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On the Existence of a Competitive Equilibrium: 1930–1954

By E. ROY WEINTRAUB
Duke University

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I. *Introduction*

THERE ARE AT LEAST THREE approaches to writing histories of economic thought. In the first, the past is examined for precursors, and present day theories and truths are located in past achievements. Connecting modern understandings with past work can strip away wrong-headed lines of argument and unfruitful tangents and can produce narratives in which lines of inquiry are clear, distinct, and cumulative. Such histories are occasionally useful. When they are written by distinguished economists they can help the profession to identify major lines of influence, or at least those lines of influence that will later define the shared myth. Yet looking back from the present distorts the past. The history is bloodless. Truths pile on truths, and theorems get stronger. The present appears inevitable.

A second perspective which can shape writing in the history of economics is "methodology." The author, rather like a philosopher of science, can treat the past as case study materials, or data, as he constructs and tests explanations of disciplinary change. If, for example, one wishes to defend the idea that economic knowledge progresses discontinuously, one might use the past to identify putative revolutions. The research imperative will develop the historical materials so that the conjecture can be falsified. "The Keynesian revolution was a revolution in the sense of Kuhn" is a potentially falsifiable proposition. The proposition, "Keynes' economics represented no theoretical break with the past," is also potentially falsifiable. The data that are adduced to settle such questions are the historical materials themselves. When fairly selected and presented, the materials come alive and inform our understanding of what constitutes disciplinary progress, of how economic theories may be appraised.

The difficulty with this approach is that the evidence is narrowly selected to test the hypotheses. Writings on Keynes' contributions, my own included, confirm the hazards of this type of historical study. Though most Keynesiana is Whig history at its worst ("if *uncertainty* is the lost theme in modern theory, at least Keynes was not blind") some elements of this literature genuinely attempt either to defend a notion of progress or alternatively to explain how it ends. Such methodological writing may focus on major issues of historical reconstruction, but the selection of materials necessarily ignores data, or historical evidence, troublesome to the thesis. The actual history frequently provides not only the "evidence," but much that is irrelevant to the testable proposition. Yet such irrelevancies may be important. What is the methodologist to make of contradictory arguments that the historical personage used to support his main idea?

The third major approach is more traditionally historical. It seeks to produce a coherent narrative. "Explanation" of the historical evidence proceeds through detailed examination of the materials, the authors, and the context. Such writing *may* be guided by notions of how economic science progresses, but this perspective is not usually explicit. An historian who believes that the social and intellectual environment shapes the intellectual work of a period may construct the narrative to emphasize elements of social and intellectual history. In any event, the narrative is less self-consciously "philosophy searching for examples" or the "present searching for justification."

Because what will follow, in subsequent sections, must be tested and evaluated from the disparate perspectives of practicing economists, methodologists of economics and historians of economic science, it is probably necessary to identify my own limitations. I am, by training, neither an historian nor a philosopher. Indeed, I was not even trained as an economist. Although I grew up around economists, I studied mathematics and philosophy as an undergraduate, did graduate work in applied mathematics, and wrote a dissertation on a problem in general equilibrium theory in economics. My interests in the background of the subject have remained to the present.

The following sections represent my own efforts to construct a history of one portion of general equilibrium analysis from the early 1930s to the early 1950s. If general equilibrium analysis constitutes a "paradigm" (Kuhn, 1962), or a "sequence of models" (Koopmans, 1957) or a "research program" (Lakatos, 1978) of rather special importance, it is something of a conundrum, and a bother, that the historical materials themselves are so sketchy. By all reasonable criteria general equilibrium analysis defines a coherent body of thought as fully articulated as Keynesian economics. Yet the historical material associated with the latter runs to the tens of thousands of pages, while that of the former hardly exists in an organized fashion. Nobel Laureates Arrow, Hicks, Klein, Koopmans, and Samuelson are all associated with this tradition, yet narrative histories of general equilibrium analysis rarely proceed beyond Walras and Pareto. The history of neo-Walrasian analysis is in the footnotes of current contributions, or in sections titled "Notes on the Literature" in modern treatises.

In what follows, I shall focus on the idea of equilibrium itself, and shall therefore emphasize those works which are related to the problem of "existence of equilibrium." It is too early, in dealing with these materials, to write the account as Lakatos would have preferred, as a rational reconstruction (with the real events in the footnotes so that the "ideal story" can be compared to the actual story). Neither is it feasible for me imaginatively to recreate Vienna in the 1930s, or the Cowles Commission in the 1940s. I shall likewise suppress my inclinations to draw any lessons from this account.[1] Such an enterprise is better left for a longer treatment, at a later time.

II. *Setting the Stage: Cassel's System*

Although the main story begins with Walras, for present purposes the state of knowledge of general equilibrium analysis in 1930 can be defined by Gustav Cassel's *The Theory of Social Economy,* the first edition of which appeared in German in 1918.[2] In the early pages of this work, part treatise, part textbook, Cassel clearly set out the divisions between the consumers and producers, and integrated the market outcomes in product and factor markets. As in his earlier *"Grundriss einer elementaren Preislehre"* (1899), Cassel argued strongly against marginal utility and "value." Instead Cassel placed *prices* at the center of his allocation theory and used demand itself as a primitive concept. His verbal treatment of production, the relationship of inputs to outputs, recognized substitution possibilities in a way that his mathematical analysis did not.

After the introduction of his laws, or market principles, Cassel set out a formal system in Chapter IV, section 16 (2nd edition, English translation, 1932), "Arithmetical Treatment of the Problem of Equilibrium." Although there are no references in his book to Walras, a fact noted by Wicksell and Schumpeter, Cassel presented a Walrasian system, *without utility,* and organized its components in a way that would later suggest an approach to the existence question.

His production system considered r factors of production with "$R_1, R_2 \ldots R_r$, the quantities of them which are available in a given period" (Cassel, 1932, p. 142). There are n goods produced with a technology given by the technical coefficients a_{ij} where "to produce the unit quantity of commodity 1, the quantities $a_{11}, \ldots a_{1r}$ of the factors of production may be necessary" (p. 142). With factor prices $q_1, q_2, \ldots q_r$ and product prices $p_1, p_2, \ldots p_n$, we have Cassel's equations

$$
\begin{aligned}
a_{11}q_1 + a_{12}q_2 + \ldots + a_{1r}q_r &= p_1 \\
a_{21}q_1 + a_{22}q_2 + \ldots + a_{2r}q_r &= p_2 \\
&\vdots \\
a_{n1}q_1 + a_{n2}q_2 + \ldots + a_{nr}q_r &= p_n
\end{aligned}
\tag{3}
$$

so factor prices, and unit costs, determine product prices.[3]

Cassel went on to state that, once prices are known, demands for each commodity "can be calculated by means of the following series of equations:

$$
\begin{aligned}
D_1 &= F_1(p_1, \ldots p_n) \\
D_2 &= F_2(p_1, \ldots p_n) \\
&\vdots \\
D_n &= F_n(p_1, \ldots p_n)\text{" [p. 143].}
\end{aligned}
\tag{4}
$$

Cassel's "principle of scarcity," akin to a tendency to market clearing, was then

[1] In comments on earlier drafts of this paper, several readers raised questions like "You argue that the linear programming literature was important in the ultimate development of the existence proofs. Doesn't this suggest that constant returns to scale assumptions were (are?) necessary for competitive equilibria conclusions?" I have tried to avoid addressing such questions in this narrative.

One of the difficulties for a methodologist evaluating a line of inquiry in economics is that the *critical* faculties of the economist, and the *analytic* faculties of the methodologist, join to confuse the project when the economist is writing history from a methodological perspective. This essay, therefore, only attempts to get the narrative straight. I hope to address the critical questions at a later time.

[2] I do not mean to suggest by this that Cassel's book was as important as Walras' *Elements.* Rather the literature I shall examine is based on Cassel's treatment. In that sense *The Theory of Social Economy* is the natural overture to the acts which follow. (An English translation of the first edition appeared in 1923.)

[3] I shall, in this paper, identify equations with the numbers that appear in the original versions to preserve the integrity of quoted passages which refer to those equations.

invoked by asserting that "when prices are in equilibrium every demand must be satisfied by the supply" (p. 143) yielding

$$D_1 = S_1, D_2 = S_2, \ldots D_n = S_n, \tag{5}$$

"where S_1, S_2 . . . S_n are the quantities of each of the different commodities produced within a unit period" (p. 144).

Cassel, without explicitly defining a symbol for factor demands, next argued that knowing the quantities S_i "we can calculate the demands which are made upon the factors of production" (p. 144) as

$$\begin{array}{l} \text{the quantity } a_{11}S_1 + a_{21}S_2 + \ldots + a_{n1}S_n \\ \quad \text{of factor of production 1.} \\ \text{the quantity } a_{12}S_1 + a_{22}S_2 + \ldots + a_{n2}S_n \\ \quad \text{of factor of production 2.} \\ \quad \vdots \\ \text{the quantity } a_{1r}S_1 + a_{2r}S_2 + \ldots + a_{nr}S_n \\ \quad \text{of factor of production } r \text{ [p. 144].} \end{array} \tag{6}$$

Recalling that available factor supplies were denoted R_i, equilibrium (Cassel's "scarcity") requires:

$$\begin{array}{l} R_1 = a_{11}S_1 + a_{21}S_2 + \ldots + a_{n1}S_n \\ R_2 = a_{12}S_1 + a_{22}S_2 + \ldots + a_{n2}S_n \\ \vdots \\ R_r = a_{1r}S_1 + a_{2r}S_2 + \ldots + a_{nr}S_n \text{ [p. 144].} \end{array} \tag{7}$$

Cassel's system was then argued through to equilibrium by suggesting that, given a set of factor prices, product prices were determined by (3). This yields demands from (4) and thus supplies from (5). Supplies determine factor demands from (6) and "the coincidence of these requirements with the available quantity of factors of production is guaranteed by equations (7)" (p. 145).

There are two points to notice. First, since Cassel specifically restricted his argument to goods and factors which were "scarce," he necessarily thought of factors as having positive factor prices so that, since $a_{ij} > 0$, all product prices were non-negative. Second, although he *argued* the solution by an iterative or causal chain (he indeed had as many equations as unknowns) he did *not* argue that a solution exists because of this equality. Indeed, he did not count equations at all. (And later in the section he argued that the functions F_i are, in effect, homogeneous of degree zero in prices and income, so that careless equation counting would have produced the wrong answer for existence of a relative price equilibrium.)

It is not usually recognized that Cassel's discussion of this system, on pp. 152–55, was then extended to "the society which is progressing at a uniform rate. In it, the quantities of the factors of production which are available in any period, that is our $R_1, \ldots R_r$, are subject to a uniform increase. We shall represent by c the fixed rate of this increase, and of the uniform progress of the society generally" (p. 152). The result is that, for his given system, (3) and (7) must be modified for "as production is now assumed to increase uniformly, there must be substituted, for these unit quantities, other quantities which steadily increase in the percentage c. . . . [If] a series of successive unit periods are considered, they must be multiplied by ascending powers of a constant factor, which is clearly determined by c" (p. 153). For this dynamic, albeit uniformly growing, society, Cassel "talked through" his equations again to produce an equilibrium which now involved a new relationship: "The ratio between the two parts [reproduction sector and real increase of capital sector] determines the degree of saving and the rate of progress c" (p. 154). A rate of interest had appeared.

It is not necessary to refocus Cassel's argument. What is important is to recognize that Cassel's statement of the pricing problem, or the determination of prices by a system involving interrelated supply and demand in product and factor markets, was textbook knowledge prior to 1930, especially in those European countries where written German could be understood. S. L. Barron of the London School of Economics prepared an English translation for Harcourt Brace from the fifth German edition, and that translation appeared in the United States in 1932. I do not claim, nor do I believe it to be true, that Cassel's book represented an analytical improvement on Walras or Pareto. In a real sense, *The Theory of Social Economy* was a text that could have been, and was, used much as Marshall's *Principles* was used by teachers and students. As we shall see, it was Cassel's formulation which spurred developments in the 1930s. We may thus begin the narrative by assuming that Cassel's presentation of the Walrasian general equilibrium system, modified by excluding utility considerations, was available for study by any economist interested immediately prior to the early 1930s.

III. *Menger's Vienna Colloquium*

Wittgenstein's Vienna, by Janik and Toulmin (1973), is a charming, detailed, and well-argued intellectual and cultural history of Vienna prior to the First World War. In this book the authors identify the social, political, and philosophical milieu which formed the backdrop for Ludwig Wittgenstein's philosophical contributions. I am not aware, however, of a similar full-length history of Vienna in the late 1920s and 1930s, a period of intense activity in mathematics, philosophy, and economics.

Alfred J. Ayer's autobiography, *Part of My Life,* notes the impact of Vienna at this time on a young English philosopher who had letters of introduction to Moritz Schlick and thus to the center of philosophical activity. The group of philosophers, mathematicians, and scientists "had come into being in the late 1920s. Its manifesto: *Wissenschaftliche Weltauffassung: Der Wiener Kreis*—The Scientific View of the World: The Vienna Circle—was published in 1929 . . ." (Ayer, 1977, p. 129). The tradition of Ernst Mach was carried on by Schlick, Otto Neurath, Rudoph Carnap, Friedrich Waismann, together with the mathematicians "Menge [sic] and Hahn" (ibid. p. 133) and Kurt Gödel. The Polish logicians, particularly A. Tarski, and the Berlin philosophers Hans Reichenbach, Richard von Mises, and Carl Hempel, maintained close relations to the Circle—Ayer and W. V. Quine were the most distinguished overseas visitors.

As Ayer notes, "one of the principal aims of the Vienna Circle was to rebuild the bridge between philosophy and science which had been largely broken by the romantic movement and the accompanying rise of idealist metaphysics at the beginning of the nineteenth century" (p. 129). The related view of the centrality of science to philosophy, and the premier role that mathematics must play in philosophy (and science) can be said to have infused the intellectual life of all who participated, and specifically Karl Menger.

Menger was, at that time, a professor of mathematics at the University of Vienna. The son of the distinguished economist Carl Menger, he played an important role in Central European mathematics, since there were few professorial positions, and Vienna was an intellectual magnet for the generation of gifted mathematicians of the time. As S. M. Ulam, in his own autobiography, *Adventures of a Mathematician,* wrote: "My plans were to go west (go west, young man!); first I wanted to spend a few weeks in Vienna to see Karl Menger, a famous geometer and topologist, whom I had met in Poland through Kuratowski" (p. 56).

As Ulam stresses repeatedly, there was a burgeoning of mathematical activity in this period of the mid-1920s to mid-1930s in Central Europe. The Poles included Banach, Kuratowski, Schauder, Borsuk, Mazur, Tarski, Steinhaus, Kac, Lomnicki, and of course Ulam. The Hungarians included John von Neumann, and the physicists E. P. Wigner and Edward Teller. And Vienna attracted them all.

As Menger later recalled, "In the fall of 1927 a man of 25 called at the Mathematical Institute of the University of Vienna. Since he expressed a predilection for geometry he was referred to me. He introduced himself as Abraham Wald" (Menger, 1952, p. 14). As a Jew in Cluj, Rumania, "Wald was not admitted to the local gymnasium . . . He studied by himself and was admitted to the University of Cluj" (Wolfowitz, 1952, p. 1). Upon graduation, Wald came to Vienna.

"Wald enrolled in the university, but during the next two years Vienna did not see much of him" (Menger, 1952, p. 14) because of the lack of formal course restrictions, the freedom to learn on one's own and, for Wald, service in the Rumanian army. "It was not until February 1930 that he and I again had extended conversations. Then he came unexpectedly to hand me a manuscript which purported to contain the solution of a famous problem" (ibid.). There was a serious error in the paper, but Wald persevered, asking for other problems and topics to explore. He had some success.

