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## **Lionel W. McKenzie and the Existence of a Competitive Equilibrium, Redux**

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### **Part I: Memoir as Historiography**

I first heard of Lionel McKenzie in 1966 as I made the transition from the graduate program in mathematics at the University of Pennsylvania to its graduate program in applied mathematics, with my "application" being economics. Lawrence R. Klein, my advisor, sent me to read material on the stability of general equilibrium, and so I became familiar with

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McKenzie's work. In the stability literature McKenzie had explored restrictions on the Jacobian matrix of the individual markets' excess demand functions, associated in exchange economies with properties of utility functions, which allowed inferences about the Jacobian's eigenvalues. Since negative definite matrices, with negative real parts of eigenvalues, sufficed to demonstrate stability of the underlying linear system, these theorems allowed one to link economic assumptions with the local stability of general competitive equilibria. Thus I became aware of McKenzie's work on dominant diagonal theorems and saw how his results fit together with the stability analysis of Arrow and Hurwitz more generally, analysis dependent on properties of the Liapunov function associated with the particular tâtonnement dynamical system.

Even though it was the time of burgeoning work in capital theory, in growth theory and multisector growth models, and even though Edwin Burmeister was on my thesis committee, I never paid much attention to those ideas as I pushed forward, with narrow focus, on generalizing results on the stability of competitive equilibrium to a stochastic environment. Thus I never did come across McKenzie's work on growth theory, and turnpike theorems, even though those ideas were important to mathematical economic theory in the second half of the 1960s.

As matters developed, I finished a dissertation, and got a job, and began writing papers based on my dissertation. Those papers concerning the stochastic stability of stochastic general equilibrium systems did not find ready acceptance in journals, and this was not to change until Steven Turnovsky, at Harvard, wrote a dissertation (supervised by Robert Dorfman) similar to mine but using more restricted discrete time methods and difference equations. At that point there was a short burst of interest in these models, prompted perhaps by my collaboration with

Turnovsky, which was fortunate because I was trying very hard to leave my first job for a more conducive work environment. Since I had been a graduate student in mathematics, I had had no natural way to break into the economics job market -- my thesis advisor was spending my "job market year" teaching in Osaka in Japan. My advisor did get me into the "job market" mix of economics graduate students the following year, and I made plans to attend the American Economic Association meetings in New York City in December 1969 .

In the early fall of 1969 I received a letter from Wassily Leontief, AEA President, congratulating me as one of four young scholars selected to present my thesis results at the AEA meetings, in the invited doctoral dissertations session. Since aside from the invited plenary sessions, this was the only invited session on the program, I was thrilled. When I arrived at the meetings, and went to the session where I was to present my thesis results, it turned out that my discussant was Lionel McKenzie. After I gave my presentation, he got up and made comments about the paper and the results which were positive and encouraging and I remember sitting there, listening to him, feeling that life could not be better. After the session, McKenzie and I chatted briefly and cordially as I thanked him for his very kind remarks. He was indeed kinder than he needed to be, since my paper was very opaque. Nevertheless that paper would appear in the *Papers and Proceedings* of those meetings, and so I had a publication in the *American Economic Review*. That was the only time I ever met Lionel McKenzie. On the basis of my new found visibility in the economics profession, and a couple of other publications with revises and resubmits, I had a host of job interviews and one of the best of them was with Duke University. Over that spring, as matters progressed, I became more and more interested in the position at

Duke as Duke became more and more interested in my filling that position, and it eventually led to my paying a recruitment visit to Duke in late spring 1970.

Though I have many memories of that visit, one that especially stands out was my obligatory meeting with the dean of the faculty, the physicist Harold Lewis. In those days when departments talked about faculty lines, there really was a concrete referent for that which is today a metaphor. When I sat in Lewis' office he brought out from a desk drawer his chart of the department of economics which had as its first column numbers in sequence from one to around thirty. The horizontal axis was "year", beginning around 1930. Names appeared on row lines as people were hired, and then the row became blank as they left or died, only to be filled in with another name as the new person hired was added on that line. He showed me one of those lines, around the middle of the page, on which the name Lionel McKenzie had been written in the row and that his name with an arrow from it continued for a number of years from 1947 to 1957. Then that row was blank. He said to me that the position I was being recruited for was on that "McKenzie line". I thought it was fitting and quite delicious that I was going to be, if they offered and I accepted, the job, the new instantiation in the Duke Economics Department of Lionel W. McKenzie.

Over the first half dozen years that I taught at Duke, beginning in 1970, my research concern was to take what knowledge I had about the dynamics of competitive general equilibrium systems and extend that into discussions about the role of general equilibrium analysis in economics more generally. Thus I began reading extensively in areas of what is now called the neoclassical synthesis, at the same time as I was teaching courses in graduate

microeconomics and macroeconomics. At that time, I recall being interested in how micro and macro fit together because after all that was what the neoclassical syntheses was “all about”. Since I had never really studied economics in any detail, my attempts to understand these matters led me to do an enormous amount of reading, and in attempting to synthesize what I knew and was learning, I decided to write about it in the form of a survey article. Mark Perlman, the founding editor of the *Journal of Economic Literature* had paid a visit to Duke University in that period, and suggested to me, following a suggestion to him from my colleague Martin Bronfenbrenner, that I think about doing a survey on the emerging literature on the microfoundations of macroeconomics. In due course I produced that survey and as Perlman was extending such surveys into small books for a Cambridge University Press series titled Cambridge Surveys of Economic Literature, I extended that survey to a modest size book.

Although I was fully familiar with the stability literatures in all their different varieties, until that point I had never spent a great deal of time looking at the somewhat earlier work on the existence of a competitive equilibrium. As a consequence of learning this and teaching this material I began to see the connections among the various early approaches to proving the existence of a competitive equilibrium and I started using the phrase Arrow-Debreu-McKenzie model (ADM model) to provide a reference phrase for the competitive equilibrium model. I was aware that others were calling this the Arrow-Debreu model, but as I noted in my microfoundations book (Weintraub 1979, 27 fn 5), “many authors use the term Arrow-Debreu model. Since, however, the *proof* of existence on which current work is based came out of McKenzie [1959], it seems appropriate to give McKenzie equal billing.”

I realized at that time that this reference was slightly unusual, but my own reading of those literatures suggested that it was entirely fair. As a consequence, I appreciated the fact that in subsequent years individuals like Hugo Sonnenschein also began to referring to the ADM model instead of the Arrow-Debreu model. It appears then that mine was the first such use of the phrase, which has pleased me over the years.

By around 1980 I was continuing to do work in general equilibrium theory, but I was increasingly perplexed. Specifically, I became concerned with issues of the epistemological status of neoclassical economics, or general equilibrium theory, which I had understood, and was teaching as, the core of neoclassical economics. Those issues of what is termed methodology grew increasingly to frustrate me, as there seemed to be no way to reconstruct the bases of the theory without having a much clearer idea of how the analysis of the general competitive model had developed. Most treatments were analytical, and dealt with the history of these ideas only in the kind of schematic terms that mathematicians would use to describe the specific antecedents of the problems they worked on. Evaluating the claims on the theory without having an historical understanding of the development of the theory seemed to me to be a foolish way to proceed. Consequently, in order to develop the history in order to use it as case study materials for discussion of the philosophical bases of the theory, I began to do historical work. My colleagues, Craufurd Goodwin and Neil De Marchi facilitated this new adventure, and having as students Arjo Klamer, Janet Seiz, and Rodney Maddock provided me with a ready sounding board for musings and speculations. Since De Marchi and I had sent Klamer off to talk to new classical economists, doing a proto oral history of the development of new classical economics, it seemed reasonable for me to make direct approaches to as many of the individuals associated

with the development of existence proofs of the ADM general equilibrium model as I could locate. With the encouragement of Moses Abramovitz, then editor of the *Journal of Economic Literature*, and Craufurd Goodwin who was hosting the *History of Economics Society* meetings in 1982 at Duke University, I made plans to develop a history of the existence of competitive equilibrium analysis to present to those *HES* meetings and to further develop it into a survey piece for the *JEL*. During the 1980-81 academic year I read nearly everything I could get my hands on from that postwar period, and wrote letters to as many of the participants as I could find. My files show that this group included Kenneth Arrow, Gerard Debreu, Lionel McKenzie, Tjalling Koopmans, Gerhard Tintner, Carl Christ, John Chipman, Nicholas Georgescu-Roegin, and Paul Samuelson. I also was in contact with Allen Wallis, and Paul Wolfowitz, son of Jacob Wolfowitz who was Abraham Wald's literary executor.

