accommodation at Carmel, a short ride down the coast.<sup>232</sup> The foreword of the monograph is marked as signed on November 15, 1940 in Carmel, California. The foreword contained very little apart from the assertion that “preliminary research for this investigation was concluded in 1931” at NBER. He showered lavish praise on the three assistants who had worked with him on different phases of the investigation.

Information arrived from Harvard that the publisher’s processing plan had been delayed. Originally, it was planned to have the book published by the first of December 1940 but that turned out to be impossible due to the many questions regarding charts and tables. Harvard University Press did not get the final word on the manuscript from Gilboy until 27 November 1940. The publisher wrote to Leontief two days later that the galleys had just gone to the shop for paging. It would not be possible to publish the book before the middle of January and hopefully before the end (it appeared one month later). The delay

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<sup>232</sup> Communication from Svetlana Alpers.

meant, the publisher told Leontief, that the end-of-year conventions of AEA, ASA and Econometric Society were missed as an opportunity for disseminating and propagating the book, and that “next year we shall certainly be able to include the book in the exhibits at the meetings”.<sup>233</sup> But next year United States was at war which reduced the general academic interest in new books (but increased the interest for others, such as the Bureau of Labor Statistics).

On 11 December 1940 Leontief, still in Berkeley, received the last of the page proofs and realized that the Quesnay quote was missing and he immediately sent a duplicate. It turned out that HUP had forgotten about but Leontief’s reminder arrived in time to insert it. Leontief also asked for the dedication “To My Father and Mother” to be included and appropriately placed.

Just after New Year 1941 the journey continued to Mexico City where the family lived in a rented flat in Rio Lerma 76, not far from the central avenue *Paseo de la reforma*. In his first letter exchange with Gilboy from Mexico he asked for his six complimentary books to be sent to his five Harvard colleagues Chamberlin, Harris, Haberler, Mason and Schumpeter and one to his parents, at the time living in Leontief’s house in Vermont. Gilboy suggested only six journals for review copies, “as no one is sending review copies to European journals at the moment”.<sup>234</sup> Leontief added one more US journal and the *Canadian Journal of Economics*.

The fifth of February, 1941 was too early to celebrate the book, it would still take a month, but it was celebration nevertheless. Svetlana had her fifth birthday that day and it was celebrated outside in the large patio of Rio Lerma 76. Svetlana danced to the music of a band playing.

Gilboy wrote on 5 March when the book had been out for one or two weeks. She could report that HUP so far had sold eighteen copies of the book. Leontief commented that eighteen copies didn’t seem to indicate a very lively demand. In fact that turned out to be fair judgement on the total sales of the book. HUP printed 614 copies of the book in 1941 and it took six years to dispose of them.<sup>235</sup> The price proposed by HUP was \$2.00 or \$2.50 based on the number of pages. This book was of course a lot more expensive to print than most, if not all, other books of the same size. Leontief had earlier suggested \$3.50 as a fair price. Gilboy decided that the price would be \$2.50 without seeking Leontief’s consent. She was in this process not only an intermediary between Leontief and the HUP, she also represented the Harvard Committee which had already paid HUP an agreed subsidy of \$1375.

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<sup>233</sup> D.T. Pottinger, Harvard University Pres to Leontief, 29 November 1940.

<sup>234</sup> Gilboy to Leontief, 29 January 1941. The six journals were *Econometrica*, *JASA*, *AER*, *QJE*, *J. of Math. Statistics* and *JPE*.

<sup>235</sup> Parys (2016).



A part of the original Leontief manuscript with explanatory notes on statistical sources and methods of computation was not included in the book but left to be reproduced as a supplement to be distributed. Both Leontief and Gilboy were hesitant about the supplement: mimeograph or duplication?; what price to charge?; how many copies to prepare? There was not question of dropping the supplement as it had been offered to readers in the foreword as distributed by the Harvard Committee for Research in the Social Sciences. And equally important, it was an obligation to offer complete documentation. Gilboy and Leontief agreed to postpone decisions until some requests for the supplement had come in, duplicated copies could after all be done very quickly.

By 14 March 1941 Gilboy had received nine requests for the supplement. She wondered of there would be a call for more than 100 copies. She had come to realize that the Foreword had not indicated that the supplement was not free and people who requested it apparently were hoping to get it free. She suggested a charge of \$2.00 but would take it up with the Committee. The Committee decided on the supplement copies to be prepared on Harvard's duplication machine and sold for \$0.50 per copy, 100 copies would cost a maximum of \$125.