> It seemed to me that Wald had exactly the spirit which prevailed among the young mathematicians who gathered together about every other week [alternating weeks with the Wiener Kreis?] in what we called our Mathematical Colloquium, so I at once invited him to present his results there. Gödel and Nöbeling, Alt and Beer . . . [were regulars and] Čech, Knaster, and Tarski were frequent guests . . . [together with] students and visitors . . . from abroad, especially the United States and Japan. It was in this stimulating atmosphere that Wald spent his formative years [Menger, op. cit. p. 15].

Writing after Wald's death Menger recalled (and understated) his own act of great personal decency.

> [Wald] received his Ph.D. in 1931. At that time of economic and incipient political unrest, it was out of the question to secure for him a position at the University of Vienna, although such a connection would certainly have been as profitable for that institution as for himself. Outside of the Colloquium, my friend Hahn was the only mathematician who knew Wald personally . . . Wald, with his characteristic modesty, told me that he would be perfectly satisfied with any small private position which would enable him to continue his work in our Mathematical Colloquium. I remembered that my friend Karl Schlesinger, a well-to-do banker and economist, wished to broaden his knowledge of higher mathematics, so I recommended Wald to him [Menger, op. cit. p. 18].

Karl Schlesinger had been born in Budapest in 1889 and moved to Vienna after Bela Kun's communist revolution in 1919. He was the real link between Walras, the progenitor of general equilibrium analysis, and the nascent developments. As Oskar Morgenstern has remarked, "Schlesinger's *Theorie der Geld-und Kreditwirtschaft* (1914) made him the only immediate follower of Walras, other than Wicksell, to advance Walras' theory of money" (1968, in Morgenstern, 1976, p. 509). In addition to developing a theory of the indirect utility of money, "Schlesinger derived an excess demand equation for money that is virtually identical with the one commonly ascribed to Keynes. He was also probably the first to develop the notion of the equilibrium rate of interest" (ibid).

Although Don Patinkin (1965, pp. 576–78) gives Schlesinger appropriate recognition for his contributions to monetary economics, few others have appreciated his work, leading Schumpeter to write that Schlesinger's book is a "striking [instance] of the fact that in our field first-class performance is neither a necessary nor a sufficient condition for success" (Schumpeter, 1954, p. 1082 n.).

Morgenstern notes that "In his 1914 book Schlesinger made extensive use of some simple mathematics, uncommon at that time in German economic writing . . . A wealthy financier and a member of many industrial and financial boards . . . he never held an academic post but was an active and highly respected member of the Vienna Economic Society" (Morgenstern, op. cit. p. 509). Indeed, the 1934 volume of *Econometrica* lists Schlesinger as a member of the fledging Econometric society, whose Viennese business may have been conducted "frequently at odd hours in coffee houses . . ." (Morgenstern, op. cit. p. 510).

It is quite clear that the association between Schlesinger and Wald was educational on both sides, and it is likely that the banker used Cassel's newly revised *Theory of Social Economy* as a touchstone for the mathematical discussions. We can surmise that Menger kept track of the pair's progress because he reports that "I asked [1931?] Schlesinger to present his formulation of the equations [of economic production] to the Colloquium" (Menger, op. cit. p. 18). Menger goes on to refer to ". . . Schlesinger's modification of the original equations of Walras and Cassel" (ibid. p. 18).

It was probably an early version of Schlesinger's paper *"Über die Produktionsgleichungen der ökonomischen Wertlehre"* ("On the Production Equations of Economic Value Theory") that formed the Colloquium presentation; the final paper appeared in the 1933–1934 edition of the Proceedings (*"Ergebnisse"* in German) of that Colloquium.

Recalling Cassel's notation, let r_i denote "available . . . units of input R_i." Assume m inputs and n outputs S_i where s_i is the amount of S_i produced. Schlesinger defined input prices as ρ_i and output prices as σ_i.

Schlesinger thus produced the equations:

$$
\begin{aligned}
r_1 &= a_{11}s_1 + a_{12}s_2 + \ldots + a_{1n}s_n \\
r_2 &= a_{21}s_1 + a_{22}s_2 + \ldots + a_{2n}s_n \\
&\vdots \\
r_m &= a_{m1}s_1 + a_{m2}s_2 + \ldots + a_{mn}s_n \\
\sigma_1 &= a_{11}\rho_1 + a_{21}\rho_2 + \ldots + a_{m1}\rho_m \\
\sigma_2 &= a_{12}\rho_1 + a_{22}\rho_2 + \ldots + a_{m2}\rho_m \\
&\vdots \\
\sigma_n &= a_{1n}\rho_1 + a_{2n}\rho_2 + \ldots + a_{mn}\rho_m \\
\sigma_1 &= f_1(s_1, s_2, \ldots s_n) \\
\sigma_2 &= f_2(s_1, s_2, \ldots s_n) \\
&\vdots \\
\sigma_n &= f_n(s_1, s_2, \ldots s_n)
\end{aligned}
$$
[pp. 278–79].

Schlesinger noted that both Heinrich von Stackelberg (1933) and Hans Neisser (1932) had "observed that these equations do not necessarily possess a solution, and above all, do not necessarily possess a solution whose values are positive (as is required for it to represent the values of ρ_i, σ_j, and s_j)" (Schlesinger, p. 279). The problem he identified was that Walras and Cassel used the R_i to refer only to "scarce" inputs. Yet scarcity is simply not exogeneous, being "in turn dependent on demand curves, technical production possibilities, etc." (ibid.).

The short paper then argued that for scarce inputs,

$$r_i = a_{i1}s_1 + a_{i2}s_2 + \ldots + a_{in}s_n \text{ and } \rho_i > 0$$

while for free inputs,

$$r_j \geqq a_{j1}s_1 + a_{j2}s_2 + \ldots + a_{jn}s_n \text{ and } \rho_j = 0$$

Thus the first m equations must be replaced by

$$r_i = a_{i1}s_1 + a_{i2}s_2 + \ldots + a_{in}s_n + u_i$$

(for $i = 1, 2, \ldots m$) where, $u_i \geqq 0$ and if $u_i > 0$ then $\rho_i = 0$ (for $i = 1, 2, \ldots m$).[4]

In effect, the $m + 2n$ Casselian equations are replaced by "$m + 2n$ equations and m side conditions in $2m + 2n$ unknowns, u_i, σ_j, s_j, r_i ($i = 1, \ldots m$; $j = 1, \ldots n$)" (ibid.).

There are two important points here. First, equilibrium has been characterized by inequalities and equations, so that arguments based on "as many unknowns as equations" break down. Existence of equilibrium is problematical. Second, the complementary slackness conditions, of the

[4] u_i is a "slack" variable. It measures the discrepency between the two sides of an inequality.

later programming literature, are fully defined.[5] It is not stretching imagination to attribute the conciseness, and elegance, of this note to the help of the Menger Colloquium and Wald, although the idea is certainly Schlesinger's. (But see Schlesinger's footnote, p. 279, citing a similar argument by F. Zeuthen.)

There is, however, a conundrum. Cassel wrote the demand relationship as $D_i = F_i(p_1, \ldots p_n)$. Schlesinger writes it as $\sigma_i = f_i(s_1, \ldots s_n)$. The former expresses demand quantities as functions of prices, the latter has demand prices as functions of quantities. I have no hypothesis about the reasons for this change from a "Walrasian" to a "Marshallian" demand relationship. Mathematically, if the Jacobian

$$\left(\frac{\partial f_i}{\partial s_j}\right)$$

is everywhere invertible, then the inverse function theorem could produce a simple local translation from one system to the other. Alternatively, Schlesinger's formulation allows "eliminating" the σ_i unknowns at an early stage of the conceptual argument, but there seems to be an information loss in this procedure. In any event, it was this system, using inverse Walrasian demand functions, which formed the basis for Wald's subsequent analysis of the existence of equilibrium.

It is hard to escape the presentiment that this analysis was "planned" by Menger since non-negativity, as Morgenstern wrote, "was only part of a wider interest, felt especially by Menger, namely, that the practice of the mere counting of equations and unknowns, which had satisfied economists up to that time, had to be overcome by the actual demonstration of whether or not such systems have a solution" (1951, in Morgenstern, 1976, p. 494). Yet Menger, by later admission, was on his own approaching the problem incorrectly.

> I had frequently discussed the problem with members of the Mathematical Colloquium . . . but I must confess I was on the wrong track. I believed imputing the price of a product to its factors to be somewhat analogous to finding the distribution of the weight of a horizontal plate over its various points of support. In statics this problem is insoluble if the plate is supported at more than three points, since statics supplies only three linear equations . . . In constructing bridges and the like . . . the support is not rigid and by supplementing the static considerations with the theory of elasticity one indeed obtains a unique solution of the (nonlinear) problem. I asked myself whether there was perhaps an economic analogue of the elasticity considerations [Menger, 1973, p. 47].

Yet it was the economist Schlesinger who, by recasting the Walras-Cassel system with complementary slackness conditions, led the way to the existence theorem that Wald developed. A little thought suggests that, if a solution to the Schlesinger system may be obtained, its properties are rooted in the non-negativity conditions and the restrictions on the functions f_i. "The great achievement of Wald was the proof . . . of the unique solution . . . provided that the functions . . . connecting the prices of the products with the quantities produced satisfy certain conditions implied by the Principle of Marginal Utility" (Menger, ibid. p. 51). We can speculate on the pleasure this approach must have provided to the son of Carl Menger.

Wald actually wrote four papers on the subject of the existence of an equilibrium for the (modified) Walras-Cassel system. The first appeared in print in the *Ergebnisse* (the Menger colloquium's proceedings) of March, 1934, as *"Über die eindeutige positive Lösbarkeit der neuen Productionsgleichungen* (I)" ("On the Unique Non-negative Solvability of the New Production Equations, Part I"). The paper is completely formal, beginning with the

[5] Such conditions entail that if a certain inequality is non-binding, a related variable equals zero.

Theorem: The equation system (Sch) [Schlesinger]

$$r_i = \sum_{j=1}^{n} a_{ij}s_j + u_i \qquad (i = 1,. . .m),$$

$$\sigma_j = \sum_{i=1}^{m} a_{ij}\rho_i,\ \sigma_j = f_j(s_j) \qquad (j = 1,. . .n)$$

in which the r_i and a_{ij} are given quantities, the f_j are known functions, the u_i, ρ_i, s_j, σ_j are unknown quantities, possesses a single valued solution set in the values u_i, s_j, σ_j when the following conditions hold:

1. $r_i \geqq 0$ $(i = 1,. . .m)$.
2. $a_{ij} \geqq 0$ $(i = 1,. . .m; j = 1,. . .n)$.
3. For each j $(j = 1,. . .n)$ there is at least one i $(i = 1,. . .m)$ for which $a_{ij} \neq 0$.
4. For each of the values $j = 1,. . .n$, the function $f_j(s_j)$ is defined for every positive value of s_j, its value is non-negative, continuous, and strictly monotone decreasing, i.e., $s_j' < s_j$ implies $f_j(s_j') > f_j(s_j)$, and in addition $\lim_{s_j=0} f_j(s_j) = \infty$, provided that the following side conditions also hold:
   (a) $s_j \geqq 0$ $(j = 1,. . .n)$
   (b) $\sigma_j \geqq 0$ $(j = 1,. . .n)$
   (c) $\rho_i \geqq 0$ $(i = 1,. . .m)$
   (d) $u_i \geqq 0$ $(i = 1,. . .m)$
   (e) if $u_i > 0$ then $\rho_i = 0$ $(i = 1,. . .m)$

[Wald in Baumol and Goldfeld, 1968, p. 281].

The fundamental feature (and limitation) of the theorem[6] is the simplification introduced by assuming that the demand price for good j is a function only of the quantity of good j. All other goods quantities have no effect on good j's price, and good j has a downward sloping demand curve. The proof itself is tedious; it is a mathematical induction argument on n, the number of goods, and involves a "trial" solution of the r_i's, which from the linear equalities and side conditions generate sequences of r_i's, ρ's, u's, and σ's; then delicate continuity arguments develop into a proof by contradiction. The inductive step is even more tedious,[7] although Wald makes clever use of the concept, later explicitly introduced by Hicks, of a composite commodity.

This paper is an excellent example of what one of Wald's later collaborators noted about Wald's lecture notes. "They are rigorous, accurate, and clear, but some of the proofs are clumsy, and the organization . . . could be improved . . . Wald seldom bothered to rework his writings for mathematical elegance or clarity—only new results interested him . . . He seldom gave an intuitive justification of the theorems, probably because he himself needed it so little" (Wolfowitz, 1952, p. 3).

The paper, as it appeared, was followed by comments by Schams, who noted that Wald had introduced a value-theoretic premise into Cassel's equations, and by Menger, whose closing remark was prophetic:

> In any event I wish to remark in conclusion that with Wald's work we bring to a close the period in which economists simply *formulated* equations, without concern for the existence or uniqueness of their solutions, or at best, made sure that the number of equations and unknowns be equal (something that is neither necessary nor sufficient for solvability and uniqueness). In the future as the economists formulate equations and concern themselves with their solution (as the physicists have long done) they will have to deal explicitly with the deep mathematical questions of existence and uniqueness [Menger, in Wald, 1935; in Baumol and Goldfeld, 1968, p. 288].

In November of 1934 Wald published *"Über die Produktionsgleichungen der Ökonomischen Wertlehre (II)"* ("On The Production Equations of Economic Value Theory II") which replaced the demand functions of the March paper with the Schlesinger functions $\sigma_j = f_j(s_1, s_2, . . . s_n)$, $(j = 1,2,. . .n)$.[8]

[6] Conditions 1–2 are non-negativity constraints, as are conditions 4(a)–4(c). Conditions 4(d) and 4(e) are the complementary slackness restrictions.

[7] In a mathematical induction argument one proves the proposition for $n = 1$, and from the assumption that the proposition is true for $n = k$, one proves its truth for $n = k + 1$ thus establishing the result for all n. An existence proof by contradiction, moreover, provides no hint of how the assumptions "work" to guarantee existence.

[8] It is worth remembering that Wald's demand functions refer to the market demand, not individual demands.

This paper replaced the assumption (4) about the monotonicity of $f_j(s_j)$ with the following assumption:

> Let $\Delta s_1,\ldots \Delta s_n$ be n numbers among which at least one is < 0, and let
>
> $$\sum_{j=1}^{n} \sigma_j \Delta s_j \leqq 0, \tag{6}$$
>
> then we must have
>
> $$\sum_{j=1}^{n} \sigma_j' \ \Delta s_j < 0$$
>
> where $\sigma_j' = f_j(s_1 + \Delta s_1, \ldots s_n + \Delta s_n)$ $(j = 1, 2, \ldots n)$. [Wald, 1935, in Baumol and Goldfeld, 1968, p. 290].

The proof is a simple generalization of the earlier proof. Most significant, however, are the remarks on assumption (6). Wald states:

> Let w be any member of the economy who, when prices are $\sigma_1, \ldots \sigma_n$ demands s_{w1} of $S_1, \ldots$ and s_{wn} units of S_n. The amount s_j—the number of units of S_j produced [in equilibrium]—is the sum of the amounts s_{wj} for all members of the economy, W . . . [Now suppose] ($6w$) if at prices $\sigma_1, \ldots \sigma_n$ individual w demands $s_{w1}, \ldots s_{wn}$, and with prices $\sigma_1', \ldots \sigma_n'$ he demands $s_{w1} + \Delta s_{w1}, \ldots s_{wn} + \Delta s_{wn}$ where at least one of the $\Delta s_{wj} < 0$ and where [if]
>
> $$\sum_{j=1}^{n} \sigma_j \Delta s_{wj} \leqq 0$$
>
> then
>
> $$\sum_{j=1}^{n} \sigma_j' \Delta s_{wj} < 0$$
>
> [ibid., p. 292].