As a result of the highly structured questions I had sent out to these individuals, and my detailed reading of the work done at that time, I began to shape a perspective on the development of general equilibrium (the existence proofs) from the period of the early 1930s in Vienna through to the *Econometrica* papers in the mid 1950s. It was a complex story with many threads and many linkages. My narrative explored in detail the work done in Karl Menger's Vienna in association with Menger's mathematical colloquium, and tracked the movement of people and ideas to the United States both before and after the Anschluss. I was fascinated by the role of Abraham Wald, and the impact of John von Neumann. Thus my questions and responses from the various participants concerned mostly the background of the connections from Europe. My story then picked up again with the biographies and intellectual development of Kenneth Arrow, Gerard Debreu and Lionel McKenzie, bringing the latter two on stage for the 1953 meetings of

the Econometric Society in Chicago, and the subsequent publication of the Arrow-Debreu and McKenzie papers on the existence of a competitive equilibrium. Using the correspondence exchanges I had had, and the hints and suggestions to places to look in the various published literatures, I was thus able to tell a story which credited Arrow, Debreu, and McKenzie without raising issues of the appropriate share of recognition for each of them. I persisted however in referring to the Arrow-Debreu-McKenzie model.

During the period that I was gathering information for the paper (which eventually appeared in the *JEL* in March 1983), I began to sense that a bit below the surface of the history I was constructing lay some issues that were difficult for my respondents to address, and thus for me to understand fully. This part of the story has to begin with the announcement on October 25<sup>th</sup>, 1972 by the Royal Swedish Academy of Sciences of the award of the Bank of Sweden Prize in Economic Sciences in Memory of Alfred Nobel to John R. Hicks and Kenneth Arrow. The specific press release identifies the award “for their pioneering contributions to general economic equilibrium theory and welfare theory.” The discussion of Arrow’s work mentions “the pioneering work, a paper from 1954, was written together with Gerhard [sic!] Debreu.” It goes on to state that the model presented in their paper became the starting point for further research in this field. The discussion then moves off to Arrow’s work in welfare economics associated specifically with the fundamental theorems of welfare economics, the Pareto efficiency of the competitive equilibrium. But nowhere is Arrow’s contribution to social choice theory mentioned, and nowhere is it noted that Debreu also published papers on welfare economics, e.g. his paper “A Fundamental Theorem on Resource Allocation”.

It was a curious pairing, Arrow with Hicks. The latter's book *Value and Capital* appeared in 1939 developing the ideas of choice theory presented earlier in the 1930s, and extending some of the competitive equilibrium arguments to a dynamic setting, but it played virtually no role in subsequent developments in general equilibrium analysis. Moreover, Hicks himself even as early as 1972 was in the process of repudiating, or at least back pedaling away from, his 1939 book, and to oppose the intellectual frameworks of both IS-LM analysis and general equilibrium theory.

Thus the question of the assignment of "credit" for work in general equilibrium was confused even before 1972, else Hicks and Arrow could not have been linked in any kind of symmetric fashion. It is curious, moreover, that Debreu was described in terms similar to that of Arrow but that there was no prize for him. And there was no mention of the role of Lionel McKenzie and the other pioneers in the development of proofs of the existence of a competitive equilibrium, Hukukane Nikaido and David Gale.

From my earlier decision in the 1970s to refer not to the Arrow-Debreu model but rather to the Arrow-Debreu-McKenzie model, I was aware that there were issues associated with the apportioning of credit. The issues of priority, and the complex intertwining of contributions that Robert Merton (1973) described and analyzed so well as the problems of simultaneous discovery, were not at the forefront of my thinking, but must have been lurking beneath the surface. Thus some of the correspondence that I had with the participants, as I was constructing the 1983 paper, never "made it" into the paper itself, as I did not understand at that time what the "crediting" issues might have been.

In one of the early responses to my set of questions, on November 19th, 1981, Kenneth Arrow wrote to me as follows:

“By the time I got to the Cowles Commission in 1947, there seemed to be more awareness about the existence question and of Wald’s work. Patinkin was stimulated enough to write to Wald about the importance of the inequalities in the definition of equilibrium. Wald replied that they were essential to the proof, a point which I hadn’t understood, so this correspondence made an impression on me. The next crucial stages in my development were, on the one hand, Nash’s papers on the equilibrium point as a solution concept for gains and on the other the development of production theory on the basis of linear programming, by Koopmans, as you surmised. According to my recollection, someone at Rand prepared an English translation of the *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 competitive equilibrium could be described as the equilibrium point of the

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 showing that convexity and compactness conditions can be assumed with no loss of generality so that the Nash theorem could be applied. Some correspondence revealed that Debreu in Chicago (I was in Stanford by this time) was working on very similar lines, [in fall 1951], 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. Meanwhile, McKenzie, working independently, had published his paper first, though it was somewhat less general.” (p. 2)

In Debreu’s initial response to my inquiry, in a letter dated December 7 1981, he recalled that

“after having spent the first six months of 1949 at Harvard, I visited Berkley in the summer and the Cowles Commission in Chicago for several weeks in the fall. In June 1950 I joined the Cowles Commission as a staff member for a period of more than 10 years...during my visit to the Cowles Commission in the fall of 1949 I did not have close contacts with the faculty...According to

my recollection, 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 the most important of those papers appeared in *Econometrica*, October 1951, 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, and insofar as I can trust my memory of events that took place 30 years ago, the research I did on that question was not stimulated at its inception by Wald's articles, nor, I believe, was it influenced in its development by them." (pp. 1-2)

In his first response to my inquiries, Lionel McKenzie traced his own intellectual development and wrote

"my paper on 'Ideal output and the interdependence of firms', *Economic Journal* 1951, more or less developed from the [Oxford, with Hicks] thesis. I wrote this paper at Duke in 1946-48. I saw in the problems that arose in this paper the need for a general equilibrium analysis. ...At about the same time I noticed an abstract to a paper given by Koopmans to an Econometric Society meeting on activity analysis. I decided that this was just the type of theory I needed 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...”

On his return to Duke following the year at Cowles, McKenzie pursued several questions that he had begun to address in a paper written for Koopmans’s class at Chicago. Regarding this paper, on the existence of equilibrium (model of world trade), he noted

“I believe I was the first to use the Kakutani theorem this way, although I believe Nikaido’s use of it in his paper in *Metroeconomica* (1956) was independent of mine. His paper was delayed in publication. ...My paper and the paper of Arrow-Debreu which were developed completely independently, were presented to the December, 1952, meetings of the Econometric Society [in Chicago]. I recall that Koopmans, Debreu, Beckman, 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 result. As it happens, we were both wrong. Debreu says he spoke up after asking Koopmans’s advice before the session. Later in his office, Debreu gave me a private exposition of their results.”