In the same letter Gilboy expressed the hope that Leontief had got the received the book, she had checked with HUP made them swear that they had sent it. Leontief hadn't got it, perhaps because the Leontief family had gone on an excursion for some weeks to Taxco, Guerrero and the forwarding of mail was imperfect. Leontief wrote to HUP on 21 March that the book "finally caught up with me in the Mexican wilderness." He wrote to Gilboy the same day to say that he was at a loss in estimating the demand for the supplement. He suggested 100-125 copies, adding, "There hardly will be a rush of orders."<sup>236</sup>

Leontief may have been disappointed over the sales of the book and Harvard University Press was no less so. But they drew different conclusions about the future interest in Leontief's work. Harvard University Press refused to publish the second enlarged version of the 1941 book on the grounds that the monograph was "obsolete and of little scientific interest." Oxford University Press bought the publishing rights from Harvard University Press for one dollar!<sup>237</sup>

## Concluding remarks

The completion of the monograph was only the end of the beginning of input-output economics at a still early stage of Wassily Leontief's career. The current section of the

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<sup>236</sup> Leontief to Mr. Malone, HUP, 21 March 1941; Leontief to Gilboy, 21 March 1941.

<sup>237</sup> Parys (2016).

paper is thus not a conclusion, its aim is only to give some pointers for the next phase in Leontief saga, the phase which by his own expression, would “bring up the other wind.”<sup>238</sup>

The monograph was exceedingly well documented but had no appeal to the general reader. It documented an innovation in economic analysis at just the right time in history. The monograph was not suitable as a textbook. This was reflected in the sales figures which were dismal compared with those of books by Leontief’s Harvard colleagues. The delay in the publishing process implying that the book was not published until the beginning of 1941 detracted from the general academic attention to the book. But at the same time the demand for what Leontief had to offer was noticed by some of those highly concerned with the need for better tools for analysis for the mobilization of wartime resources. This led into the second phase of Leontief’s involvement with input-output project: the cooperation with the Bureau of Labor Statistics. That story will not be told here; main features of it can be found in Kohli (2001).

Leontief said in an interview in 1997 that Roosevelt’s Secretary of Labor, Frances Perkins, the first woman to be appointed to the Presidential Cabinet, had written to him that the President had asked what would happen to the American economy after the war.<sup>239</sup> Perkins had in 1933 appointed Isador Lubin as Commissioner of Labor Statistics. Lubin had succeeded in upgrading and expanding the Bureau of Labor Statistics (BLS) to become a professionally-staffed institution with considerable analytic capacity. In April 1941, while Leontief was still on sabbatical in Mexico, Congress invited to Lubin (bypassing other agencies) to set up a Defense Problems Unit to study the problems the United States would have to address to resume normal economic operations after the war. As this was before Pearl Harbor USA was not at war but the building up of the defense had been going on for some time and would clearly demand more resources, and much more if USA was drawn into the war.

Lubin accepted and left it to one of his Chief Lieutenants, Donald Davenport, to establish a division right away. It was called the *Defense Labor Problems Division*. Davenport had come to BLS from Harvard but had never met Leontief at Harvard but was aware of his work. The division studied Leontief’s work but was uncertain how to make use of it for their assignment. It was obvious that a new input-output table was needed based on the 1939 Census. Leontief and BLS worked out that this could be done by setting up a unit at the Littauer Center, manned by BLS, and directed by Leontief.

The acting head of BLS, Dal Hitchcock, cabled Leontief on 5 December 1941 to request an appointment at Harvard on the afternoon of 11 December to confer on the

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<sup>238</sup> «The input-output analysis represents an attempt to straighten out our line of advance by bringing up the other wing – the study of interindustrial relationships.» Leontief (1951, p.204).

<sup>239</sup> See Foley 1998, p.122. It is not when the letter was written.

Bureaus post defense studies.<sup>240</sup> In between these dates the Pearl Harbor attack took place. Davenport's Division was renamed the *Post-War Labor Problems Division* and a more intensive collaboration between Leontief and BLS was initiated. In addition to the supervision of the statistical work BLS wanted Leontief to be involved in the analytical work of BLS in Washington, which meant frequent trips between Cambridge and Washington from 1942. But there were also another and more powerful government agency which demanded assistance from Leontief, as revealed in the following excerpt from an interview with Donald Davenport:<sup>241</sup>

Davenport: I think we had several conferences in Washington and Dr. Leontief came down. He's the world's worst lecturer. You could hardly understand him because of his English is so broken.<sup>242</sup>

Grossman: He writes well.

Davenport: Oh yes. He reads well. He's a very able man but then we ran into another difficulty. There were very few people in the United States at that time who was completely at home in the Russian language and who understood economics. Leontief was one of those. He was a native born Russian and he spoke Russian like a native, probably better Russian than he did English and the Office of Economic Warfare preempted him – took him away from us, although we only had him on a consulting per diem basis, but they had a higher claim than we had to his services and he was taken away from us.