Wald then remarks that, although ($6w$) holds for every individual, it may happen that (6) is not true. It is clear that ($6w$) is the weak axiom of revealed preference, refined by Samuelson in 1938. Wald requires, however, (6) or ($6w$) to hold in the aggregate. It may not. The weak axiom of revealed preference is an extremely strong assumption.[9]

Even more interesting, however, is Wald's final remark, in which he states that ($6w$) in the aggregate can be derived from the assumption that, if ϕ denotes marginal utility, "for every j the number $\partial\phi_{wj} / \partial s_{wj}$ is negative and that it is large in comparison with the number $\partial\phi_{wj} / \partial s_{wk}$ for $k \neq j$" (ibid., p. 293). This is similar to the restrictions that appear in a version of the "dominant diagonal theorem" discovered over two decades later in the stability of equilibrium literature. The effect of that theorem is that multimarket interaction can be conceptually identified with partial equilibrium, in which neither uniqueness nor stability is of great moment.[10] The power of Wald's intuition is thus evident, for strong forms of the uniqueness of equilibrium theorems of the 1950s required the assumption of aggregate revealed preference.

One cannot leave the discussion of this paper without citing the printed comment by Kurt Gödel: "In reality the demand of each individual depends also on his income, and this in turn depends on the prices of the factors of production. One might formulate an equation system which takes this into account and investigate the existence of a solution" (ibid., p. 293).[11]

[9] If p is a price vector, and $E(p)$ is an n-market set of excess demand functions, and $p = p^*$ is equilibrium, then the Liapunov function $V = \sum_i (p_i - p_i^*)^2$ is differentiated as $2\Sigma \dot{p}_i(p_i - p_i^*)$. If the tâtonnement is given by $\dot{p}_i = E_i(p)$, then $\dot{V} = 2\Sigma p_i E_i(p) - 2\Sigma p_i^* E_i(p)$. The first term is zero by Walras Law. If the weak axiom holds in the aggregate, the second summation is positive, so $\dot{V} < 0$, so equilibrium is stable. This is the strongest stability theorem extant, and it comes immediately from the unreasonable strength of the aggregate weak axiom.

[10] Conceptually, the dominant diagonal theorem reduces the multimarket interaction problem $\dot{p} = Ap$ to the problem $\dot{p} = (\text{diag } B)p$ where the $n \times n$ matrix A is reduced to the diagonal matrix B. For the latter problem, price *change* for good i depends only on the price of good i, *not* on other goods' prices. The multimarket linkages are effectively broken—partial equilibrium analysis gives acceptable results.

[11] This suggested generalization of Wald's system was not feasible. In a comment to the author on an earlier draft, Lionel McKenzie noted that "Walras had written his demand functions with factor prices as well as goods prices as arguments. How these could be turned into Wald's inverse demand functions is far from obvious. Somehow income would

Wald wrote two other papers on this subject of equilibrium. One, on equilibrium in an exchange economy, was mentioned by title only in the final issue of the *Ergebnisse* (1935–1936, p. 84). Chipman, in his classic "A Survey of the Theory of International Trade, Part 2," quotes a letter from Morgenstern which states, in part, "The paper . . . was written but is lost. Probably Wald himself lost it when coming to this country and never bothered to rewrite it . . ." (reprinted in Baumol and Goldfeld, 1968, p. 270).[12]

On the fourth and final paper, Morgenstern writes: "In view of the significance [of the three papers] I persuaded Wald to write an expository article . . ." (1951, in Morgenstern, 1976, p. 494). This paper, titled *"Über einige Gleichungssysteme der Mathematischen Ökonomie,"* appeared in 1936 in the *Zeitschrift für Nationalökonomie,* and was translated by Otto Eckstein in 1951 for *Econometrica* as "On Some Systems of Equations of Mathematical Economics."

This monumental, and clear, survey paper reviewed the theorems (but not the proofs) of the two published papers and took great care to develop the ideas and intuition behind the various assumptions. There is a full discussion, for example, of the "revealed preference" argument. This initial section of the paper ends with a passage that has not, I believe, been previously noted:

> . . . it is assumed that nothing is saved, and hence the problem of capital formation and of the rate of interest is not treated at all . . . ; second, it is assumed that the production of a unit of S_j is technically possible by one method only . . . In a later note the author will treat a corresponding dynamic system of equations in which capital formation and the interest rate will be considered, and in which the technical coefficients will be assumed to be variable. The solvability of this system will then be examined [Wald, 1951, p. 379].

Section II of this paper dealt with the "equations of exchange." Wald supposes indifference curves are given by differential equations; the first order conditions and budget equations appear, and Walras' Law is used to reduce the number of independent equations by one. The assumptions that no individual holds negative stocks, there are positive stocks of each good, each individual has a positive endowment, and diminishing marginal utility prevails are claimed, by Wald, to insure a competitive exchange equilibrium as long as the marginal utility of a good is independent of the amount held of other goods; substitutes *and* complements are excluded. These demand restrictions are very strong. Wald takes Walras to task by presenting Walras' own discussion and interjecting "A rather vague argument!" (p. 384). It is indeed a tragedy that the proofs for the theorems of this paper never appeared because of the forced closing of the *Ergebnisse.*

The final substantive section of this 1936 paper concerned "Existence and Stability of Equilibrium in Cournot's Duopoly" and contained, aside from some very uninteresting specific reaction functions, an early and correct use of the equilibrium-stability distinction. Wald wrote:

have to be specified, even for outputs which are not producible. The only way to make sense of Wald's demand functions is to fix income at unity, say, and invert the ordinary demand functions $x_i = d_i(p_1, \ldots p_n, 1)$.

However this does not work if there is more than one consumer, since it does not reflect changes in income distribution which will accompany price changes. In other words, to make sense of the demand functions it seems necessary to anticipate the solution of the whole system, or else to suppose there is only one consumer" (April 16, 1982).

[12] In a letter to the author (June 4, 1982), Kenneth Arrow notes that "There is an historical puzzle here, which may not be solvable with our data. The results on the equilibrium in an exchange economy do not seem provable with the methods Wald used earlier. In the *Zeitschrift* paper, [Wald] refers to these results and states that they require modern results in mathematics. This hints at the possibility that he did use a fixed point theorem. Gerard Debreu and I will try to consider this problem further. However, since the manuscript is lost, we shall probably never know."

> We define an equilibrium point D to be stable if, with a sufficiently small, but otherwise arbitrary, departure of the supplies of the two producers from the equilibrium point D, *the reaction mechanism* [with my italics added] again leads to the equilibrium point D. [1951, p. 392].

Morgenstern's advice to Wald, to do an expository survey, had not been gratuitous. In 1933, shortly after Wald had done his two initial papers, Menger introduced Wald to "Oskar Morgenstern who was then director of the *Institut für Konjunkturforschung*. Morgenstern appreciated Wald's talents and increasingly employed Wald in his institute" (Wolfowitz, 1952, p. 2). As Morgenstern recalled, "[Out] of an arrangement made at first only for convenience—he getting a modest subsidy from the Institute for advising on a variety of minor statistical difficulties such as were to be expected in an economic research unit—I developed a strong desire to interest him genuinely and more fully in economics and statistics" (1951, in Morgenstern, 1976, p. 493). Morgenstern was persuasive. In 1936 Wald published a book on the analysis of seasonal variations of time series, *Berechnung und Ausschaltung von Saisonschwankungen* (*Beiträge zur Konjunkturforschung*, Vol. 9), and thus developed the interest in statistical theory that increasingly drew him away from mathematical economic theory. Wald continued, however, to attend the Menger Colloquium and indeed was a coeditor with Gödel and Menger for issue number 7 of the *Ergebnisse;* he had three mathematical pieces in the final issue 8 (1937) which contained the remarkable paper by John von Neumann, *"Über ein Ökonomisches Gleichungssystem und eine Verallgemeinerung des Brouwerschen Fixpunktsatzes."*

IV. *The von Neumann Connection*

John von Neumann was born in 1903 in Budapest to a well-to-do family. Privately educated prior to gymnasium, he showed early mathematical talent, received private tutoring, and "was already recognized as a professional mathematician" (Ulam, 1958, p. 2) before his matriculation at the University of Budapest. Although enrolled there as a mathematics student, he instead took courses at the *Eidgenössiche Technische Hochschule* in Zurich in chemistry, returning to Budapest only to take exams at the end of each semester. "He received his doctorate in mathematics in Budapest at about the same time as his chemistry degree in Zurich" (ibid.).

In 1927 he became a *Privat Dozent* at the University of Berlin, and held that position for three years. "[During] that time [he] became well known to the mathematicians of the world through his publications in set theory, algebra, and quantum mechanics" (ibid.).

It was a paper submitted in July 1927, and published in 1928 in *Mathematische Annalen* (*100*, pp. 295–320), titled *"Zur Theorie Der Gesellschaftsspiele"* ("The Theory of Games") that initiates an economist's interest in von Neumann. That paper (translated in *Contributions to the Theory of Games, IV,* A. W. Tucker and R. D. Luce, eds., Princeton University Press, 1959) contains an articulation of games with finitely many strategies and the first proof of the min-max theorem. The proof used a fixed-point argument to establish the existence of a saddle point for a function

$$h(\xi,\eta) = \sum_{p=1}^{M+1} \sum_{q=1}^{N+1} \alpha_{pq}\xi_p\eta_q$$

where the α are constants and ξ_p and η_q are vertices of appropriate dimensioned simplexes[13] (1928, in von Neumann, 1963,

[13] A simplex in R is the point 1. In R^2 it is the line formed by joining (0,1) and (1,0). In R^n it is the set of points $\{x: x_i \in [0,1] \text{ and } \sum_{1=1}^{n} x_i = 1\}$. A simplex in R^n is thus an R^{n-1} dimensional object.

p. 13). We shall return to this paper later when discussing the theory of games. For the present, it suffices to note that the min-max theorem has, as a context, certain "dual" systems of inequalities with explicit non-negativity constraints on the ξ_p and η_q (somewhat masked by their interpretation as weights on the vertices of a simplex). It can be assumed that this paper of von Neumann's was known to mathematicians by 1930.

In that year von Neumann accepted a visiting professorship at Princeton, which was made permanent in 1931. In 1933 he accepted an invitation to join the Institute for Advanced Study as a professor, where he remained through his mathematical career. During summers, through the 1930s, von Neumann travelled to Europe to seminars, conferences, and mathematical meetings. It is certain that he passed through Vienna and visited Menger's seminar. (It is also certain, though, that he and Morgenstern did not meet until 1939. Morgenstern, 1976, p. 807.)

Von Neumann apparently presented a talk to a Princeton mathematics seminar in the winter of 1932 on equilibrium in a dynamic economy.* A final version of this paper appeared in 1937 as the last paper of the last *Ergebnisse* issue (*Heft* 8, 1935–1936); it was also presented to the Menger Colloquium in 1937 (ibid., p. 807) and Wald, a co-editor of the issue, may have played a role in its polishing. In any event this paper, *"Über ein ökonomisches Gleichungssystem und eine Verallgemeinerung des Brouwerschen Fixpunktsatzes"* ("On an economic equation system and a generalization of the Brouwer Fixed Point Theorem")[14] compels our attention.

Von Neumann's paper is, in my view, the single most important article in mathematical economics. As Tjalling Koopmans noted in 1964, "The paper contains the first explicit statement . . . of what has been subsequently called the activity analysis model of production . . . [further its main purpose was to] exhibit a model of competitive equilibrium . . . [and] the paper contains the first rigorous, formal, and fully explicit model in non-aggregative capital theory. . ." (Koopmans, 1964, p. 356). Yet Koopmans claims too little. The paper also contains the first use in economics of certain, now common, tools: explicit duality arguments, explicit fixed-point techniques for an existence proof, and convexity arguments.

The paper assumes n goods $G_1, \ldots G_n$ and m processes $P_1, \ldots P_m$ so, if the processes are linear,

* I have recently been told the following story by Axel and Earlene Leijonhufvud who, in the course of their oral history project on emigré economists, had been told it by the late Jacob Marschak.

During the late 1920s (approximately 1928) Leo Szilard organized several mathematicians and physicists in Berlin into an informal study group to hear lectures about the role of mathematics in other disciplines. Marschak was asked to give such a lecture about economics. He talked about the Walrasian (Cassellian?) equations of general equilibrium and apparently noted some problems about free goods. One of the mathematicians became extremely agitated, and began a stream of interruptions, arguing that instead of equations the equilibrium relationships should be described by inequalities. That mathematician was von Neumann.

This story, told by Marschak, suggests that the genesis of von Neumann's *Ergebnisse* paper was quite specific and roughly contemporary with von Neumann's paper on game theory. The min-max idea, the duality ideas, and the fixed-point theorem strategy of proof were used in each paper. The papers appear, then, to be naturally related not only by content, but by place of origin.

[14] This article was translated for the *Review of Economic Studies* in Volume 13, 1945–1946 by George Morgenstern (who later changed his last name to Morton) as "A Model of General Economic Equilibrium." The printing was terrible. The *Ergebnisse* date is wrong. The translator (?) changed the name of the economy from W to E, making the second reference to the "splitting" of W a total mystery. It is in this form that the paper is reprinted in both von Neumann's *Collected Works, Volume VI,* and in Peter Newman's *Readings in Mathematical Economics, Volume II.* Although Baumol and Goldfeld's *Precursors in Mathematical Economics* reprints with the corrections for the *Ergebnisse* date, the mysterious W remains to plague even an attentive reader.

$$P_i: \sum_{j=1}^{n} a_{ij}G_j \longrightarrow \sum_{j=1}^{n} b_{ij}G_j \text{ where } a_{ij}$$

is "used up" and b_{ij} is "produced.[15] If x_i is the intensity of the i^{th} process, and y_j is the price of good j, while the economy expands at a rate α, and β is the interest factor, then the equations of the economy are (using von Neumann's numbering scheme):

(3) $x_i \geqq 0$

(4) $y_j \geqq 0$

(5) $\sum_{i=1}^{m} x_i > 0$

(6) $\sum_{j=1}^{n} y_j > 0$

(7) $\alpha \sum_{i=1}^{m} a_{ij}x_i \leqq \sum_{i=1}^{m} b_{ij}x_i$

(7′) where, if $<$, then $y_j = 0$ and

(8) $\beta \sum_{j=1}^{n} a_{ij}y_j \geqq \sum_{j=1}^{n} b_{ij}y_j$

(8′) where, if $>$, $x_i = 0$.

Von Neumann's model[16] thus assumes "that there are constant returns (to scale); . . . that the natural factors of production, including labour, can be expanded in unlimited quantities . . . [and] consumption of goods takes place only through the processes of production which include the necessities of life consumed by workers and employees. In other words we assume that all income in excess of necessities of life will be reinvested" (von Neumann, 1945–1946, p. 2).

Von Neumann also assumes that $a_{ij} + b_{ij} > 0$ to prevent the break-up of the economy into "sub-economies."

[15] Thus the process P_i "maps" or transforms the first sum to the second sum. a_{ij} is thus the amount of good G_j used up in process P_i operating at unit intensity, while b_{ij} is the amount of good G_j produced by that same process. Modern usage suppresses the letters P and G.