To reiterate, this material was embedded in longer letters I had received from Arrow, Debreu, and McKenzie. I used the bare responses, together with the responses from Chipman, Koopmans, Tintner, etc. to construct the draft of the paper I submitted to the *JEL* with a letter to Abramovitz dated March 2, 1982. I also sent copies of that draft to at least Arrow, Debreu, McKenzie, Koopmans, and Chipman. The first response I received to this second round of letters, dated March 24, 1982, was from Debreu. In it, he expanded his remarks about having learned of the Arrow paper while at Cowles, commenting

“In 1950-51, 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 (“On Extensions of the Classical Theorems of Welfare Economics”) 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 proceedings of the second Berkeley Symposium...where Arrow’s article was to appear”.

With respect to the session in which McKenzie delivered his Econometric Society paper, Debreu wrote to me that “I have no recollection of the episode recounted...and I cannot testify one way or the other on this matter. I bring this question up because you might have interpreted

absence of comment on my part as an endorsement of the statements that you quote. T.C. Koopmans may possibly remember what happened at that session. ...Another point must also be noted that according to the [Weintraub] account Lionel had not attended the seminar where I spoke and had no knowledge of the Arrow-Debreu paper. It's stated the next day that [he said] his paper implied our result." And with respect to Arrow, Debreu notes that "I met [Arrow] neither when I visited the University of Chicago in the fall of 1949, nor when I joined the Cowles Commission in June 1950. Indeed our first meeting took place in December 1952 at Stanford."

McKenzie's own response to my draft, dated April 16, 1982, raises one specific point relevant to this present discussion: he wrote that "[with respect to Arrow and Debreu's references to Wald] I assume that the remarks on page 289 of [the] Arrow-Debreu existence paper about the weak axiom were meant to imply that the weak axiom was used to get uniqueness but was not depended on for existence. [This refers to Wald's use of the weak axiom of revealed preference]. Reading their remarks in retrospect one would have thought that they understood the special character of the theorem in view of the assumptions, but they may not have read the [Wald] proof closely!" In other words, McKenzie is shocked that Arrow, and presumably Debreu, were not aware that Wald's use of an assumption tantamount to the weak axiom of revealed preference was essential in Wald's proving the existence of equilibrium. It is the case, as McKenzie argues, that this disguised weak axiom creates a world in which there is effectively but one consumer, which makes the problem of existence quite simple. McKenzie was thus claiming that Arrow's

discussion of Wald's paper, in Arrow's letter to me, meant that Arrow was unaware of that issue<sup>2</sup>.

I've always enjoyed thinking that the publication of my paper in the *Journal of Economic Literature* in March 1983 resulted in a financial reward, albeit to someone else. For on 17 October 1983 the Royal Swedish Academy of Sciences awarded the 1983 Prize in Economic Sciences to Gerard Debreu "for having incorporated new analytical methods into economic theory and for his rigorous reformulation of the theory of general equilibrium." The prize statement goes on to note that Debreu's "first fundamental contribution came in the early 1950's in collaboration with Professor Kenneth Arrow. ...Arrow and Debreu designed a mathematical model of a market economy where different producers planned their output of goods and services and thus also their demand for factors of production in such a way that their profit was maximized. ...In this model, Arrow and Debreu managed to prove the existence of equilibrium prices, i.e., they confirmed the internal logical consistency of Smith's and Walras' model of the market economy."

In response to a *New York Times* story written by Harvard's Robert Dorfman on Debreu's Nobel Prize, a piece which failed to mention Lionel McKenzie at all, I wrote to Dorfman about his omission and sent a copy of my letter as well to McKenzie. Dorfman's note to me concluded "Of course, you are quite right about McKenzie. If I had done my research with the same scholarly thoroughness that you exhibited, I should not have overlooked McKenzie's

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<sup>2</sup> For a fuller discussion of the role of Abraham Wald in the story of the existence theorem for economic equilibrium, see (Weintraub 1983).

contribution, something I would never knowingly do. The truth is that, until I read your [Weintraub 1983] article, it never occurred to me that McKenzie's ingenious fixed-point construction was entirely independent from the Arrow-Debreu construction. I stand corrected. The closest thing I have to an excuse is the fact that I had four days in which to write the article.” (27 June 1984, pp. 1-2)

McKenzie's own response to me was that Dorfman's piece in the *Times* had been very widely read. “One person whom I believe to have a quite accurate picture of the history and the priorities is Ken Arrow. Also he is too generous to sleight anyone. Of course, I don't believe Dorfman intended to sleight me.” The person specifically omitted in that sentence in McKenzie's note is Gerard Debreu. McKenzie, an extremely courteous and responsible individual, was saying that Arrow had been correct and had an accurate picture of the publication-discovery priorities. His failing to mention Debreu in that sentence set in motion some of what I intend to argue in what follows.

It was at this point that I turned my own attention to the literature on the dynamics of a competitive equilibrium in an attempt to reconstruct the history of attempts to establish the stability of a competitive equilibrium. Complicated by my father's, Sidney Weintraub's, death in June 1983, that new project was to occupy me until the publication of my *Stabilizing Dynamics* in 1991. Following his death, and knowing that he had not wanted his papers to go to the University of Pennsylvania, I made arrangements to have his papers deposited in the Duke Rare Book, Manuscript, and Special Collections Library. Following this, I began working with Craufurd Goodwin, Neil De Marchi, and A. W. ‘Bob’ Coats, and with the director of the library,

Robert Byrd, to create an archive of the papers of twentieth century economists, particularly those who worked and made their contributions in the post World War II period. This project was immediately successful, and several important collections came to Duke within the next few years, chief among them at that stage the papers of Karl Menger, the mathematician/mathematical economist from the University of Vienna together with some of his father's papers, and the papers of Oskar Morgenstern which had been called to our attention by Martin Shubik of Yale. Each of those collections formed the basis of a *History of Political Economy* conference, each of which resulted in special supplementary hardback supplementary volume/issue of the *History of Political Economy*.

From the mid 1980s the collection grew with specific attention to the post war generation, and we received the papers of Kenneth Arrow, and a number of others including Don Patinkin, Nicholas Georgescu-Roegen, Evsey Domar, and Lloyd Metzler.

Whetted by my interest in those collections, I began a new "career" writing history of economic science with a fellowship year at the National Humanities Center in 1989-90. My book *Stabilizing Dynamics* (1991) took shape, reflecting an increased interest in constructivist historiography, thus moving away from treating historical materials as case studies on which to test one or another theory from the philosophy of science about how science, in this case economics, progresses. As the *Stabilizing Dynamics* project came to fruition, my attention turned back to the issues of the role of mathematics in economics more generally and I began a long process of treating the development, the co-development, of mathematics and economics over the twentieth century. Thinking at the time that I was really writing about "formalism" in

economics, I visited Berkeley for a week in 1991 to interview Gerard Debreu about the Bourbakist turn in economics that he helped to create in the early 1950s, and which would reach fruition in his own projects with his 1959 *Theory of Value*.

With a hiatus in writing resulting from my accepting an acting deanship for two years I was finally able to return to some of my previous work around 1997, fortunate in having a student, Ted Gayer, who wanted to do a part of his thesis on a history of economics topic. He began looking through the Kenneth Arrow papers and there he was able to locate, and follow the stream of, discussions about the Arrow-Debreu paper itself. Upon learning as well that the associate editor for *Econometrica* dealing with that paper was Nicholas Georgescu-Roegen, Gayer tracked down, in the Georgescu-Roegen archive at Duke, the publication history of the famous Arrow-Debreu paper. Gayer and I (Weintraub and Gayer 2000) were able by the end of 1999 to produce a coherent story, one that deepened the story I had been able to tell in 1983 without then having had access to any of these archival materials, letters, and private papers. What emerged was that the straightforward A induces B induces C, etc. story that I had told in 1983 needed some major readjustments.