It was Office of Strategic Services (OSS), established in June 1942, Leontief would work for. The work was highly classified and little is known about it. OSS was headed by William J. Donovan, who (curiously) had been a member of the Econometric Society since 1934. Leontief worked under William L. Langer, who until World War II had been chairman of the history department at Harvard. He was Donovan's deputy until he became head of the Research and Analysis Branch of the OSS.

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One of the first reviews of the book was by Leontief's former *Astwik* colleague Hans Neisser in *American Economic Review* in September 1941. Neisser had known Leontief when it seemed that it was the *Versuch* treatise which would create a name for him among

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<sup>240</sup> Hitchcock to Leontief, 5 December 1941.

<sup>241</sup> Interview of Donald Davenport by Jonathan Grossman, 19 Jan. 1965.

<sup>242</sup> Leontief's "broken" English was noted, not least by his students, few of whom shared Davenport's sensitivity.

economists worldwide. By way of introduction Neisser mentioned Leontief's important contributions in partial equilibrium analysis, "which he is now inclined to look down upon." Neisser gave a concise assessment of what Leontief's assumption implied of qualifications of the Walrasian equation system and critical and quite critical of Leontief's inclination to minimize significance of the qualifying assumptions. Neisser had no trouble recognizing the importance of the pioneering effort:

"Who would not admire the ingenuity he shows in simplifying the Walras system in such a way that it, for the first time, became available to quantitative determination? It is much more important that such a work was attempted than that the first approach is perfect in all respects." (Neisser (1941))

As mentioned above Leontief had applied to the Harvard Committee in 1938 for a project on governmental receipts and expenditures in their relation to American industries. Leontief acknowledged of having the government transactions fully integrated. After the sabbatical year Leontief applied again at the first opportunity in February 1942 for a continuation of the project of integrating government transactions, and gave it the following comment:

"The project constitutes an extension of my analysis of the "Structure of American Economy" (published by the Harvard Press in 1941). This latter line of research has been taken over by a governmental agency (Department of Labor) which is extending it on a large scale. The same might happen in some future time to the subject of the present project. For obvious reasons I prefer, however, to develop the basic approach to the new problem quite independently of any official sponsorship."

Leontief's preference of remaining in control of his project comes through here. Government transactions were duly integrated in the 1939 accounts. BLS' interest in input-output tables and the cooperation with Leontief laid the foundation for an enormous boost for input-output analysis which over the following three-four decades conquered all continents. The Harvard Economic Research Project (HERP), established and directed by Leontief in 1948, supported financially in the early years by Rockefeller Foundation and the US Air Force.

The key to the success was the slight reformulation and reinterpretation of the input output structure of the 1941 monograph which can tentatively be dated to have taken place in 1943, resulting in the *open Leontief model*.

Leontief Harvard period lasted until 1975, ending not in retirement but in a decision to leave after “disenchantment” with Harvard’s Economics Department.<sup>243</sup> The disenchantment had set in much earlier.

## Acknowledgement

For the early part of Leontief’s life in St. Petersburg and Berlin I am in great debt to three persons: Svetlana Alpers, Svetlana Kaliadina, and Claus Wittich. Svetlana Alpers, Leontief’s daughter, made available to me some years ago an interview she taped with her father in 1989, and more recently a wonderful long book essay book she more recently wrote about her grandfather, Leontief Sr. I am also deeply grateful for answering my queries, volunteering advice, and correcting my mistakes (some of them). Svetlana Kaliadina, author and co-author, of two most valuable scholarly articles about Leontief and the Leontief family, kindly arranged for me permission to publish the two articles in a special issue of *Economic Systems Research*, for which I was guest editor. Claus Wittich has on many occasions let me draw on his great insight of all things, German and Russian, related to Leontief and his world, and also annotated, translated and overseen English edition of the Kaliadina papers. I owe great thanks to Harvard Archives for letting me access the Wassily Leontief Papers, and the National Library of Norway for access to Ragnar Frisch correspondence, and Humboldt University for access to Leontief’s student records. I thank furthermore Wilfried Parys, Guido Rauscher, Guido Erreygers for advice and information over the years, Anna Pauls for translation, Inger Bjerkodden for unflinching encouragement, and Robert Solow and Ann Carter for being living memories of times past.

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<sup>243</sup> See *Harvard Crimson* (29 Jan. 1975). *Harvard Gazette* (31 Jan. 1975) cited Leontief’s “profound disappointment” at “my own inability to change the direction of the Department”.

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