[16] Thus intensities x_i and prices y_j are dual variables. Assumptions (3) and (4) are non-negativity restrictions, (5) and (6) are viability assumptions, and (7) and (7′), and (8) and (8′) are complementary slackness conditions for the dual inequality systems.

The proof that there exist x's and y's satisfying (3) – (8′) is instructive. First replace (3) and (5) by (3′) and (5′) where x' replaces x, and replace (4) and (6) by (4′) and (6′) with y' replacing y. Now define

$$\phi(X', Y') = \frac{\sum_{i=1}^{m}\sum_{j=1}^{n} b_{ij}\, x_i'\; y_j'}{\sum_{i=1}^{m}\sum_{j=1}^{n} a_{ij}\, x_i'\; y_j'}$$

where x_i' and y_j' are "variables." Von Neumann shows that a solution to the original system exists if, and only if, for X a vector of x's and Y a vector of y's,

> $\phi(X, Y')$ assumes its minimum value for Y' if $Y' = Y$ [and] (7**)
> $\phi(X', Y)$ assumes its maximum value for X' if $X' = X$ [ibid. p. 5]. (8**)

Thus the existence of an equilibrium is equivalent to the existence of a saddle-point of $\phi(X', Y')$ and, at an equilibrium (saddle-point), $\alpha = \beta = \phi(X, Y)$.

Homogeneity in X and Y allows the x_i and y_i to be elements of the m and n dimensional simplexes; the vector $(X, Y) = (x_1, \ldots x_m; y_1, \ldots y_n)$ lives in R^{m+n}. The problem is really to show that the x's which solve (8**) for given y's can be used to generate y's which solve (7**) for given x's. Thus if "V = the set of all $(X, Y) = (x_1, \ldots x_m; y_1, \ldots y_n)$ fulfilling [(7)** and] . . . W = the set of all $(X, Y) = (x_1, \ldots x_m; y_1, \ldots y_n)$ fulfilling [(8)**] . . ." then if V and W have a point in common, that point is the equilibrium (ibid., p. 6).

Von Neumann demonstrated this by establishing a much more general result. Notice first that homogeneity in X and Y enables one to use the fact that X is a simplex in R^m, and Y is a simplex in R^n. The problem is thus set up as one of establishing a fixed point of a mapping whose domain is a simplex in R^{m+n}. The Brouwer fixed point theorem deals with continuous mappings of simplexes to themselves. Von

Neumann established a more general result than Brouwer's about fixed points of continuous mappings. This enabled him to show that indeed *V* and *W* did have a point in common: a set of points which solved 7** could generate a set of points which solved 8** and there was at least one point which each such set had in common.

The narrow focus of our narrative, on equilibrium, rules out noting more than the fact that von Neumann had actually solved the problem, initially posed by Cassel and further defined by Wald, of establishing an equilibrium in a uniformly expanding economy. Indeed, von Neumann established that for an economy in such an equilibrium, the rate of interest equalled the rate of growth. Such a result, however, paid explicit attention to the price-quantity duality, the complementary slackness conditions induced by the non-negativity constraints, and the convexity of the production and price sets induced by returns to scale and homogeneity. (In some ways, one of the most interesting and curious ironies of this history is that the very general fixed point theorem developed in the von Neumann paper was not necessary to obtain his result on the existence of an equilibrium growth path. A decade later several authors, starting with Loomis in 1946, were able to demonstrate this fact.[17])

As a matter of history, it is unclear just how von Neumann's 1932 seminar paper at Princeton, certainly done independently of Wald and the Menger Colloquium, was refined by contact with Menger (and Wald?) into the version which was published in 1937. It is certainly true that in his 1936 survey Wald had indicated that *he* intended to examine equilibrium in a growing economy, which suggests that when that paper was written, probably in 1935, he was not aware of von Neumann's work. In any event Wald did *not* write such a paper; perhaps, helping edit the von Neumann paper for the final Volume 8 of the *Ergebnisse,* he saw results stronger than those of his own projected paper. It is also true, of course, that Wald's own models had a different "flavor" from von Neumann's. Wald had emphasized factor use and supply and the problem of allocating scarce resources. Von Neumann emphasized the choice of activities, and his complementary slackness relations involved factor prices and activity levels with profit inequalities.

It is worth noting another element of the story here. Morgenstern, well-trained as an economist, had had a continuing interest in the interaction of events and predictions, especially the interaction of agents when their predictions and foresight link their behaviors in the marketplace. In a 1935 *Zeitschrift für Nationalökonomie* paper entitled *"Vollkommene Voraussicht und Wirtschaftliches Gleichgewicht"* Morgenstern presented the now famous Sherlock Holmes-Moriarity "I think-he thinks . . ." problem with its "strategic" reasoning. Morgenstern was invited by Schlick to present these ideas to the Vienna Circle; he later recalled that "I repeated this talk, at Menger's request, in his Colloquium and after the meeting broke up, a mathematician named Edward Čech came up to me and said that the questions I had raised were identical with those dealt with by John von Neumann [in his 1928 paper] . . ." (Morgenstern, 1976, pp. 806–07).

V. *The Westward Movement*

Von Neumann obtained a permanent position in the U.S. in 1931. Other European scientists, no less conscious of what Churchill called "the gathering storm," had likewise begun to consider emigrating by the mid-1930s. There was a lengthy period in which "The political situation in Austria deteriorated from month to

[17] I am grateful to Gerard Debreu for alerting me to this point.

month. The *Ergebnisse* was criticized [in 1937] (with specific reference to Wald) for its large number of Jewish contributions . . ." (Menger, 1952, p. 19). Schlick had been assassinated.

The Nazis entered Vienna in March 1938; "Schlesinger, who occupied a rather prominent position, chose death that same day" (ibid.).[18] As Morgenstern recalled, "I was dismissed as 'politically unbearable' from the University as well as from my Institute, which I had left [while on a trip to the U.S.] in the hands of my deputy who emerged as a Nazi." (Morgenstern, 1976, p. 807). "Wald himself continued for a few weeks after Hitler's arrival in Vienna. He was dismissed by Morgenstern's successor but not otherwise molested. But I was greatly worried about his future as long as he remained in Austria, and with other friends, I tried to get him to the United States" (Menger, 1952, p. 19).

That effort had been proceeding even prior to March, 1938. On Morgenstern's initiative (and also that of Ragnar Frisch) Alfred Cowles had extended an invitation, in 1937, to Wald to become a staff member of the Cowles Commission in Colorado Springs. He had apparently decided to accept the offer even before the Nazi takeover, yet as a Rumanian citizen, Wald had to exit from that country, and "he had great difficulty getting back to Rumania, from where he went to the United States . . ." (Morgenstern, 1951, in 1976, p. 493) apparently by way of Cuba (Wallis, 1982, p. 1).

Menger had also gone to the U.S. in 1937, taking an appointment first at Notre Dame and eventually settling at the Illinois Institute of Technology in Chicago.

[18] There is a terrible irony in Schlesinger's suicide, since he was instrumental in arranging the mechanism by which many German academics were relocated in England. Leo Szilard's "Reminiscences," (copyrighted in 1968 by Gertrud Weiss Szilard) tells the following story. Szilard wrote that "while I was in Vienna [in April, 1933] the first people were dismissed from German universities, just two or three; it was however quite clear what would happen. I met, by pure chance, walking in the street a colleague of mine, Dr. Jacob Marschak, who was an economist at Heidelberg and who is now [1960] a professor at Yale. He also was rather sensitive; not being a German, but coming from Russia he had seen revolutions and upheavals, and he went to Vienna where he had relatives because he wanted to see what was going to happen in Germany. I told him that I thought since we were out here we may as well make up our minds what needed to be done and take up this lot of scholars and scientists who will have to leave Germany and the German universities. He said that he knew a rather wealthy economist in Vienna who might have some advice to give. His name was Schlesinger and he had a very beautiful apartment in the *Liechtensteinpalais*. We went to see him and he said, 'Yes, it is quite possible that there will be wholesale dismissals from German universities; why don't we go and discuss this with Professor Jastrow.' Professor Jastrow was an economist mainly interested in the history of prices, and we went to see him—the three of us now—and Jastrow said, 'Yes, yes, this is something one should seriously consider,' and then he said, 'You know, Sir William Beveridge is at present in Vienna. He came here to work with me on the history of prices, and perhaps we ought to talk to him.' So I said, 'Where is he staying'? and he said, 'He's staying at the Hotel Regina.' It so happened that I was staying at the Hotel Regina, so I volunteered to look up Sir William Beveridge and try to get him interested in this.

I saw Beveridge and he immediately said that at the London School of Economics he had already heard about dismissals, and he was already taking steps to take on one of those dismissed, and that he was all in favor of doing something in England to receive those who have to leave German universities. So I phoned Schlesinger and suggested that he invite Beveridge to dinner. Schlesinger said no, he wouldn't invite him to dinner because Englishmen, if you invite them to dinner, get very conceited. However, he would invite him to tea. So we had tea, and in this brief get-together, Schlesinger and Marschak and Beveridge, it was agreed that Beveridge, when he got back to England, and when he got the most important things he had on the docket out of the way, would try to form a committee which would set itself the task of finding places for those who have to leave German universities. He suggested that I come to London and that I occasionally prod him on this, and that if I were to prod him long enough and frequently enough, he thought he would do it. Soon thereafter he left, and soon after he left, I left and went to London.

When I came to London I phoned Beveridge. Beveridge said that his schedule had changed and that he found that he was free and that he could take up this job at once, and this is the history of the birth of the so-called Academic Assistance Council in England (Szilard in Fleming/Bailyn, 1969. pp. 97–98).

Morgenstern, half of his salary paid for three years by the Rockefeller Foundation (which had supported the Vienna Institute) secured a position at Princeton. Gerhard Tintner, also associated with the group of Vienna economists at Morgenstern's Institute, came to the Cowles Commission in 1936 and began teaching at Colorado College. In 1937 he left for a distinguished career at Iowa State University. It is reasonable to believe that Tintner, as well as Frisch, was the link between Cowles and Wald.

Wald's reputation had preceeded him. Volume 5 of *Econometrica,* for 1937, contains the first reference in English that I have been able to locate to Wald's work.[19] A report on the December 28–30, 1936 meeting of the Econometric Society in Chicago notes:

> He [Tintner] also referred to the remarkable work of a young Viennese mathematician, A. Wald, who has shown the conditions under which the Walrasian equations have one and only one solution. His proof (published in *Ergebnisse eines Mathematischen Kolloquiums,* edited by Karl Menger, 1935 and 1936) assumes, however, that the utilities derived from different commodities are independent. There is not yet a solution of the general case [p. 188].

(The same *Econometrica* volume (p. 91) also notes that both Schlesinger and Wald attended the Econometric Society's meetings in France on September 11–15, 1937).

Wald had arrived in the U.S. primarily as a mathematician familiar with the problems associated with statistical time series of interest to economists, work that was close to the central focus of the Cowles Commission at that time. He was known, and his work on equilibrium was known, at least to Morgenstern at Princeton and Tintner at Cowles and then Iowa State. The work had been noted at the Econometric Society meetings in 1936, and referred to in *Econometrica* in 1937. His paper in the *Zeitschrift für Nationalökonomie,* a major economics journal, placed the analysis of equilibrium in general equilibrium systems in the "public domain"; although written in German, it was available to those interested.

Wald's work had crossed the Atlantic before its author. And he himself probably owed his life to that work, which had gained him a U.S. visa. "His parents and his sisters were murdered in the gas chambers of *Ossoviec* (Auschwitz); his brother Martin, the engineer, perished as a slave laborer in Western Germany" (Menger, 1952, p. 19). Eight members of his immediate family had been murdered. After the war, "he succeeded in bringing the sole survivor, his brother Hermann, to this country, and he took great comfort in his company" (Morgenstern, 1951, in 1976, p. 497).

VI. *In the United States: The 1930s*[20]

> As early as 1912, while [Irving] Fisher was vice-president of the American Association for the Advancement of Science, he had attempted to organize a society to promote research in quantitative and mathematical economics. Wesley C. Mitchell, Henry L. Moore, and a few others had been interested but they were too few, and for the time being nothing came of their vision [Christ, 1952, p. 5].

It was not until 1928, when Frisch enlisted the support of Charles F. Roos and Irving Fisher, that this threesome took the steps to organize the Econometric Society. The group formally constituted itself as an international society in December

[19] There was an additional reference to Wald's survey paper in Vol. IX, No. 2 (July, 1938) of the *Zeitschrift für Nationalökonomie.* A paper by Alexander Bilimovic, *"Einige Bermerkungen zur Theorie der Planwirtschaft"* (pp. 147–66) cites Wald's allocation equilibrium results on p. 151 with reference to planning.

[20] Much of the material for this section is drawn from the extremely useful *Economic Theory and Measurement: A Twenty Year Research Report, 1932–1952,* written by Carl Christ in 1952 for the Cowles Commission. This report is the most useful source on the topic of this section.

1930 in Cleveland, and in September 1931 in Lausanne, Switzerland. Its activities at first were limited to meetings, usually held in conjunction with the AEA and the AAAS.

At about this time Alfred Cowles, discouraged by the poor performance of stock market and business forecasters, was persuaded to foster his own interests by subsidizing the fledgling Econometric Society and a journal, to be called *Econometrica*. To this end the state of Colorado, Cowles's home base, chartered the Cowles Commission for Research in Economics in September 1932, a group whose purpose was ". . . to educate and benefit its members and mankind, and to advance the scientific study and development . . . of economic theory in its relation to mathematics and statistics" (ibid., p. 11).

In organization, the Econometric Society sponsored the Cowles Commission and guided it through an Advisory Committee whose initial members included Fisher, Frisch, A. L. Bowley of LSE, Mitchell of the NBER, and Carl Snyder of the New York Federal Reserve Bank. The first issue of *Econometrica* appeared in January, 1933.

It would, I believe, be fair to characterize the American members of the Econometric Society as more guided by statistical interests than theoretical ones. Despite the reknown of Fisher, much of the economics profession had a bias of sorts against mathematical theory which did not extend so much to statistical work. Although there were individual exceptions, it was not uncommon for an economist to eschew, completely, any training in mathematics. (My colleague, Martin Bronfenbrenner, a graduate student at Chicago beginning in the mid-1930s, took some calculus courses while a graduate student and recalls that such training was not required.)

It was not as though formal theory, or even general equilibrium economics, was unknown in the U.S. Cassel's *The Theory of Social Economy* was used as a textbook for an economic theory course in the University of Chicago Business School in the mid-1930s. Henry Schultz was developing a research tradition in quantitative economics at Chicago. Harold Hotelling, at Columbia, was cited and well-regarded. The NBER was fully involved in careful measurement and analysis of economic time series.

Yet the times were hostile to mathematical economics. The central problems of economic science were focused by the depression and mass unemployment. Many young economists were involved in the policy experiments of the New Deal. Theoretical work was partially shaped by such events. And the theoretical explosion associated with Keynes' *General Theory of Employment, Interest, and Money* in 1936 consumed the passion and interest of economists with a taste for theory. There were not that many professional economists, many fewer still with interests in theory, and for these the intellectual action was in the emerging Keynesian program.

It is thus not too far off the mark to identify the Cowles Commission with mathematical economic theory in the U.S.[21] The summer conferences in Colorado, in 1935 and 1936, included Harold T. Davis, Hotelling, August Loesch, Isadore Lubin, Snyder, Fisher, R. A. Fisher, Corrado Gini, E. J. Working and others with interests in the relationship between economics, mathematics, and statistics.