The history of the Arrow-Debreu paper became clearer. As noted above, Debreu had given a version of the paper on 27 December 1952, and McKenzie gave his paper the next day. It may have been the case that there were other discussions of the Arrow-Debreu paper, since it was produced both as an Office of Naval Research paper under a grant to Arrow, and as a Cowles Commission discussion paper. It is unlikely that McKenzie gave his paper to any other audience prior to that date since none of his Duke University colleagues would have known what

he was talking about. Thus through the spring of 1953, it must have been the case that the Arrow-Debreu paper was redone, re-polished, and checked a number of times prior to its submission to *Econometrica*. The evidence we have is a letter “dated 15 June 1953, from Robert Strotz, (then managing editor of *Econometrica*) to Nicholas Georgescu-Roegen that dealt with three separate matters. The third paragraph reads “I am enclosing three copies of the manuscript submitted by Arrow and Debreu which falls in your department [as associate editor]. I hope you’ll be good enough to arrange for the refereeing of this paper and to advise me on it. I should mention that rather similar paper was submitted some time earlier by Lionel McKenzie and that it has not yet completed [sic] processing. As a matter of fact it is being read at present by Leo Hurwicz and John Nash. I suppose, therefore, that those two readers should not be burdened further with the Arrow-Debreu paper.”

As Weintraub and Gayer (2001) discuss in their paper, the Georgescu-Roegen correspondence shows that he chose, as referees for the Arrow-Debreu paper, William Baumol of Princeton University and the not very good mathematician Cecil Phipps, of the University of Florida. The Phipps story, a curiosity but one that casts a great deal of light on the connection between mathematics and economics in that immediate post war period, has been told in (Weintraub and Gayer, 2001). There are, however, three surprises that emerge from the Georgescu-Roegen Papers. First, the McKenzie paper was submitted to *Econometrica* well before (months before) the Arrow-Debreu paper. Second, the referees for the McKenzie paper were Leonid Hurwicz and John Nash. And third, in a bizarre aftermath to the publication of the Arrow-Debreu paper, referee Phipps demanded that the editors of *Econometrica*, since they would not reject the Arrow-Debreu paper as he had insisted, publish a letter by him saying that

that paper was all wrong. This brought forward a multi-party exchange of letters initiated by Editor Strotz who attempted to gather a number of responses to the Phipps objection. Among those he asked to comment on the merits of the Phipps objections to Arrow-Debreu were McKenzie and Hukukane Nikaido.

Thus at the time the Arrow-Debreu paper was last treated historically in the open literature, in Weintraub-Gayer (2001) and Weintraub (2002), McKenzie appeared to have dropped from the story. This was to be the situation up until the past year or so, when the finding aids to the Lionel McKenzie papers were finally made available in the Economists Papers Project at Duke University. I had taken two passes at the issue of assaying “appropriate credit” to Lionel McKenzie for his work in establishing the existence of a competitive equilibrium, first in my 1983 *JEL* paper and second in the *JHET* paper with Ted Gayer, a paper that was lightly edited for my 2002 *How Economics Became a Mathematical Science*.

It is time for a third pass, occasioned by the newly available McKenzie and Solow papers at Duke. Telling this story involves not only re-shading and reemphasizing and retelling the story that I had earlier developed, but incorporating some startling and rather disturbing new material related to the Arrow-Debreu and McKenzie papers, and will in fact require my going back to rewrite even those past attempts. It is this narrative that I shall now engage.

## Part II: From Montezuma to Oxford

In the 2009 phone directory for the town of Montezuma, in Macon County, Georgia, there are nineteen McKenzies in a total population, from the 2000 census of 3,999. This is much larger of course than the population was in 1919, when Lionel McKenzie was born there. The 1910 Census population of Montezuma was 1,630 while in 1920 the population was 1,827 and by 1930 Montezuma's population had increased to 2,284. The town, then as now, approximately one third white, two thirds black, was an agricultural town in an agricultural county in an agricultural state.

Aside from a very few short notes (e.g. on web pages, author information, etc.) there is not very much biographical material on Lionel McKenzie<sup>3</sup>, and he himself attempted only one autobiographical paper, published in an obscure journal. That narrative appeared in Keio University's *Keio Economic Studies* in 1999, McKenzie's lecture there in June 1998 on the occasion of his honorary degree. There are two other bits of autobiographical writing of which I am aware, letters to two individuals, carbon copies of which he retained for his files, written in 1989 and 1990. The first of these letters is to the distinguished lawyer and civil rights activist Morris Abram<sup>4</sup> (July 28, 1989) sent on the basis of someone having sent McKenzie a copy of the

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<sup>3</sup> The most interesting of these short pieces was done by Tapan Mitra and Kazuo Nishimura as the first essay in the volume they edited titled *Equilibrium, Trade, and Growth: selected papers by Lionel W. McKenzie*. (Cambridge; MIT Press, 2009)

<sup>4</sup> Morris Abram was an eminent civil rights activist and lawyer, and President of Brandeis University, who was born in the small town of Fitzgerald, Georgia. Among his many high level

then old *Atlanta Constitution* article editorial noting Abram's and McKenzie's appointment as Rhodes Scholars from Georgia in 1939.

In this letter to Abram, which notes his having read "with intense interest" Abram's own then recent autobiography *The Day Is Short*, he describes some of his own memories of place and family in Montezuma in the early days. He recalls that

"[T]here were some Jewish families in Montezuma, from early times. The earliest of those I knew something of were the Happs. I had a picture of my father's baseball team at the turn of the century and the business manger, the only one in Mufti, was a Happ. Daddy was a pitcher. His crowning achievement was pitching two games in one day, winning both I believe. He left some notes on his life which are in a style of Mark Twain, I would say, not a copy, however, since he was not a reader. My Mother was a college student (one year at Wesleyan – her father thought too much education was not good for women) and former schoolteacher. That no doubt explains her buying me the *Book of Knowledge* and *Harvard Classics* from door to door salesman and

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positions he was chief counsel of the Peace Corps and a partner in the New York law firm of Paul, Weiss, Rifkind, Wharton, & Garrison. His law degree was from the University of Chicago following his undergraduate career at the University of Georgia with the highest grade point average in the school's history at that time.

starting me on my way. You may be interested to know that Mrs. Happ attended our Baptist church, not very orthodox I fear. My crowd also included a Jewish boy, Phil Brinen, who seemed to fit in very well but his family saved him by moving to Atlanta after high school. We lost track of him. I remember he once told me his father was fond of reading Tolstoy. That impressed me since Tolstoy was my favorite author at the time. We had a marvelous set of his works in the Carnegie Library beautifully printed. There was another family, the Hirschberg's, whom I knew in Oglethorpe (two miles away) because I collected for magazines from them. There was a son in that family who at this distance reminds me of you. Then there was Max Cohen or Bernd-Cohen, as he amended his name to make it more distinctive, who was a painter. I can recall my mother refusing to believe that Mary Mullino had allowed him to paint her in the nude, but I think she did. His mother also lived in Montezuma, boarding with a cousin of mine, Cleone McKenzie. She used to walk out the highway to Macon as far as the big oak many afternoons, a distance of five miles, to the amazement of the towns folk. Max Cohen's brother had a house on this highway overlooking the Flint River Valley, facing the west so that the sunsets were marvelous. He had a German woman as housekeeper, and it was assumed something more.

Max got me an offer to attend the University of Pennsylvania, after Middle Georgia College, which would be paid for by John Lewis, a Philadelphia lawyer, while I boarded with Max who then lived outside of Philadelphia on the main line. Daddy and I were all for it but Mama objected. I think on religious grounds. That ended up in the expected way since I married a Jewish girl. It also lost me to physics and I'll bet a Nobel Prize because physics was my real love and I would probably have done well with it. I still go back to Montezuma at great intervals since I have many friends and some distant relatives there. The McKenzie's, not my immediate branch, were the most notable family in Montezuma in my youth. It is much changed now, as you can imagine, with an integrated school system and new industries."