The Cowles staff was forming too; Tintner joined in 1936. The position of director was hard to fill on Roos' departure in 1937. The distinguished director sought could not be induced to move to Colorado Springs, so offers to Frisch, Jacob Marschak, and Theodore Yntema were de-

[21] The major exception was to be Paul Samuelson, who was later joined by Robert Solow.

clined. The summer conferences in 1937–1939 saw such visitors as R. G. D. Allen, Mordecai Ezekiel, Trygve Haavelmo, Abba Lerner, Horst Mendershausen, Rene Roy, Schultz, and, in 1938, Wald, who was appointed a fellow of the Commission for 1938–1939.

In 1939 the Cowles Commission left Colorado for Chicago, and an affiliation with the University of Chicago. Schultz' tragic death in November, 1938 had led that University to consider "The possibility of adopting a group such as the Cowles Commission" (ibid., p. 20) to replace Schultz. Yntema became Director of Research for the Commission, the Chicago staff members Oscar Lange, Jacob Mosak, and H. Gregg Lewis became part-time staff of the Commission. Wald did not move from Colorado to Chicago; instead he accepted a position proferred by Hotelling (on a grant from a Carnegie Foundation) as Hotelling's assistant at Columbia, and a temporary faculty position in mathematics at Queens College.

Lange left Chicago to visit Columbia in 1942, and eventually resigned in 1945. During this time, Yntema resigned as Research Director for Cowles, and Marschak accepted a professorship on the Chicago Faculty and the directorship of Cowles. He brought Haavelmo to Cowles in 1943, Koopmans and Lawrence R. Klein in 1944, and Kenneth Arrow, Herman Chernoff, and Herbert Simon in 1947.

> The reorientation which Marschak and his new staff wrought in the Cowles Commission's research program is sketched in the following passage from the Annual Report for 1943 . . .:
>
> The method of the studies . . . is conditioned by the following four characteristics of economic data and economic theory: a) the theory is a system of simultaneous equations, not a single equation; b) some or all of these equations include 'random' terms . . .; c) many data are given in the form of time series . . .; d) many published data refer to aggregates . . . To develop and improve suitable methods seems, at the present state of our knowledge, at least as important as to obtain immediate results. Accordingly, the Commission has planned the publication of studies on the general theory of economic measurements . . . It is planned to continue these methodological studies systematically . . . [Christ, op. cit., pp. 30–31].

With such a program adumbrated by 1943, there was clear recognition of the centrality of general equilibrium analysis in the development of economic theories sufficiently rich to provide a basis for empirical work. As an exemplar, consider the book, published in 1943 by the University of Chicago Press, *Studies in Mathematical Economics and Econometrics in Memory of Henry Schultz*. This volume, edited by Lange, Francis McIntyre, and Yntema, drew its contributions primarily from those associated with the Cowles Commission, Schultz' "successor" at Chicago.

For our purposes, it suffices to note William Jaffe's article on Walras, Lange's on Say's Law in a general equilibrium system, and Mosak's on the Slutsky equation (an article which thanks Wald for a crucial proof in an optimization argument). Tintner had a piece on "nonstatic" production theory which cites the Wald and von Neumann *Ergebnisse* papers in an existence of equilibrium argument. The paper by Allen Wallis and Milton Friedman on indifference functions cited an immensely important 1940 paper by Wald on Indifference Surfaces and Engel Curves. General equilibrium theory was in the air, but it was not the Menger Colloquium which had cast it aloft. The primary propellant was John R. Hicks' *Value and Capital.*

In an earlier book (Weintraub, 1979) I attempted to indicate how Hicks' 1939 volume effected one of its primary objectives, stated in his book's Introduction, of being "able to see just why it is that Mr. Keynes reaches different results from earlier economists on crucial matters of social policy . . ." (Hicks, 1939, p. 4). Nonetheless, Hicks' work had a more concrete genesis: "Our present task [in this book] may therefore be expressed in historical terms as follows. We have to reconsider the value theory of Pareto [and Walras] and then to apply this improved value theory to those dynamic problems of capital which Wicksell could not reach with the tools at his command" (ibid., p. 3).

Hicks' book was not unique in its concern to revive Walras and Pareto, or at least to bring the Lausanne tradition to bear on the concerns of English speaking economists. General equilibrium theory in its broad outline was "in the air." Students at LSE in 1939–1940 did not find *Value and Capital* surprising—they had been encouraged to think along those lines by various teachers. (Sidney Weintraub, at LSE at that time, recalls having *gone* there with a portion of Pareto's *Manuel* translated.) Nevertheless, Hicks' book developed the classical general equilibrium theory from the theory of the household and the theory of the firm in modern "neo-classical" language. He then provided an "equilibrium" and "stability" analysis, the former by equation counting, the latter by static characteristics of the equilibrium relationships. The properties of equilibrium were well treated: the possibility of equilibrium was not recognized as a technically serious issue. Nonetheless Hicks' *Value and Capital* was a signal event for economists. The general equilibrium approach was developed in English; the most modern results in Value Theory—indifference curves and production functions—were integrated in the analysis, and the properties of the enriched system were explored in the clear prose of an Oxford-trained economist. The formal mathematical underpinnings were confined, as in Marshall, to mathematical appendices. It was there that the more mathematically sophisticated readers could appreciate the design. The macroeconomic orientation of much of Hicks' argument linked the concerns of the Keynesian literature, a macroeconomic literature, to an intellectual framework, general equilibrium theory, in which the microeconomics was fully articulated. The result was that microtheorists and Keynesians, mathematical sophisticates and innumerates, could study *Value and Capital* together and see in its analysis a unity where before there appeared chaos.

The book had an immediate impact. It suffices to note, in this vein, the April 1941 *Econometrica* article by Paul A. Samuelson, "The Stability of Equilibrium: Comparative Statics and Dynamics," in which the mathematical definitions of stability and equilibrium were set out (but recall Wald's 1936 *Zeitschrift* paper) and explored. Comparative static analysis was formally defined; both stability analysis and comparative statics were applied to the problem of multiple markets. Samuelson contrasted his approach with that of Hicks, and further explored the problem by analyzing the IS-LM "Keynesian" model as a system of simultaneous equations which deserve an explicit analysis of equilibrium. However, Samuelson assumed the existence of equilibrium. He simply noted that behavior of variables x_i in a set of n functional equations of the general form

$$F^i[x_{1\,-\infty}^{\;t}(\tau), x_{2\,-\infty}^{\;t}(\tau), \ldots x_{n\,-\infty}^{\;t}(\tau)] = 0,$$

$$i = 1, 2, \ldots n$$

is "determined once certain initial conditions are specified" (Samuelson, 1941; in Arrow 1971, p. 137).

Further he stated that, following Frisch:

> . . . stationary or equilibrium values of the variables are given by the set of constants ($x_1^0, \ldots x_n^0$) which satisfy these equations identically, or
>
> $$F^i[x_{1\,-\infty}^{\;t_0}, x_{2\,-\infty}^{\;t_0}, \ldots x_{n\,-\infty}^{\;t_0}] = 0, \quad i = 1, 2, \ldots n$$
>
> [ibid., p. 137–38].

He had, however, a footnote to this sentence which read:

Of course, such a set need not exist. Thus, the simple system

$$\frac{dx}{dt} = e^x - x$$

has no stationary equilibrium values since $e^x - x$ has no real roots. Similarly,

$$\frac{dx}{dt} = 1$$

defines no stationary equilibrium position [ibid., p. 138].

Samuelson's techniques, including those present in his later *Foundations of Economic Analysis,* avoided existence problems for general equilibrium systems. Samuelson generally linearized the functions $f_i(x_1, \ldots x_n)$ which appear in a system like $\dot{x}_i = f_i(x_1, \ldots x_n)$, where the Taylor series is taken at an "equilibrium" x^*. Then non-vanishing of a Jacobian suffices for independence of the equations, and thus the "assumed" equilibrium is not inconsistent with the equations. This procedure, however, begs all existence of equilibrium questions for non-trivial equation systems.[22]

But lest it be said that all economists had followed Hicks in his approach to the analysis of general equilibrium systems, consider the June 1941 appearance, in the *Journal of Political Economy,* of the article "Professor Hicks on Value and Capital" by Oskar Morgenstern. This 29 page review article was an attack on Hicks' book, written as a magnificent polemic. The centerpiece of Morgenstern's assault was Hicks' method of counting equations to establish existence of an equilibrium for a general equilibrium system. Morgenstern wrote:

> Hicks' assertion is incorrect even from the point of view of history of doctrine. Moreover it is systematically incorrect because the determinateness of a system of equations does not necessarily depend only upon the equality of the number of unknowns with the number of equations . . . We have as yet such [existence] proofs, only for two systems of equations, those of von Neumann and of Wald" [reprinted in Morgenstern, 1976, pp. 370–71].

Morgenstern's basic point was that difficulties, truly serious difficulties in the mathematical-logical analysis of economic systems, cannot simply be assumed away by cheerful prose and appeals to common sense. Morgenstern later recalled von Neumann's comment on Hicks, and mathematical economics in the Hicksian tradition as:

> You know, Oskar, if those books are unearthed sometime a few hundred years hence, people will not believe they were written in our time. Rather they will think that they are about contemporary with Newton, so primitive is their mathematics. Economics is simply still a million miles away from the state in which an advanced science is, such as physics [Morgenstern, 1976, p. 810].

Morgenstern wrote his blast at Hicks, and recalled von Neumann's comments on such writings, while he and von Neumann were fully engaging in the collaboration that resulted in the *Theory of Games and Economic Behavior.* It was this work, and the development of activity analysis and programming, that led to the articulation of the general equilibrium model, and analysis of equilibrium, in the early 1950s by Arrow, Debreu, and McKenzie. The next sections will thus present the history and developments associated with games and activity analysis.

VII. *The Theory of Games*

Čech's comment to Morgenstern, that von Neumann had a similar interest in

[22] Put another way, the Taylor series-Jacobian technique defined the conditions under which "equation-counting" provided necessary and sufficient conditions for the existence of a *local* equilibrium. But since the Taylor series technique usually involved an expansion *around* the equilibrium point, the entire procedure was more directed to exploring the properties of a putative equilibrium than demonstrating the possibility that such an equilibrium existed.

strategic behavior, led to Morgenstern's first conversation with von Neumann in February, 1939 in Princeton while at an afternoon tea with Niels Bohr. "Von Neumann told me that he had done no work on game theory since 1928 or on the expanding economy model. He may have thought one way or the other about it, but never in any systematic way, nor had he put down anything on paper" (Morgenstern, 1976, p. 808).

Morgenstern's conversations with von Neumann led to the economist's decision to "write a paper showing economists the essence and significance of game theory as it then existed" (ibid.). While in an early draft, the paper was read by von Neumann who suggested a collaboration. This delighted Morgenstern, who later referred to this event of the Fall of 1940 by saying "*'Er war mir ein Geschenk des Himmels!'* [He] was my gift from Heaven" (ibid.).

The collaboration was intense and the paper expanded first to two papers, then to a small pamphlet, and ultimately to a very large book. The substantive Chapter II, defining and axiomatizing the mathematical concept of a game, was entirely new. (The 1928 paper had rather taken "game" to be understood in ordinary language, and proceeded to analyze the choice (strategy) structure for two and three person zero-sum games, while extending the zero-sum idea to games with more than three players.) The fundamental existence theorem for two person zero-sum games appeared in Section 17 of the book, with references to von Neumann's 1928 paper.[23] A footnote (p. 154, 2nd edition) states that the min-max problem which is related to the game appears, in a more general setting, in certain economic models like those presented in von Neumann's *Ergebnisse* paper.[24] The authors there note "it seems worth remarking that two widely different problems related to mathematical economics—although discussed by entirely different methods—lead to the same mathematical problem—and at that to one of a rather uncommon type: The 'Min-Max type.' There may be some deeper formal connections here [with the *Ergebnisse* paper] . . . The subject should be clarified further" (von Neumann-Morgenstern, 1947, p. 154, n. 1).

The footnote continues, pointing out that the original proof used Brouwer's fixed-point theorem, and the *Ergebnisse* paper used a generalization of that result, which itself had been simplified by Kakutani in a 1941 paper. That paper, "A Generalization of Brouwer's Fixed Point Theorem," appeared in the *Duke Mathematical Journal,* which received it in January 1941 while Kakutani was at the Institute for Advanced Study, von Neumann's base. Kakutani had, however, begun corresponding with the editor of that journal in 1939, from Japan, so it would appear that the article, while "polished" through consultation with von Neumann, was conceived independently. The anonymous referee of the paper was willing to accept it for printing primarily because it was short and, unlike other fixed-point theorems, was written in English, not German: [Anonymous, 1941, p. 1.] (Kakutani thanked A. D. Wallace, the topologist, for help in discussing the problem.) The fixed point theorem itself stated that "If $x \rightarrow \phi(x)$ is an upper semi-continuous point-to-set-mapping of an r-dimensional closed simplex S into $A(S)$ [the set of closed convex subsets of S] then there exists an $x_0 \epsilon S$ such that $\phi(x_0) \epsilon A(S)$." A corollary showed that S could be any compact convex sub-

[23] The theorem was that any two-person zero-sum game had an equilibrium solution in mixed strategies.

[24] Recall that the function ϕ, introduced in von Neumann's *Ergebnisse* paper, had a min-max solution at the "equilibrium" of the activity levels and prices.

set of a Euclidean space.[25] Using the theorem Kakutani proved the von Neumann min-max theorem in seven lines.

There was, however, a simpler way to prove the min-max theorem. As von Neumann and Morgenstern state, in the same footnote (p. 154) Jean Ville had provided a proof of the min-max theorem using elementary (non-topological) techniques in 1938 in a paper *"Sur la Théorie Genérale des Jeux où Intervient l'Habileté des Joueurs."* This proof is based on convexity arguments, and the supporting hyperplane theorem.[26] (Morgenstern's "discovery" of the Ville paper is discussed in his 1976 *JEL* article.)

The importance of this proof, originally due to Ville, but much improved by von Neumann and Morgenstern, should not be understated. Chapter 16 of the *Theory of Games and Economic Behavior* contains the "tools" used in the proof of the fundamental min-max theorem of Section 17. Thus Section 16 contains a clear and comprehensive discussion of the geometry of R^n, vector operations, hyperplanes and half-spaces, convex sets in R^n, and the supporting hyperplane theorem. This discussion leads to the Theorem of the Alternative for matrices which generates the fundamental duality results for the dual systems of linear inequalities crucial to min-max, and the later programming, arguments. This chapter presaged an entirely new approach to the structure of economic optimization theory, an approach which led to a global characterization of objective functions and constraint sets through convexity arguments. (Local characterization, using partial derivatives or marginal conditions, had been the standard approach since the "marginal revolution" of the latter part of the 19th century.)

The manuscript went to the printer in 1943, and appeared in September, 1944. Von Neumann indicated to Morgenstern that "he did not expect a rapid acceptance [of the book's ideas], rather we would have to wait for another generation. This view was shared by some of our friends, especially by Wolfgang Pauli and Hermann Weyl" (Morgenstern, 1976, p. 813).

In a magnificent understatement, Morgenstern reflected "matters turned out in some ways quite differently" (ibid.). Leonid Hurwicz and Marschak wrote lengthy notices of the theory in 1945 and 1946, as did Wald in 1947, while Wald "in 1945 had already laid a new theory of the foundations of statistical estimation based on the theory of the zero-sum two-person game" (ibid.).[27]

The basic payoffs in the games von Neumann and Morgenstern considered were not money sums, or arbitrary "stuff." Early in the collaboration Morgenstern had ar-

[25] Intuitively, the Brouwer theorem says that if a continuous function f maps a compact-convex set $S \subset R_n$ to itself as $f: S \rightarrow S$, then there is at least one point $\hat{x} \epsilon S$ such that $f(\hat{x}) = \hat{x}$. The Kakutani theorem generalizes this result (or simplifies van Neumann's generalization of Brouwer) in two ways. First, f is a correspondence mapping points $x \epsilon S$ to *subsets* $S_x \subset S$. Continuity of the function is then weakened to upper semi-continuity of the correspondence. If S is compact and convex, and S_x is convex, then the conclusion of the Kakutani theorem states that there is some $\hat{x}$ such that $\hat{x} \epsilon f(\hat{x}) = S_{\hat{x}}$.

[26] This theorem is now recognized as a corollary to the Hahn-Banach theorem of functional analysis. One version used by economists, the separating hyperplane theorem, states that given a convex set and a point outside that set, there is a plane, through the point, which does not intersect the interior of the convex set.

[27] Lest this connection go unremarked, we refer to Wallis' excellent history "The Statistical Research Group, 1942–1945":

> Wald's work on decision theory [based on the min-max idea] had begun before his association with SRG [The Statistical Research Group, in 1943]. When Savage first joined SRG, I introduced him to Wald at lunch one day. Wald discussed some of his ideas on decision theory and Savage, who was a former research assistant of von Neumann's, remarked that he knew a rather obscure paper that would interest Wald, namely, von Neumann's 1928 paper on games. Wald laughed, and said that some of his ideas were based on that paper. It is a highly technical and academic paper [Wallis, 1980, p. 334].

gued against von Neumann's money payoff idea. "I was not very happy about this, knowing the importance of the utility concept, and I insisted we do more" (Morgenstern, 1976, p. 809). Their thinking about the manner in which the analyst could obtain a numerical representation of utility led to an axiomatization of choices in risky situations which allowed the inference of the existence of a real continuous function as an order-preserving representation of utility. This is now called the von Neumann-Morgenstern utility indicator, which is unique up to linear transformations, and interpretable as "expected utility." The first edition of 1944 contained the axiomatization, and the indicator, but no proof. The book's success, and the need for a second edition in 1947, allowed drafting "a substantial appendix, giving the proof that our system of axioms for a numerical utility, set forth in the first edition, indeed gave the desired result" (ibid., p. 814).

This line of analysis may be carried ahead to the early 1950s, where it had an indirect relationship to the existence-of-equilibrium papers to be examined later. In an April, 1950 paper in *Econometrica*, "Rational Behavior, Uncertain Prospects, and Measurable Utility," Marschak, formerly of Cowles, provided a set of simpler axioms for a finite set of sure prospects. Herman Rubin, a former Cowles research associate and research consultant to Cowles from 1949, generalized the Marschak paper to an infinite number of sure prospects. Then I. N. Herstein, and John Milnor, the former a Chicago mathematician and research associate at Cowles in 1951, the latter similarly a distinguished mathematician then at Princeton and associated with the RAND Corporation, did separate papers "cleaning up" the axiomatization. The Herstein-Milnor collaboration then yielded a Cowles paper (number 65) late in 1952, which appeared in *Econometrica* in 1953 as "An Axiomatic Approach to Measurable Utility."

Such work, on the von Neumann-Morgenstern axiom system, was very "hot" at Cowles in that period. The French mathematical economist Gerard Debreu, recently arrived in the U.S. via a Rockefeller Fellowship, and a Cowles research associate from June 1950, proved that numerical representation (by utility) of preferences was a quite general proposition, not restricted to the case of "risky" prospects. His results, developed in an April 1952 Cowles discussion paper (number 2040) appeared with the title "Representation of a Preference Ordering By a Numerical Function" as Chapter XI in the 1954 book *Decision Processes,* edited by Thrall, Coombs, and Davis.

There is one further development worth identifying at this point in the narrative, for it provides a direct bridge between the theory of games and the existence-of-equilibrium papers. In what is certainly the shortest (less than one page of type) article of major importance to economists, John Nash, a Princeton mathematician, generalized the von Neumann-Morgenstern equilibrium for two-person zero-sum games to n-person games. His paper, which appeared in *The Proceedings of the National Academy of Sciences,* Volume 36, 1950 (submitted in late 1949), was called "Equilibrium Points in N-Person Games." Nash defined an "equilibrium" of an n-person game to be an n-tuple of strategies (one for each player) such that for any player, his *own* strategy is optimal (yields highest payoff) against the equilibrium (n-1)-tuple defined by the remaining players (their own equilibrium strategies being determined analogously). The proof used the Kakutani Fixed Point Theorem to show that all n-person games possess such an equilibrium (now called a "Nash Equilibrium"); Nash noted that "In the two-person zero-sum case the 'main theorem' [of von Neumann and Morgenstern] and the existence of an equilibrium point are equivalent" (Nash, 1950, p. 49). As von Neumann's *Ergebnisse* paper on economics generalized the basic equilibrium of two-person game theory presented in 1928, so the later Debreu, and Arrow and Debreu, papers on economic systems would generalize the n-person equilibrium idea of Nash. The pace of new results had quickened; the list of contributors to this literature was growing rapidly.

There remains, however, one other distinct line of work to examine before we can reconcentrate our attention on the existence-of-equilibrium literature. For simultaneously with, and related to, the burgeoning work on the theory of games, there was a literature growing on "activity analysis," or "linear models," or "programming." This work was to have a direct bearing on the existence-of-equilibrium literature.

VIII. *Activity Analysis*

In late June 1949, the Cowles Commission hosted a Chicago conference on "Linear Programming." The participants formed a virtual "Who's Who" of mathematical economics, from Arrow, George Dantzig, and David Gale, to Koopmans, Samuelson, and Marshall Wood, from Arman Alchian to Tibor Scitovsky. In addition to economists, there were mathematicians, statisticians, administrators and military planners. The subject matter, representing a confluence of distinct lines of inquiry, was focused and developed by the papers of that conference. Those proceedings formed the 1951 book *Activity Analysis of Production and Allocation;* its editor was Koopmans.

Koopmans had received his doctorate from Leiden, The Netherlands, in 1936. From 1938 to 1940 he did business cycle research at the League of Nations in Geneva. In 1940 he came to the U.S., and from 1942 to 1944 was a statistician at the Combined Shipping Adjustment Board in Washington. He joined the Cowles staff in 1944, and in 1948 was appointed Research Director at Cowles, following Marschak.

In his "Introduction" to the 1951 volume, Koopmans clearly identified four lines of research which had jointly created the subject matter of linear programming, or activity analysis. "A specific historical origin of the work in this volume is found in discussions among Austrian and German economists in the thirties on generalizations of the Walrasian equation systems of mathematical economics" (Koopmans, 1951, p. 1). The ensuing paragraphs provide an explicit recognition of the work of Wald and von Neumann, and they conclude: "We have dwelt on these discussions in some detail because even among mathematical economists their value seems to have been insufficiently realized" (ibid., p. 2).

The second line of influence was the "new" welfare economics, which from Barone through Lange and Abba Lerner had "The underlying idea . . . that the comparison of the benefits from the alternative uses of each good, where not secured by competitive market situations, can be built into the administrative processes that decide the allocation of that good" (ibid., p. 3).

The third stream was "the work on interindustry relationships, initiated, developed, and stimulated largely by Leontief . . . and given statistical expression by measurements and tabulations provided by the Bureau of Labor Statistics" (ibid.). Thus the welfare economics ideas of how a planner or decision maker could develop efficient allocation schemes was linked to Leontief's Input-Output tableaux, with their explicit linear structure.

The final stream of ideas was based on the work by Dantzig and Wood for the U.S. Department of the Air Force, and related work at the RAND Corporation (in conjunction with the Air Force), on the organization of defense, the conduct of the war, and other specifically war-related allocation problems. Koopmans' own work at the Shipping Board, for instance, had dealt with the efficient routing of cargo ships. As Koopmans noted, "It does seem that governmental agencies, for whatever reason, have so far provided a better environment and more sympathetic support for the systematic study, abstract and applied, of principles and methods of allocation of resources than private industry" (Koopmans, 1951, p. 4). This, of course, was also the view of physicists, chemists and engineers in that Manhattan Project period.

The papers themselves, presented or abstracted in June 1949, provide a useful overview of the concerns and research directions of mathematical economists in the later 1940s. They are thus worth examining for the beam of light they cast into the early 1950s. The general methodology was developed in the papers by Wood and Dantzig, and Koopmans. The former pair provided a revision of their 1948 paper, called in the 1951 book "The Programming of Interdependent Activities: General Discussion." The authors defined "programming, or program planning, . . . as the construction of a schedule of actions by means of which an economy, organization, or other complex of activities may move from one defined state to another . . ." (Wood and Dantzig, in Koopmans 1951, p. 15). They cited von Neumann's 1937 paper as a progenitor of such an approach, which can take two forms. "In the first formulation, the quantities of each of several activities contributing directly to objectives (or 'final demand') are specified for each time period . . . In the second . . . we seek to determine that program which will, in some sense, most nearly accomplish objectives without exceeding stated resource limitations" (ibid., p. 16–17).

The Koopmans paper, "Analysis of Production As An Efficient Combination of Activities" was the central paper of the collection. Koopmans himself has noted "that when I came to read [the *Ergebnisse* papers] I was more interested in von Neumann's . . . work. I brought [that] paper by von Neumann to Dantzig's attention, who then went to explain his work to von Neumann, who in turn introduced George [Dantzig] to duality ideas" (Koopmans, Jan. 13, 1982, p. 2).

The 1951 version of the Koopmans paper had been read, in an earlier version, at the Madison meeting of the Econometric Society in September, 1948. Koopmans credited Dantzig with interesting him in a more general context than his own initial analysis of transportation problems. The idea of the paper is to go "behind" the given "technique" that economists used in their production function analysis. "The 'technique' employed in production is itself the result of managerial choice (going beyond the discarding of unwanted factor quantities). Managers choose between, or employ efficient combinations of, several processes to obtain in some sense best results" (Koopmans, 1951, p. 34). The paper "axiomatized" production through activity analysis in a fashion analogous to the contemporaneous axiomatization of utility and consumer choice.

The model began by defining the scalar y_n, $n = 1, 2,. . .N$ as the total *net* output of the n^{th} commodity. The k^{th} activity is a set of coefficients a_{nk} $(n = 1, 2,. . .N)$ "indicating the rate of flow per unit of time of each of the N commodities involved in the unit amount of that activity" (ibid., p. 36). Nonnegative scalars x_k indicate the amount or level of the k^{th} activity, whose corresponding flow is $x_k a_{nk}$ $(n = 1, 2,. . .N)$.

This notation leads to the definition:

> A point y in the commodity space $[R^n]$ is called possible in a technology A $[(a_{nk})]$ if there exists a point x [in R^K] in the activity space satisfying $y = Ax$, $x \geqq 0$ [ibid., p. 47].[28]

The technology A is structured by several postulates. The first states that there is no x satisfying $y = Ax = 0$, $x \geq 0$. This rules out reversible modes of production, or the production of inputs from outputs. Second, there is no vector x satisfying $y = Ax \geq 0$, $x \geqq 0$; one can't produce something from nothing.

It should be clear that the activity levels defined by x are in the form of restrictions defined by the intersection of half-planes. The analysis thus can be formally developed as properties of cones in R^n, or alternatively as special kinds of convex sets. Hence by defining "cones" to satisfy certain properties, one has a formal model for the production analysis, and the production results can be interpreted in the algebraic structure of the model conclusions. Specifically the role of prices, shadow prices actually, can be developed from certain orthogonality arguments related to supporting hyperplanes.

Other papers in the conference volume attest to the worth of this approach to choice under constraint. The Leontief model was explored by Harlan Smith, Samuelson, Koopmans, Arrow, and Nicholas Georgescu-Roegen. There were applications of linear programming to crop rotation by Clifford Hildreth and Stanley Reiter, to program planning (for the military) by Wood and Murray Geisler, to the aircraft industry by Wood, to transportation by Koopmans, and to technical change by Simon. The mathematics of convex cones was separately presented by Gale and Murray Gerstenhaber. The relationship between linear programming and game theory, following von Neumann and Morgenstern's conjecture (noted in the previous section), was delineated by Dantzig, and by Gale, Harold Kuhn, and Albert Tucker. And finally the computation of efficient solutions, using Dantzig's simplex method, was defined and examined by Dantzig, Robert Dorfman, and Koopmans.

The conference had been important. Many of the mathematical economists in the United States had participated and shared their understandings. They developed a coherent framework for the programming approach which emphasized convexity and the allied properties of general topology and algebra:

> The belief may here be expressed that the theory of point sets in general, and of convex sets in particular, will be an increasingly important tool in economics. In many economic problems a preference ranking of alternatives representable by points in a space is confronted with an opportunity set. Often both the opportunity set and the set of points preferred-or-indifferent to any given point can be assumed convex. In such cases the use of convexity properties readily permits the study of optimizing choice from all available alternatives. On the other hand, the methods of calculus, more familiar to economists, permit at best a comparison of the chosen alternative with alternatives in its neighborhood, and that only if the required number of derivatives exist [Koopmans, 1951, p. 10].

Thus by mid-1949, and certainly by 1950, mathematical economists had (1) some knowledge of attempts, and successes, in establishing the existence of equilibrium in sensibly specified economic models; (2) a basic understanding of useful ways to model interrelated constrained choice systems; and (3) fixed-point theorem techniques for demonstrating compatibility of strategies or independent choices. The problem of showing the existence of a competitive equilibrium was accessible. The work remained to be done.

[28] If x is an n-vector, $x > 0$ means $x_i > 0$ for all i. $x \geq 0$ means $x_i \geqq 0$ for all i and for some j, $x_j > 0$. $x \geqq 0$ means $x_i \geqq 0$ for all i.

IX. *Properties of the Competitive Equilibrium*

Koopmans' forecast that convexity and point set topology ideas would characterize new approaches to modeling in economic theory was prescient. Simultaneously in August 1950, on the East and West Coasts Gerard Debreu and Kenneth J. Arrow presented models of a competitive economy and proved, for them, that competitive equilibria are Pareto-efficient, and that Pareto-efficient allocations can be realized by a price system such that the allocation is also a competitive equilibrium.

Gerard Debreu had studied at the *Ecole Normale Supérieure* from 1941–1944 prior to serving in the French Army from 1944–1945. From 1946–1948 he was a research associate at the *Centre National de la Recherche Scientifique* (CNRS) and taught a course on business cycles at the *Ecole d'Application de l'Institut National de la Statistique et des Etudes Economiques*. He gained a Rockefeller Fellowship to study in the U.S. from December 1948 to December 1949, spending the first six months at Harvard, the summer at Berkeley, and part of the fall at the Cowles Commission. He was appointed to the Cowles staff in June 1950 and held that appointment for more than ten years.

His first Cowles paper (New Series No. 45) of June 1950 formed the basis of the paper he presented at the Harvard meeting of the Econometric Society, and it was published in the July 1951 issue of *Econometrica* as "The Coefficient of Resource Utilization." Debreu provided, in that paper, "a non-calculus proof of the intrinsic existence of price systems associated with the optimal complexes of physical resources–the basic theorem of the new welfare economics . . . This proof is based on convexity properties . . ." (p. 274).