The other personal/autobiographical letter was written on March 11, 1990, to a Mr. Langdon York, a star student at Macon County high school in Montezuma, the high school from which he himself had graduated. McKenzie wrote to him after he saw a piece in the Montezuma newspaper:

"I was stuck by your plan to attend Middle George College for two years before going to [Georgia] Tech because I attended Middle Georgia for two years long ago before going to Duke. I also noticed that you are interested in science and engineering which

parallels my interest at your age. I had an appointment to West Point from Walter George<sup>5</sup> but I allegedly failed the physical exam. In my opinion, I was rejected on ideological grounds. I answered a couple of questions of a political nature in a way that my interviewers did not like. Anyway, I later regarded that as a close call. After being at MGC for two years I was interested in a number of subjects including economics and physics. Possibly because I had read *Wealth of Nations* by Adam Smith from *The Harvard Classics* I chose economics when I entered Duke. I have often doubted that I made a wise decision. The upshot was that I tried to do economics as though it were physics which may not have been a good strategy.”<sup>6</sup>

McKenzie’s record at Duke University was exemplary. Winning honors and election to Phi Beta Kappa as a junior he took economics, French, comparative government, economic thought, ethics and social economics, receiving grades of A in every course. In his senior year he

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<sup>5</sup> Walter F. George was the U.S. Senator from Georgia from 1922-1957. See his official Senate biography at <http://bioguide.congress.gov/scripts/biodisplay.pl?index=g000131>.

<sup>6</sup> The letter goes on to talk about McKenzie’s connection with Richard Feynman, and he encloses a book by Feynman as a gift with his letter to that high school student.

continued with economics and political science doing these in seminars and writing a thesis in his senior year, again receiving A's in every course<sup>7</sup>.

In the Kaio piece, which McKenzie titled "A Scholar's Progress", he noted again that

"I think the book by Adam Smith...played a larger role in turning me to economics. It was possible also probably important that we were suffering from the Great Economic Depression then and my social studies course based on ideas borrowed from a survey course at the University of Chicago emphasized the economic problems of my region and state. In any case the upshot was that when I transferred to Duke University after graduating from junior college I chose to enter an honors course in philosophy, politics, and economics modeled after the course of the same name at Oxford University<sup>8</sup>. I concentrated in economics. I should say that the strongest competition for economics in my life plans was

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<sup>7</sup> These grades suggest an astonishingly competent young scholar since 'A's then were not what 'A's have become today. For example, as late as 1955, the Department of Economics at Duke was requiring its faculty members to award no more than 11 percent A's to seniors and to fail one percent, while only 10% of the freshman economics students could receive A's and 10% had to receive Fs.

<sup>8</sup> When Duke University created a PPE Certificate Program for its undergraduates around 2004, this was done with no awareness that the undergraduate college had ever had such a program.

physics to which I had been attracted by popular books on science by Eddington and Jeans<sup>9</sup>. I would be less than candid not to admit that I have often wondered whether my choice was a mistake. In my final year at Duke I won a Rhodes Scholarship from Georgia and I intended to pursue the same course as at Duke, popularly called PPE at Oxford. However this was not to be since the Second World War intervened.” (p. 1)

McKenzie’s Kaio piece continues:

“after my study at Oxford was postponed indefinitely by the war, I applied to Princeton for support in their graduate program and entered the Princeton Graduate College in the fall of 1939 to work toward a Ph.D. in economics<sup>10</sup>. I knew very little about either economics or the character of the Princeton department when I

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<sup>9</sup> Sir Arthur Eddington and Sir James Jeans were the founders, in the interwar years, of the new field of cosmology. Eddington wrote the immensely successful *The Expanding Universe* (1929) while Jeans sold many copies of his *The Mysterious Universe* (1930)

<sup>10</sup> Frank de Vyver, at Duke, had recently arrived from Princeton to teach labor economics. It is certain that de Vyver had encouraged McKenzie.

entered there<sup>11</sup>. However, I found a number of quite stimulating professors in the program. I would particular mention Frank Graham and Oskar Morgenstern....Morgenstern taught me advanced economic theory where we read and criticized the new book *Value and Capital* by John Hicks. ...I should mention that Morgenstern was not uncritical of Hicks's book. In particular he ridiculed Hicks's use of the equality of the number of equations and number of variables to conclude that an economic equilibrium existed<sup>12</sup>. Morgenstern had known Abraham Wald and John von Neumann in Vienna, and he was familiar with the papers on existence of equilibrium by Wald and von Neumann which were read to the colloquium by Karl Menger. However he did not give us references to these papers in the seminar...I also benefited from a course in mathematical economics led by our resident statistician, Atchison Duncan. ...Duncan also encouraged me to read Wilson's *Advanced Calculus* which I did one summer despite the complete lack of preparation in calculus. I remained one further year at Princeton after completing my coursework and began a project

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<sup>11</sup> Princeton held a special place in the hearts of Southerners compared to Harvard and Yale, which were seen as New England schools. Virginian Woodrow Wilson's Princeton presidency made this connection visible.

<sup>12</sup> Indeed, Morgenstern wrote a scathing review of *Value and Capital* for the *Journal of Political Economy* at that time

with Morgenstern on futures markets, which was never completed.”

McKenzie’s Princeton transcript shows that he is a single white male who entered the Graduate School in September 1939; he passed his general examination in spring of 1941 having earlier passed both French and German language requirements. In his two years of taking coursework, 1939-40 and 1940-41, he took classes in statistics and basic economics, including history of economic doctrines, but took additional courses from Frank Graham, Frank Kemmerer, Morgenstern, Duncan, and Lutz, and his 1941-42 doctoral research work was being supervised by Morgenstern.

The Keio paper continues:

I left Princeton now that the United States was at war and spent about a year in Washington as a junior economist with the Office of Civilian Supply in the War Production Board... There I was regarded as a theorist but I don’t believe I made any significant contribution to either theory or the war effort...In 1943 I entered the Navy in a non combatant role as a cable censor in Panama and New York.....When I was in Panama ... I had done what amounted to an undergraduate course of reading in mathematics and physics, as well as a fair amount of reading in both modern and ancient philosophy... I was mustered out at the end of 1945 in

response to a plea that I needed to undertake my Rhodes Scholarship. My wife, Blanche Veron and I were married in Washington just before my entry into the Navy. ...Upon leaving the Navy I obtained an appointment in 1946 as an instructor at MIT, thanks to the recommendation of my former Princeton colleague Ansley Coale...this was after Paul Samuelson had become the star in residence publishing his *Foundations* and preparing his *Principles* text. ...I attended Samuelson's graduate class on economic theory but I remember little about the content. I do recall asking Samuelson in his office to explain his theory of revealed preference to me but I did not recall that he succeeded. My teaching at MIT was in industrial organization, where my expertise could be questioned, rather than in theory. After two semesters there I resigned to take up my deferred scholarship at Oxford. I was tempted to dip into physics at Oxford but, partly because I was also supported by a grant from the Social Science Research Council, I decided against such a course. My Oxford supervisor was John Hicks, and I entered in the D. Phil. program there. I spent very much, perhaps most of my time at Oxford, paying attention to subjects other than economics. In particular I attended several lectures series in philosophy with Gilbert Ryle and also Friedrich Waismann who had been a member of the Vienna Circle of philosophy in earlier years. ...Of course, I

attended Hicks's class which involved reading and discussing articles in the journal literature, especially recent ones. The project I was attempting with Hicks was an examination of the modern welfare economics to which Hicks had made contributions. Ian Little who had attended Hicks's class, along with Paul Streeten, was also writing on the subjects and was my closest associate at Oxford. As it happened the leading economic scholar at the LSE, William Baumol, and the leading scholar at Cambridge, Jan de Van Graaff, were writing on the same subject. Indeed we three gave the three presentations to the joint Oxford Cambridge LSE seminar in the academic year 1947-48."