The paper defines individual consumption vectors x_i and the now usual ordering (which Debreu notes need not entail the existence of a utility function) on consumption vectors. Production is treated by total input vectors y whose components are negative for outputs, positive for inputs. (Debreu cites the Koopmans "Activity Analysis" paper although Debreu was not present at that Cowles conference.) With set summation, $x = \Sigma x_i$ where x is the total consumption vector. Thus $z = x + y$ is *total* net consumption, and if z^0 is "the utilizable physical resources vector . . . [the] constraints imposed by the economic system are $y \epsilon Y$ [technologically possible productions and] $z \leqq z^0$" (p. 278). The problem is to characterize an optimal list of production vectors y_j^0 and consumption vectors x_i^0 by means of a price vector $p > 0$ such that $p \cdot (x_i - x_i^0) \geqq 0$ for all i and every x_i preferred or indifferent to x_i^0, and $p(y_j - y_j^0) \geqq 0$ for every j and every feasible y_j. Debreu thus exploited the set theoretic structure of both the consumption and production spaces to obtain the *definition* of a competitive equilibrium and a characterization of that equilibrium as Pareto-efficient.

Debreu noted (p. 282) that "K. J. Arrow's paper . . . contains [also] a non-calculus proof of the basic theorem. Unfortunately, I had his manuscript in my hands for too short a time to appraise it fully here." Recently Debreu has expanded on that note, recalling that:

> In 1950–1951, The Cowles Commission had an internal refereeing process and it is in this connection that I was shown the manuscript of K. J. Arrow's paper by William B. Simpson, then Assistant Director of Research of The Cowles Commission. As I recall, W. B. Simpson asked me whether Arrow's contribution should be included in The Cowles Commission Reprint Series, and also to comment on the substance of the paper. Little time was available, presumably because of a deadline imposed by the editor of the [volume] in which Arrow's paper was to appear. Since this was before the age of the Xerox machines, I had a copy of this paper in my hands only for a brief period. My comments must have been superficial and

> only Arrow's kindness can have led him to describe them [in his paper] as 'helpful' [Debreu, March 1982, p. 2].

Arrow had graduated as a mathematics major from CCNY in 1940. He became a graduate student in mathematics at Columbia in that year, intending to study mathematical statistics. He notes, though, "I found when I got there that mathematical statistics was not taught in the Mathematics Department but only by Harold Hotelling, who was a full Professor of Economics, and by an assistant, financed not by the University but by a grant to Hotelling from the Carnegie Corporation. The assistant was named Abraham Wald. Out of curiosity I took Hotelling's course in mathematical economics and immediately became hooked" (Arrow, 1981, p. 1).

Arrow's reading in this period, 1940–1942, was designed to move him from mathematics to economics. He recalls that

> . . . of course Hicks' *Value and Capital* made the biggest impression on me. Somewhere during this year (or possibly the next, when I was actually enrolled in the Economics Department), I realized, probably with some guidance from someone, that the existence of a solution to the equations of general equilibrium was an open question. I also learned, probably from Hotelling, of Wald's papers in the *Ergebnisse* . . . I am pretty sure that I did not hear about Wald's paper from [Wald]; but I do remember asking him about them and about possible generalizations (particularly with regard to the production assumptions). He felt the field was very difficult and did not encourage further work . . . I did read the papers at the time, but in retrospect I feel my understanding was most imperfect. My German was . . . not very good, but I think it was the complexity of the argument that really put me off. I did not believe I was the one capable of really improving on the results [ibid.].

Arrow served in the Weather Division of the Army Air Force from 1942–1946, returned briefly to Columbia upon his military discharge, and joined the Cowles Commission in April 1947. He remained at Chicago until July 1949, when he accepted an appointment at Stanford. (His Ph.D. from Columbia was conferred in 1951.) This period, from the Cowles years through the early 1950s, was an immensely active period for Arrow.

Arrow's paper, referred to above by Debreu, was presented in Berkeley in August 1950. It appeared in *The Proceedings of the Second Berkeley Symposium on Mathematical Statistics and Probability*, edited by Jerzy Neyman (1951). It was titled "An Extension of The Basic Theorems of Classical Welfare Economics," and indeed acknowledged "helpful comments" from Gerard Debreu. (It was reprinted as Cowles Commission Paper, New Series, No. 54.) Its Summary states that "The classical theorem of welfare economics on the relation between the price system and the achievement of optimal economic welfare is reviewed from the viewpoint of convex set theory. It is found that the theorem can be extended to cover the cases where the social optima are of the nature of corner maxima, and also where there are points of saturation in the preference fields of the members of society" (Arrow, 1951, p. 507).

The paper began by reviewing the "marginal analysis" treatment of equilibrium and Pareto-efficiency, pointing out the difficulties with assuring, in a calculus treatment, non-negative prices and the necessary production of *every* product by every firm. The alternative approach, via convex sets, appeared in Section III, with appropriate prior references to von Neumann, Koopmans, Wood, and Dantzig. Arrow worked with "distributions" X_{ij}, where this number represents the amount of commodity i to be given to individual j; X is a social distribution, an m by n array. For given X, the numbers X_{1j}, $X_{2j}, \ldots X_{nj}$ define the bundle X_j. Arrow assumed that quantities consumed must be nonnegative, individuals' preferences were "selfish," and preferences were strictly convex. Suppose the social bundle ($\Sigma_{j=1}^{m} X_j$) is obtained by aggregating goods, so it is a "bundle whose i^{th} component is the sum of the i^{th} components of the bundles $X_1, \ldots X_m$"; then ΣX_j must lie in a set T known as the transformation set (p. 511). Arrow assumed T was nonnull, compact, and convex and, if x is in T, $x_i \geqq 0$ for every component x_i of x. Since preferences are defined over the X_j's, optimality is thus easy to define, given T. Using the separating hyperplane theorem for convex sets, Arrow demonstrated that equilibrium outcomes are optimal, and optimal distributions generate price vectors (which are defined by hyperplanes which are known to support the appropriate convex sets) which equate supply and demand, and are thus competitive equilibria. The paper by Arrow thus nicely dovetails with that of Debreu in indicating how consumers and producers, their respective choices, and competitive equilibria may be defined using the language of convex sets and supporting hyperplanes. The model of a competitive economy had made its appearance in a modern coherent fashion. The welfare properties of a competitive equilibrium for such a model were thus settled by 1951. What remained was the demonstration that there could indeed be such a competitive equilibrium for the model.

X. *Lionel McKenzie's International Trade Models*

Lionel McKenzie was a graduate student in economics at Princeton from 1939–1941. His background was certainly less mathematical than that of either Arrow or Debreu, yet in 1940–1941 he attended a course given by Morgenstern which examined Hicks' *Value and Capital*. He recalls that Morgenstern "cited the deep results arrived at on this subject [of solvability] by Abraham Wald and John von Neumann . . ." (McKenzie, January 1982, p. 1. This obviously was at the time Morgenstern was writing his *JPE* review of Hicks.) McKenzie also recalls that "I was present when von Neumann presented his growth model to the Princeton graduate economics seminar . . . However as Morgenstern [notes in his *JEL* memoir] the paper was not understood by anyone present, and so far as I am concerned, he is right" (ibid.).

There was another major influence on McKenzie at Princeton, the distinguished international trade theorist, Frank Graham. In his 1974 conference paper "Why Compute Economic Equilibria," McKenzie noted:

> When I was a student of Frank Graham in the academic year 1939–40, he gave us a simple general equilibrium model of world trade as an exercise for his course. This model involved several countries and several commodities, and we knew no algorithm for solving it. We used trial and error [p. 1].

McKenzie, in that 1974 paper, quotes from a footnote in Graham's 1948 book *The Theory of International Values* on this subject. Graham wrote:

> It has been suggested that a mathematical formula should be developed which would provide the solution instanter. This would, surely, be desirable, but mathematicians of great repute, to whom I have submitted the problem, have been unable to furnish any such formula (perhaps because they were not sufficiently interested to devote to it the necessary time) [p. 95].

McKenzie commented on Graham's remark by noting ". . . verbal tradition at Princeton was that the mathematician of [Graham's] footnote was John von Neumann . . . It was also part of graduate student lore that a famous colleague [Morgenstern?] chided Graham for having no existence proof and no proof of uniqueness of equilibrium" (McKenzie, 1974, p. 5).

In any event, McKenzie's student work was interrupted by the war; he saw military service in Panama. Upon demobilization McKenzie studied economics at Oxford, recalling "[Although] I read Wilson's *Advanced Calculus* one summer [at Princeton] . . . I was also reading philosophy and did not pursue the mathematical approach to economics" (McKenzie, 1982, p. 2). McKenzie's Oxford thesis, done under Hicks' supervision, was on "cost-benefit type analysis à la Harberger" (ibid., p. 2); it was judged to need revision by Roy Harrod and Hubert Henderson. McKenzie refused, so settled for a BLitt degree, which in any event was no obstacle to his employment at Duke University in 1947.

While at Duke, in 1949, McKenzie noticed an abstract of the Koopmans paper given at the Econometric Society (Cowles) meeting on activity analysis. He recalls:

> I decided that this was just the type of theory I needed[29] so I wrote to Jacob Marschak at the Cowles Commission in Chicago about the possibility of visiting. This led to my stay at Chicago for one full year (12 months) in 1949–50, where I attended the seminars of Koopmans and Marschak and had the company of John Chipman, Edmond Malinvaud, Gerard Debreu, Martin Beckmann, et alia, taking the equivalent of a master's program in math. In Koopmans' class I wrote a term paper on Specialization in Graham's Model of World Trade [ibid., p. 2].

That term paper, revised after McKenzie's return to Duke, appeared in the *Review of Economic Studies,* 1953–1954, *21* (3, No. 56, pp. 165–80) as "Specialisation and Efficiency in World Production." It began by noting that

> . . . the systems of Walras, Cassel, and Leontiev assume a given set of productive processes which are always in use. There is one field of traditional economics, however, where explicit discrimination between processes to be used and processes to be suppressed has always been the fundamental object of analysis. The problem of specialisation in international trade according to comparative advantage is precisely the problem of selecting a group of productive processes to be used in the interest of maximum world output . . . Although the classical economists would no doubt expect these points [of competitive equilibrium] to be points of maximum world output, they did not fully expose the relation of maximum output to the possibility of competitive equilibrium" [McKenzie, 1953–1954, p. 165].

The paper is well-structured. First Graham's general equilibrium international trade model is "reduced" to a model of competition where countries play the role of firms, and intermediate goods are ignored to concentrate on trade in final goods produced from a linear technology. The trade model is thus formally equivalent to the Koopmans activity analysis model, and the pattern of trade is found using techniques introduced by Koopmans and Reiter in the Cowles conference volume paper "A Model of Transportation" (in Koopmans, 1951). In outline, the Koopmans analysis was used to show that, for the Graham-McKenzie trade model, maximum world output is an "efficient equilibrium" in the sense of activity analysis. For a specific model of *m* countries, with *n* final goods able to be produced in each, McKenzie's first result was that sensible trade model restrictions assure specialization (and thus comparative advantage theorems) for each country at positive world prices. The equilibrium *pattern* of trade that emerged resembled a network problem, solvable by algorithms developed for the ship-routing problem in the nascent programming literature.

While still at Cowles, McKenzie had begun thinking about the question of existence of an equilibrium for the competitive model, especially in the context of general equilibrium of international trade. "I recall walking back from Koopmans' class with Koopmans, Beckmann,

[29] "The need for further mathematical economics had been brought to my attention when I was writing my paper (1949) on Ideal Output [which appeared in the *Economic Journal,* December, 1951] . . . [The] footnotes on pp. 794, 795, and 797 [of that paper point to the directions I was to take]" (McKenzie, April 1982, p. 3).

and Chipman on one occasion after he had discussed the relation between activity analysis and competitive equilibrium . . . [I asked] Koopmans about the existence question. He replied that it was a very deep question which had not been answered to that time" (McKenzie, January 1982, p. 2).

Yet it was not until McKenzie's return to Duke, and his examination of the just-published volume on *Activity Analysis of Production and Allocation*, that he found references to the Wald and von Neumann papers, which he located in the Duke mathematics library. Thus in 1951, while writing up the paper on specialization, McKenzie ". . . [s]omehow . . . got the idea for the mapping of social demand from the origin on the world production possibility frontier [as a technique] for proving existence [of a competitive equilibrium in the Graham model]" (ibid., p. 2). He recalls:

> I first worked this out with a smoothing of the frontier and the use of Brouwer's theorem. Then I recalled a Cowles Commission paper I had brought back with me from Chicago by Morton Slater on Kakutani's fixed point theorem and I saw that this was just what was needed for an elegant proof of the theorem . . . of course von Neumann had used essentially this theorem . . . in his paper [of 1937] . . . I had also acquired while at Chicago a set of notes [written by P. T. Bateman?] from a seminar on convex sets given by Marston Morse at the Institute for Advanced Study in Princeton in 1949–50 . . . they were useful to me in establishing the continuity properties of the support function of a convex set and my projection on the production possibility frontier, but also in helping me to understand the problem in a geometrical way [ibid., p. 3].

Except for McKenzie's remark that the idea of a "cone technology" did not play a real role in early versions of this existence proof, the paper, "On Equilibrium in Graham's Model of World Trade and Other Competitive Systems" which appeared in *Econometrica* (*22*, 1954, pp. 147–61) can be taken to define McKenzie's 1951–1952 analysis of existence of a competitive equilibrium. Given the length of the story to this point, some attention to this paper, and to those of Debreu, and Arrow and Debreu (to be considered in the next section) seems appropriate.

McKenzie's paper began by discussing the Wald papers and Graham's model and by noting that while Graham's demand functions satisfy the Wald restrictions (the Weak Axiom of Revealed Preference in the aggregate), Graham's production sector does not meet Wald's conditions; Graham used equalities not inequalities. McKenzie then stated that "It is my purpose to develop a more general existence proof where the demand functions are not confined so narrowly."

The model contained primary goods which are the labor supplies of n various countries; there are k final goods. The world technology is represented in a linear activities model by a partitioned matrix

$$A = \begin{bmatrix} A_{fin} \\ A_{pri} \end{bmatrix}$$

where $A^j_{pri} = [-1,. . .-1]$ so

$$A = \begin{bmatrix} A^1_{fin} \cdot & \cdot & \cdot A^n_{fin} \\ A^1_{pri} & & 0 \\ & \cdot & \\ & & \cdot \\ & & \quad\cdot \\ 0 & & A^n_{pri} \end{bmatrix}$$

when "*pri*" and "*fin*" denote primary and final goods.

If x denotes a column n-vector of activity levels, η denotes labor supplies, y_{fin} is a final output vector, y_{pri} is a labor input vector, w's refer to labor quantities, and p's are prices, then the model becomes, using McKenzie's numbering scheme:

Production system

$$\begin{bmatrix} A_{fin} \\ A_{pri} \end{bmatrix} x = \begin{bmatrix} Y_{fin} \\ Y_{pri} \end{bmatrix} \qquad (2.4)$$

$$x \geqq 0 \qquad (2.5)$$

$$y_{pri} \geqq -\eta < 0 \qquad (2.6)$$

Demand functions:

$$w_{jpri} = -\eta_j;\ p_{jpri} > 0 \qquad (j = 1, 2, \ldots n). \qquad (2.7)$$

$$w_{ifin} = b_i \frac{r}{p_{ifin}} \qquad (b_i > 0, \Sigma b_i = 1, i = 1, \ldots k). \qquad (2.8)$$

$$r = p_{fin} y_{fin} \quad ; \quad p_{ifin} > 0 \text{ [p. 149].} \qquad (2.9)$$

It is also assumed that $a_i^j > 0$, so each submatrix A_{fin}^j of A_{fin} in (2.4) is non-zero. r is, of course, world income and b_i is the proportion of world income devoted to the i^{th} final good.

For this model, McKenzie established directly that if X is the set of activity levels x satisfying (2.4) – (2.6), X is compact and convex. Since feasible vectors y are such that $y = A_{fin}x$, the set of feasible y, denoted Y, is also compact and convex. McKenzie then showed that "$z \epsilon Y$ is efficient" when $z \epsilon Y$ and $\alpha z \notin Y$ for $\alpha > 1$. Such z are called "extreme outputs," and they include the efficient points.

The economy's income is $p_{fin} \cdot y_{fin}$. "We shall then regard an equilibrium position as a price vector p and an attainable input-output vector y, which (1) satisfy the demand functions and (2) leave producers with no opportunity for increasing profits" (p. 152).

The profit condition is clearly equivalent to "(4.1) $p \cdot A_i^j \leqq 0$ and $p \cdot A_i^j = 0$ if $x_i^j > 0$" (p. 152. Here all goods are final goods.)

McKenzie proceeded then to characterize equilibrium:

> We may now state that (p,y) is a competitive equilibrium in the Graham model if and only if
>
> (a) y_{fin} is attainable according to (2.4), (2.5) and (2.6) (4.2)
>
> (b) The profit condition (4.1) is met.
>
> (c) p and y satisfy the demand functions (2.7), (2.8) with (2.9) and $w = y$ [ibid.].

The strategy of establishing existence of a competitive equilibrium thus involved producing p's and y's which satisfy (4.2) (*a-c*). (Hereafter the subscript *"fin"* is dropped.) The proof was given in sections 5 and 6 of the paper.

Section 5 showed that *if* an equilibrium output exists, it is unique. The working assumption of this demonstration was the Weak Axiom of Revealed Preference which McKenzie noted was "Samuelson's fundamental postulate for the theory of demand for a single consumer . . . [and which] Wald believed . . . probably held also for the body of consumers [in the aggregate]" (p. 154, note 23). The argument was straightforward, using Y's convexity, equilibrium, and the Weak Axiom.

The existence proof of section 6 did *not* use the axiom, but did postulate that the demand functions for final goods are homogeneous, non-negative, and continuous for positive prices. Also, for fixed income there is no satiation of demand for a good as its price goes to zero. The "economics" of the proof was explained simply: "We shall consider all social budget planes which touch the production transformation surface. Then we assume that total purchases, when the goods are available, will always lie on the budget plane . . . Finally, we will prove that there is a budget plane for which the chosen point lies in the section of the plane which touches the production transformation surface, assuming the demand functions to be continuous functions of the prices of final goods" (p. 155).

Normalizing prices, McKenzie took p in the closed unit simplex S (a compact, convex set). Every $p \epsilon S$ is normal (perpendicular) to some $y \epsilon Y$, where Y is also compact and convex. Now $r(p) = max\ p \cdot z$ for $z \epsilon Y$ is continuous, and so is $h(w) = \alpha w$, $\alpha \geqq 1$, $\alpha w \epsilon Y$, $kw \notin Y$ for $k > \alpha$. ($h(w)$ is the intersection of the ray from the origin containing w, a non-interior point of

Y, with the set of extreme outputs of Y.)

If y is in the set of extreme outputs, define "$g(y) = K_y$ where K_y is the intersection of the set of normals to y [which include p's] with S" (p. 157). McKenzie then established that g was upper semi-continuous. But the domain of g is a compact convex set, and K_y is a convex set, so the composition mapping $F = g \cdot h \cdot f^*$ (where f^* is the demand function suitably restricted by using $r(p)$) involves an upper semi-continuous map composed with two continuous ones. F is thus upper semi-continuous. Further, the domain of F is S, and its range is $\{K_y\}$, a set of compact convex subsets of S. The Kakutani theorem immediately yields that F has a fixed point, "so there is a p which is contained in the set K_y into which it maps [by F]" (p. 158).

If p^* is the fixed point, define $y^* = h(f(p^*))$. Then this (p^*,y^*) is easily shown to be the competitive equilibrium, demonstrating (p. 158) "Theorem 2: Any Graham model has a competitive equilibrium."

The final sections of the paper generalized the results using a free disposal assumption, variable labor supplies, and many primary goods under the assumption that consumers always have income. An appendix demonstrated the continuity of $r(p)$ and $h(w)$.

McKenzie presented his results a year and a half after he left Cowles. He notes:

> My paper and the paper of Arrow-Debreu, which were developed completely independently, were presented to the December, 1952 Chicago meetings of the Econometric Society. I recall that Koopmans, Debreu, Beckmann, and Chipman were at my session. The Arrow-Debreu paper had been given the day before and I had stayed away. However, Debreu rose in the discussion period to suggest that their paper implied my result. I replied that no doubt my paper also implied their results. As it happens, we were both wrong. Debreu [has told me] he spoke up after asking Koopmans' advice before the session. Later in his office, Debreu gave me a private exposition of their results [McKenzie, January 1982, p. 3].

XI. *The Arrow-Debreu Model*

Debreu had not attended the 1949 Cowles Commission Conference on linear programming; he joined the Cowles staff in June 1950. His paper on "The Coefficient of Resource Utilization," published in July 1951 and certainly written prior to June 1950, had gone part way to an analysis of existence of a competitive equilibrium. Debreu recalls that:

> it was when [the Koopmans monograph] was published that I learned of the existence of A. Wald's papers on general economic equilibrium, and only when the English translation [of Wald's *Zeitschrift* paper] appeared in *Econometrica*, October 1951 [as a memorial to Wald who, with his wife, had died in a plane crash in India] did I get acquainted with its contents. At that time, in the Fall of 1951, I was already at work on the problem of existence of a general economic equilibrium . . . The influences to which I responded in 1951 were the tradition of the Lausanne School and, in particular, the writings of Divisia, Hicks, and Allais; the theory of games and, in particular, the article of J. Nash; the [paper on fixed points by] Kakutani and the [1937] article of von Neumann . . .; the linear economic models of the Cowles Commission monograph of 1951 edited by T. C. Koopmans [Debreu, December 7, 1981, p. 2].

The analysis that Debreu was doing paralled the thinking of Kenneth Arrow, who had gone from Cowles to Stanford in 1949. Arrow notes:

> According to my recollection, someone at RAND prepared an English translation of the [Wald] *Ergebnisse* papers to be used by Samuelson and Solow in their projected book (sponsored by RAND), which emerged years later in collaboration with Dorfman. I read the translations and somehow derived the conviction that Wald was giving a disguised fixed-point argument (this was after seeing Nash's papers). In the Fall of 1951 I thought about this combination of ideas and quickly saw that a competitive equilibrium could be described as the equilibrium point of a suitably defined game by

adding some artificial players who chose prices and others who chose marginal utilities of income for the individuals. The Koopmans paper then played an essential role in showing that convexity and compactness conditions could be assumed with no loss of generality, so that the Nash theorem could be applied.

Some correspondence revealed that Debreu in Chicago . . . was working on very similar lines, though he introduced generalized games (in which the strategy domain of one player is affected by the strategies chosen by other players). We then combined forces and produced our joint paper [Arrow, 1981, p. 2].

This collaboration led to two papers, the first by Debreu, the second by Arrow and Debreu. They should be read as a pair. The former was communicated to the *Proceedings of the National Academy of Sciences* on August 1, 1952 and appeared in Volume 38 of those *Proceedings* in 1952, pp. 886–93. The second paper, by Arrow and Debreu, was read on December 27, 1952 at the same Chicago meetings of the Econometric Society that heard McKenzie's paper on December 28. It was published as "Existence of an Equilibrium For A Competitive Economy" (*Econometrica, 22,* July 1954, pp. 265–90).

The Debreu paper is the heart of the existence proof of the Arrow-Debreu article. Debreu began by noting that "The existence theorem presented here gives general conditions under which there is for [a certain type of] social system an equilibrium, i.e. a situation where the action of every agent belongs to his restricting subset [of actions constrained by choices made by other agents] and no agent has incentive to choose another action. This theorem has been used by Arrow and Debreu to prove the existence of equilibrium for a classical competitive economic system . . ." (Debreu, 1952, p. 887). The initial paragraphs introduce the necessary material on convex sets, agents, and their actions, and define equilibrium in the sense of Nash, but in a more general form. The basic idea of the paper is to restrict the choice set of the agent in such a fashion that, if his objective can be summarized by a function defined on the choice set with suitable continuity properties, there is an equilibrium for the generalized game. To appreciate the Debreu paper it is important to see how it was used in the Arrow-Debreu article. That paper began by introducing the notion, and meaning, of a competitive equilibrium. The next section (Section 1) introduced the notation for vector bundles and used the Koopmans production schema. For each of the n production units, j, y_j is a production plan (an ℓ-vector) in a set Y_j. Let aggregate productions be specified by $Y = \Sigma Y_j$. Then Arrow and Debreu assume

I. a. Y_j is a closed convex subset of R^ℓ containing 0; $(j = 1, \ldots n)$.
I. b. $Y \cap \Omega = 0$. [Ω is the non-negative orthant of R^ℓ.]
I. c. $Y \cap (-Y) = 0$ [p. 267].

With the technology so described, if p is a ℓ-vector of prices, then (p^*, y^*) is a competitive equilibrium only when "1. y_j^* maximizes $p^* \cdot y_j$ over the set Y_j, for each j" (ibid.).

Consumption behavior is treated in

II. The set of consumptions X_i available to individual i $(i = 1, \ldots m)$ is a closed convex subset of R^ℓ which is bounded from below . . . [and if u_i is a utility indicator for individual i]
III. a. $u_i(x_i)$ is a continuous function on X_i.
III. b. For any $x_i \epsilon X_i$, there is an $x^1{}_i \epsilon X_i$ such that $u_i(x^1{}_i) > u_i(x_i)$.
III. c. If $u_i(x_i) > u_i(x^1{}_i)$ and $0 < t < 1$, then $u_i[tx_i + (1-t)x^1{}_i] > u_i(x^1{}_i)$ [p. 268–69].

The system is closed by supposing that individuals have initial resource holdings (e.g. labor to be supplied) and they own the various production units.

. . . the i^{th} consumption unit is endowed with a vector ζ_i of initial holdings of the different types of commodities available and a contractual claim to the share α_{ij} of the profit of the j^{th} production unit for each j.

IV. a. $\zeta_i \epsilon R^{\ell}$; for some $x_i \epsilon X_i, x_i < \zeta_i$;
IV. b. for all i, j, $\alpha_{ij} \geqq 0$; for all j, $\Sigma_{i=1}^{m} \alpha_{ij} = 1$ [p. 270].

At an equilibrium x_i^* for the i^{th} consumer, it must be true that consumers are at an optimum, as 1. above said that producers are at an optimum. More formally,

2. x_i^* maximizes $u_i(x_i)$ over the set $\{x_i | x_i \epsilon X_i, p_i^* \cdot x_i \leqq p^* \cdot \zeta_i + \Sigma_j \alpha_{ij} p^* \cdot y_j^*\}$ [p. 271].

Prices are normalized to P, the unit ℓ-simplex, so

3. $p^* \epsilon P = \{p | p \epsilon R^{\ell}, p \geqq 0, \Sigma p_i = 1\}$ [and if $x = \Sigma_i x_i$, $y = \Sigma_j y_j$, $\zeta = \Sigma_i \zeta_i$, $z = x - y - \zeta$, then market equilibrium is described by]
4. $z^* \leqq 0$, $p^* \cdot z^* = 0$ [p. 271].

Thus a competitive equilibrium is described by conditions 1–4. The result Arrow and Debreu sought to establish was "Theorem I: For any economic system satisfying Assumptions I–IV, there is a competitive equilibrium [defined by conditions 1–4]" (p. 272).c7

Section II of the paper presents the Debreu result on equilibrium in an abstract economy. Section III reshapes the competitive economy into an abstract $m + n + 1$ person (Debreu) game in an imaginative fashion. Let $A_i(\bar{x}_i)$ be the set of generally feasible consumption bundles for consumer i. Then

> each of the first m participants, the consumption units, chooses a vector x_i from X_i subject to the restriction that $x_i \epsilon A_i(\bar{x}_i)$, and receives a payoff $u_i(x_i)$; the j^{th} out of the next n participants, the production units, chooses a vector y_j from Y_j (unrestricted by the actions of other participants) and receives a payoff of $p \cdot y_j$; and the last agent, the market participant, chooses p from P . . . and receives $p \cdot z$ [p. 274].

Arrow and Debreu's proof then proceeded in two major steps. First, it was shown that *if* an equilibrium exists (in the Debreu game sense) for this economic system, then that equilibrium is a competitive equilibrium as defined by conditions 1–4.

The second step requires a demonstration that the sets from which the participants choose are appropriately compact and convex. This was the delicate part of the proof; it required adjustment of the choice sets, then analysis to show that the adjustments did not affect the argument. The final step required a demonstration that the payoff functions of the various participants were appropriately continuous. This argument too has some delicate steps but, upon completion of the demonstration, the hypotheses of the Debreu theorem were established, and an equilibrium point was inferred. The theorem was thus established.

Sections IV and V weaken the assumption "that a consumption unit has initially a positive amount of every commodity available for trading" (pp. 279–80). The final Section VI, provided a short historical note which discussed the arguments, and models, of Cassel, Neisser, Stackelberg, Schlesinger and Wald. Arrow and Debreu remarked, with respect to Wald, that their paper is "much more general since Wald assumes fixed proportions among the inputs and a single output of every process. On the demand side, he makes assumptions concerning the demand functions instead of deriving them, as we do, from a utility maximization assumption. It is on this point that no direct comparison is possible . . . [although the effect of Wald's use of 'Samuelson's postulate' leads to the conclusion that in effect] he assumes a single consumption unit" (p. 289).

Arrow has commented upon this 1951 discussion of Wald, noting

> I may add that my reading of Wald in 1951 under the influence of having read Nash was wrong. I first realized this when I read a draft of McKenzie's [1977] Presidential Address to the Econometric Society [published in *Econometrica* July 1981, 49 (4), as "The Classical Theorem on Existence of Competitive Equilibrium" pp. 819–841], which asserted that his paper and that of Debreu and myself are fundamentally different from and more general than

Wald's in the treatment of consumers . . . This is not the way I had read Wald, and wrote to McKenzie to contradict him; but then I reread Wald's original papers (with the hindsight of all the work I had done, the task was no longer so formidable) and found that McKenzie was 100% right. The Weak Axiom enormously simplified the problem. Hence, my generalization in that direction was taken without awareness that I was generalizing Wald! [Arrow, 1981, p. 2].

One final point is worth noting. The Debreu part of the proof of existence for the Arrow-Debreu model was communicated to the *Proceedings of the National Academy of Sciences* by a member of the Academy. That member was von Neumann.

A Concluding Note

In this essay I have concentrated on the lines of development which culminated in the papers of Arrow-Debreu, and McKenzie. I have thus slighted the independent contribution of Hukukane Nikaido whose paper (reprinted in Newman, 1968) on multilateral exchange equilibrium was delayed in publication by *Metroeconomica* until 1956. Likewise the papers by Kuhn, and Gale (also reprinted in Newman, 1968) fall outside the scope of this narrative.

The cumulative nature of the existence-of-equilibrium literature, from Wald and von Neumann to Arrow, Debreu and McKenzie, involved many individuals, and a great deal of work. The story told here is linked to the creation of the theory of games, fixed point theory and activity analysis. This connection is not usually noted in textbook accounts. The Walrasian tradition and the Keynesian program, interacting in curious ways, is worth further study.[30] The magnificent genius of von Neumann, and his influence on modern economics, has been too seldom appreciated. The "equilibrium" story is one in which empirical work, ideas of facts and falsification, played no role at all. There are thus some lessons and implications for both the history and philosophy of economics. Perhaps this narrative will facilitate exploration of some of these issues.

[30] The connection between these two programs is clearer in the "stability" literature than in the "equilibrium" analysis. (See Hands, 1982, for an analysis of the stability of equilibrium papers in a spirit similar to my own narrative.)

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