McKenzie here passed lightly over what had been a crushing blow at the time. After all, he was a "mature student", a (delayed) Rhodes Scholar<sup>13</sup>, hoping to use his D. Phil. in lieu of a Ph.D to get a serious job in the United States. He had already been at Princeton, and had taught briefly at MIT, thus he had a taste of the first rank institutions he might be able to join with an advanced degree.

With respect to his Oxford thesis, and the disposition of it, McKenzie received a letter from John Hicks on the second of November, 1948:

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<sup>13</sup> See the recent discussion of that time at Oxford by another "Southern" Rhodes Scholar, Duke's Reynolds Price, in his memoir *Ardent Spirits: Leaving Home, Coming Back* (Price 2009).

“Dear McKenzie, You’ll probably have heard by now that your examiners did not make the recommendation we might have desired, but instead recommended that you should have the choice of a) resubmitting the thesis, or b), accepting a B. Litt. This is disappointing, but I hope you’ll realize that the option of resubmitting is rarely given, and clearly means that the examiners did recognize the quality of your work. I’m sure of what the trouble was; as I always suspected myself, you did not have time to get that intricate stuff in the middle really lucid, and until that has been done the thing has an unfinished air. ...My own feeling is that your best plan is to set to work to prepare your thesis for publication. If, a year from now, it seems to be coming into shape (considered from that point of view) it would be very good if you could put it in again, though of course that would mean coming over again to be re-examined and I appreciate that this would be a heavy burden upon you. If the thing does not seem to be coming straight in time, or if it is hopeless for you to be able to come over again, then I should take the B. Litt. I feel myself very strongly that your work has the real quality; but, as so often happens with

Oxford doctorates, the two years is just not sufficient time to get the thing to the degree of finish which is required.”<sup>14</sup>

McKenzie’s Keio piece continues

“...Upon leaving Oxford I returned to my undergraduate university Duke as an assistant professor. This is a move much more typical of Japan than of the United States. However, in order to take it, I rejected an offer from Princeton of an instructorship, which had been arranged, I believe, by Frederick Lutz. I did this partly because I expected that Princeton would want me to pursue the Oxford thesis, which I did not wish to do. I think the event has proved my decision to have been correct.” (pp. 2-3)

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<sup>14</sup> Presumably after McKenzie wrote to the examination board about the requirement that he reappear for the viva (oral re-examination) and the near impossibility of his doing so, he received a letter from the University Registry: “Dear McKenzie, I have consulted the social studies board about your difficulty in coming back for viva should you desire to have another try at the D. Phil. I’m afraid that the board was unwilling to ask for a decree exempting you from the viva voce examination on a second try. I am very sorry about this but I’m afraid it cannot be helped. This being so I now assume that you wish your original decision to stand and that you will take the B. Litt. I have told the college. I hope you are enjoying your job at Duke University and that you will not let this setback here unduly upset you, though I suppose it means that you’ll unfortunately have to begin to work for a Ph.D. at home.”

### **Part III: From Duke to Chicago to Duke**

Retuning to one's undergraduate college to teach, without a Ph.D., was not the best way to advance a scientific career in economic theory. Duke in the 1940s was, of course, a segregated southern private university. In its re-founding as a university in the 1920s, building on the existing Trinity College, it grew along the lines determined by James B. Duke, founder of Duke Power Company, and son of Washington Duke, founder of the American Tobacco Company (and benefactor of Trinity College). Trinity was to be the men's undergraduate college, to be paired with the Woman's College with which it would share the faculty and courses and curriculum but not the living space with Trinity. There would be a graduate school, and there would be new schools of law and medicine. Further, schools of engineering, nursing, and divinity would be added to the mix. Undergraduate students were almost all from the South. Over time it would remain the case that the brightest (and most affluent) male students from the region would go north the Ivy League, principally Princeton, while the brightest women students stayed in the region, and went to places like Duke, Tulane, Emory, and Vanderbilt. It was a football and fraternity/sorority culture, leavened by the usual dedicated teachers and scholars.

In the department of economics, McKenzie would find two men (there were no women in the department) of particular distinction, Joseph J. Spengler and Calvin B. Hoover, each of whom would eventually become presidents of the American Economic Association. Spengler was the most important figure in that generation of economic demographers, while Hoover created the field of comparative economics. McKenzie had only graduated eight years earlier

from Duke when he took up his appointment as an assistant professor, and consequently had to work hard to reconstruct the relationships with his former teachers, now his colleagues.<sup>15</sup>

His main scholarly task in those first years was to develop the Oxford work into some publishable articles, to begin to secure a foothold on a scholars' ladder. He reworked much of the thesis and was able to publish a paper in the *Economic Journal* on "Ideal Output and the Theory of the Firm". This piece, a non-mathematical one, established McKenzie as a bona fide economist, even though the article eschewed the kinds of mathematical theory becoming more widely employed in those immediate post war years.

How, then was McKenzie to develop his talents, and nurture his not inconsiderable ambition? It was time to go northwest. In the Keio paper, McKenzie wrote:

"[A]t Duke...I noticed a report from a meeting of the Econometric Society of a paper by Tjalling Koopmans describing his activity analysis. His work struck me as just the kind of theory I could

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<sup>15</sup> To give a sense of how McKenzie viewed his relationship with his colleagues, he saved the following statement, a carbon copy of a note which he passed out to all members of the economics department present at a faculty meeting in May of 1952 "Gentleman: I am not entirely satisfied with my remarks in this meeting. I should like to add the following assertion: 'As long as we have the textbook, classroom, compulsory attendance, five courses per semester system of instruction, we are going to have bored, disinterested students. And, by jove, I don't blame them. This will be true for all departments and all the years.' L.W. McKenzie"

have used in my paper on ideal output. This led me to apply to Jacob Marschak to visit the Cowles Commission in Chicago. To make this visit possible I received support from the Carnegie Foundation which devoted some resources to promote advanced education in the American South and also a fellowship from the Department of Economics in Chicago.”<sup>16</sup>

Additional evidence associated with McKenzie’s move to Chicago is contained in a (handwritten) letter he sent to Ian Little, one of his close friends from his Oxford years:

“Two very pleasant and exciting experiences this year have been my perusals of your book and Ryle’s book on the concept of mind. He [Ryle] might be pleased to know his book has penetrated so far....My [Oxford] thesis was written from much the same viewpoint as your book, and its function was much the same also.

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<sup>16</sup> On August 29, 1950, McKenzie received a letter from the Rockefeller Foundation’s General Education Board advising him that “the General Education Board has awarded you a fellowship in the field of economics for study at the University of Chicago. This fellowship provides for a stipend of \$60 a month for not more than 12 months beginning approx September 15, 1950 and for tuition if needed in excess of your GI allowance.” Amusingly, attached to the Rockefeller letter was the receipt from the University of Chicago bookstore for the purchase of the books by Birkhoff and Mac Lane (*Modern Algebra*), Feller, Volume 1 (*Probability Theory*) and Courant Volumes 1 and 2 (*Differential and Integral Calculus*) for which he paid a total of \$24.50.

It was an attempt to get at the meaning of welfare theory. My procedure, after criticizing the utilitarians, was to develop all the assumptions which the new type of welfare economics requires if it is to make sense. This meant that I seemed to be developing a theory myself rather than criticizing the theories of others, and at the same time the necessary assumptions were so fanciful that the theory seemed of doubtful value... I abandoned the Ph.D. at Princeton, after Graham's death<sup>17</sup> ..., encountering Morgenstern's hostility, and getting saddled with Viner as a supervisor. Whew! My position here [at Duke] is not thereby jeopardized [by not having the Ph.D] it seems. The purpose of the trip to Chicago is to get some fresh air, intellectually speaking, learn a bit of statistics, and perhaps make some kind of new start. I don't know." (Box 41 McKenzie papers, written in Spring or Summer 1950)

The Keio narrative continues

“ So after two years teaching at Duke, in the fall of 1950, I worked at the University of Chicago as a graduate student in economics. The time I spent at the Cowles Commission was decisive in setting

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<sup>17</sup> On September 24, 1949, Graham fell to his death from the stands “at Palmer Stadium at the close of the Princeton-Lafayette football game.” *AER, Papers and Proceedings*, 40, 2, (May 1950, 585-587.)

the character of my research career<sup>18</sup>. At least half of my time, which comprised four quarters or twelve months was devoted to mathematics. I had three marvelous young teachers there, not very different in age from myself. These were Irving Kaplansky in algebra, Paul Halmos in measure theory, and Saunders Mac Lane in topology<sup>19</sup>. I also benefited from a course in mathematical statistics with Jimmy Savage. Savage was writing his book on personal probability and I was able, along with Jacob Marschak, to call his attention to the work of Frank Ramsey which I'd encountered in England. Incidentally, I was indirectly responsible for calling this work to the attention of von Neumann and

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<sup>18</sup> This “counts” as an “autobiographically consequential memory”, and corresponds exactly to the theory of such as discussed in (Weintraub 2005).

<sup>19</sup> Irving Kaplansky, Mac Lane's first Ph.D student, was to become a most distinguished mathematician, eventually chairing the Chicago department and Berkeley's Mathematical Research Institute. Paul Halmos was the author of *Measure Theory*, *Naïve Set Theory*, and *Finite Dimensional Vector Spaces*, each of which became the canonical textbook concerning its subject matter, and defined a standard for mathematical exposition for decades. Saunders Mac Lane co-authored the then most important American textbook, *Modern Algebra*, and was to co-create the field of category theory with Samuel Eilenberg. His major work in algebraic topology gave birth to homological algebra and co-homology theory. Chicago was the center of the mathematical world in the immediate postwar years, and McKenzie was immensely fortunate to be there studying mathematics at that time.

Morgenstern by way of my friend Ansley Coale. Ansley reported that von Neumann expressed no surprise that the work existed but wondered why he had not succeeded in finding it. Morgenstern on the other hand questioned whether Ramsey had used an axiom system. Of course, he had. ...In economics I attended the classes of Koopmans on activity analysis and econometrics and of Jacob Marschak on decision making under uncertainty. My companions in these courses included John Chipman, Martin Beckman, and Edmund Malinvaud. Gerard Debreu, Karl Brunner, and Harry Markowitz were also in the Cowles Commission group, as well as Leo Hurwicz for awhile. I attended no classes given by a regular member of the economics department.” (p. 4)

For the purposes of this examination of the 1954 papers, it suffices to leave MacKenzie’s public discussion of his contributions to the model of interrelated markets and the existence question with his presentation of his recollection in the Keio paper. The short discussion in its specifics reiterates what he had written to me in the early 1980s as I was writing my 1983 paper. It probably serves well as McKenzie’s discreet version of what happened at that time. That is, in the Kaio paper he recalls:

“a piece of research I completed at Chicago was done in Koopmans’s class on activity analysis and was based on Graham’s model of international trade that I remembered from Princeton. It

was a multi sector analysis of comparative advantage which showed that bilateral comparison of comparative advantage was not sufficient to discover an efficient allocation of world production. This led to my article in the *Review of Economic Studies* (1954) entitled “Specialization and Efficiency in World Production”. Koopmans was pleased with this paper and suggested that I stay longer in Chicago, but I felt I should return to Duke<sup>20</sup>, a decision somewhat like my decision not to pursue the

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<sup>20</sup> It is clear the McKenzie at least explored the possibility of remaining at Chicago for a new Ph.D, for there is a letter to McKenzie from Frank de Vyver , acting economics chair at Duke, dated January 12, 1951: “I hardly know what to say about your letter of January 4 because we want you to do what you want to do, and I want you to understand that you are a member of our department and we are looking forward to having you back. On the other hand, if you felt that it would be wise for you to continue another year at Chicago and if you find it possible to finance the program, we would be able to make arrangements for you to be off on leave another year... I do not share your feeling that you are not unanimously cherished at Duke. Of course, you know the pressure on production that is around us all the time, but I think you are interested in creative writing and I am sure that there will be no objection from anyone here. Rest assured, however, that if you ask for another year’s leave that you will in no way prejudice your position at Duke. At the present time I am acting chairman of the department, so that makes this official.” Furthermore, [Chairman Calvin Bryce] Hoover always backs me up if I make decisions while he is away.

Oxford thesis or the earlier decision not to do physics at Oxford<sup>21</sup>.

I had become quite interested in mathematics and if I had stayed in Chicago I might well have transferred to the math department.”

In the Keio paper, McKenzie continues his story:

“On my return to Duke I did not immediately do as Koopmans had suggested to me and consider the factor price equalization theorem of Samuelson in the context of an activities model. The remarks Morgenstern made on the existence problem at Princeton and those made by Koopmans at Chicago had interested me in that question.

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<sup>21</sup> To get some sense of McKenzie’s belief that he was not much appreciated at Duke, there is a handwritten footnote by Robert Strotz in a January 26, 1954 letter to McKenzie which reads “To turn to a personal note, you wrote something on a recent letter concerning criticism of your teaching that you think I may have heard. I won’t entirely deny having heard something of that general sort, but I will deny paying any attention to it. What I heard from Bill Allen [at Duke] really wasn’t criticism, but I have noted that he is often overly eager to jump on the next guy. (He has a terribly competitive spirit.) May I mention in any event that I recently proposed your name as someone we should try getting to join our department (this is a vote of confidence) and the only objection I encountered was ‘What the hell courses would we give him to teach that he would be interested in?’ I confess I was at a loss for a very satisfactory answer. For the record, in case it comes up again, what is your rank at Duke, and where do you stand with your thesis? Are you at all in the market? Bob”

I knew that Graham had given his model of trade to von Neumann to ask for a way of solving for the equilibrium and von Neumann had replied that no analytic solution was possible. I found the Wald and von Neumann papers from the Karl Menger Seminar in the Duke Math Library<sup>22</sup> and read them with my rather weak German. At this time, I wrote my paper “On Equilibrium and Graham’s Model of World Trade and Other Competitive Systems”, published in *Econometrica* in 1954, but delivered to the Chicago meetings of the Econometric Society in December 1952. This was the same meeting to which the Arrow-Debreu paper on existence was reported. Debreu was present at my presentation and made an intervention to suggest that my paper was implied by theirs, which had been delivered earlier in the meeting. Though I had not heard it, I responded that my paper no doubt implied theirs. Literally both statements were false. Their paper used consumer utility functions and Debreu’s theorem on the existence of a social equilibrium, which depended on the fixed point theorem of Eilenberg and Montgomery, while I used demand functions and the more elementary Kakutani fixed point theorem. I had learned

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<sup>22</sup> When I did my 1983 paper, I too went to the Duke Math Library to find the Menger *Ergebnisse* papers of Wald and von Neumann. At that time in the 1980s the math library still was changing over to computerized record-keeping, so when I was given the volume, it had a card in the back with previous borrower’s names, the only other borrower being McKenzie!

about the Kakutani theorem from a working paper by Morton Slater, the resident mathematician at the Cowles Commission.” (p. 5)

#### **Part IV: The Existence of Equilibrium Papers**

In his Presidential address to the Econometric Society (given in Ottawa and Vienna in 1977), a revision of which was published in 1981, McKenzie says he wishes to

“discuss the present status of a classical theory on existence of competitive equilibrium that was proved in various guises in the 1950s by Arrow and Debreu, Debreu, Gale, Kuhn, McKenzie, and Nikaido. The earliest papers were those of Arrow and Debreu, and McKenzie, both of which were presented to the Econometric Society at its Chicago meeting in December, 1952. They were written independently. The paper of Nikaido was also written independently of the other papers but delayed in publication.” (p. 819)

Besides the Keio narrative, this is the only other general bit of historical reconstruction that McKenzie appears to have published concerning the 1954 existence of equilibrium paper. There is much of the story, however, that was hidden from his view. Specifically, the story of how the McKenzie paper, and the Arrow-Debreu paper, actually came to public attention was left to me, as an historian of that period in economics, to construct and disseminate from the various

authors' personal recollections (Weintraub 1983, 1985). As noted earlier, that narrative was limited in its evidentiary base by the few available archival documents bearing upon that period. In more recent years, matters began to change first with the availability of the Arrow papers, and those of Nicholas Georgescu-Roegen, the associate editor of *Econometrica* in that period, responsible for shepherding the McKenzie and Arrow-Debreu papers through the refereeing and publication process. Those materials, available in the 1990s, allowed Weintraub and Gayer (2001) and Weintraub (2002), to reconstruct the heady period of 1953-54 when those papers were moving existence of general equilibrium concerns into a more public phase, one which would culminate in Nobel Prizes to Arrow and to Debreu.

Over the past decade however several additional sets of papers have found their way to the archives. The new availability of the McKenzie and Solow and Hurwicz papers have allowed a reexamination of those issues previously "settled", and have shown how both of the earlier historical reconstructions misunderstood some important issues concerning both "priority" and "credit".

In his most recent recollection unguarded by publication constraints, and prompted by my question of his connection with Debreu in Chicago, McKenzie wrote on 8 September, 2009

"Actually I did visit the Cowles Commission in Chicago when Debreu was there and working on existence, but he kept this fact secret from me. I asked him what he was working on and he refused to say. The first thing I knew about his work was when we

both presented papers on existence to the Chicago meeting of the Econometric Society in 1952. I did not hear his paper but he heard mine and alleged there that my result was implied by his. I entered the possibility that his was also implied by mine. Both allegations were wrong, since he assumed that demand functions were derived from continuous preferences and I assumed that they were continuous and at sufficiently low prices would exceed the production limits. I cited their paper in mine but they did not pay me the same courtesy. Arrow told me that this was because Debreu did not tell him about my paper. Also they assumed free disposal without acknowledgement. In my final section I described a general linear model similar to my later work.”

McKenzie must have submitted the existence paper to *Econometrica* early in 1953 (following the presentation at the 1952 December meetings). We do have, perhaps as a result of his submission, the letter to him on March 25, 1953 from managing editor Strotz asking him to referee someone else’s paper:

“Some time ago, a professor H. Nikaido submitted a manuscript to *Econometrica* which was returned to him subsequently for revision. Professor Georgescu-Roegen saw the manuscript during the period of its processing and was stimulated by it to write a follow up note extending Nikaido’s work. Georgescu-Roegen then

saw the revision of the Nikaido manuscript and has recently submitted his own note... I would be interested in your reaction to the Nikaido papers which has not yet been done, [though] my main interest isn't getting your opinion on the quality of the paper by Georgescu-Roegen."<sup>23</sup>

In this period of immense intellectual activity by McKenzie, he begins preparing another paper to present at the Kingston, Ontario Summer meetings of the Econometric Society in September 1953. Robert Solow appears to have been the chair of the program committee, and on 9 April McKenzie writes to him

"I have recently extended Wald's theorem to general linear models assuming only continuous demand functions and Kakutani's theorem. I think this has been most significant to

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<sup>23</sup> It was a curious way to run a journal. Nikaido (whom no one in the U.S. apparently knew or knew of at that time) had submitted a paper. It landed in the hands of Georgescu-Roegen, the *Econometrica* Associate Editor for "Theory". After handling the paper as an Associate Editor, passing it on to referees, receiving their reports, and asking Nikaido to revise the paper accordingly, Georgescu-Roegen wrote his own paper responding to, and extending, Nikaido's work. This led the Managing Editor, Strotz, to ask McKenzie to referee both the original and the revised Nikaido paper but to ignore the Georgescu-Roegen paper. Should one not be uncomfortable having an editor, Georgescu-Roegen, using paper submissions as raw materials for new work to publish himself? "Conflict of interest" seems to fit the circumstances.

extensions since a long period of competitive equilibrium virtually requires linearity. My proof can be neatly illustrated with a two dimensional diagram. It is the general form of the paper I presented on Graham's model last December."

On that developing paper, McKenzie's next message to Strotz on 25 April 1953, mentions in passing that "I've definitely carried Wald's theorem to its ultimate conclusion by including the case of external economies. I hope to have a MS soon." This was to be the paper McKenzie presented at Kingston that September.

On May 1<sup>st</sup>, Solow responds to McKenzie on the subject of the paper McKenzie will give at the Kingston meeting:

"I confess to being slightly awe-struck at your rate of production of new theorems, but I am the slow and slothful type myself. In any case, the extensions of Wald's theorem that you describe strikes me as being intensely interesting. By all means get them on record at Kingston. In fact, if you get them written up before then, I would very much appreciate giving a look at your results. I would have thought that the existence of external economies would enormously complicate the problem. Perhaps it is the assumption of inelastic supply of capital goods that saved the day. If I understand you correctly, the very possibility of saturation with

capital might serve the same purpose. This sounds like a great step forward. You apparently know of Debreu's results along this line. Ken Arrow has also been working on this problem, and the two of them are publishing a joint paper to appear in *Econometrica* some time soon. Perhaps the best thing for you to do is simultaneously to plan on reporting your results at Kingston, and on submitting a paper to *Econometrica*."

Solow thus appears to be unaware that McKenzie had not only given a paper at the previous December meeting that did the same work as the Arrow and Debreu paper, but also Solow appeared not to realize that McKenzie had already submitted that paper to *Econometrica*, months before the Arrow-Debreu submission in the first week of June 1953 (Weintraub and Gayer 2001; Weintraub 2002, 192 et seq.). *It is important to be clear on this matter. Both the Arrow-Debreu and the McKenzie papers established the existence of a competitive equilibrium for suitable general equilibrium models, both papers drew on the Wald tradition, and both papers employed fixed point theorem arguments. Both papers were presented to a public audience at the Chicago Econometric Society Meeting. From that meeting, the McKenzie paper's abstract was published while that of the Arrow-Debreu paper was not, and the McKenzie paper was submitted first, by several months, to Econometrica.*

As the date for the Kingston meeting approached, McKenzie must have expressed concern about his own *Econometrica* manuscript. Strotz replied on June 9, 1953, "I am writing to tell you that I have prodded the referees, who seem to be particularly pokey. I do not want to

give you any encouragement regarding the speed with which this processing can be brought to a conclusion, but I do hope that reports might start coming in quite soon.” Soon thereafter though, Strotz is forced into writing the embarrassed note of June 23, 1953:

“Dear Mac: I thought I better write to you to explain that despite our recent promptings of the referees of your manuscript, we have to this date heard nothing from them. This is very bad luck. Your paper is in the hands of two different people and neither have [sic] so far sent me any word about it. Knowing how busy people are with the conclusion of the academic year, it is not surprising that nothing happened during the month of May or the first part of June; but one would hope that they could busy themselves with it during the past two or three weeks. I wonder what to do in this case: whether to write to them and recall the manuscript or simply to prompt them again and keep hoping to hear. If the paper is to be recalled, this means that its’ processing must be started once again and in the past several months of waiting will be a complete loss. On the other hand, one hates to throw good time after bad. Since you are the one who has the personal interest in the matter, I thought I would write to you to ask your advice. My own recommendation is, I believe, that another prompting letter and a further wait would be in order for I should certainly hope that this would spur them to some immediate action. I am planning to get

to Kingston this year; I'll look forward to seeing you there. It occurs to me that I ought to reassure you in connection with your manuscript that I shall not publish any similar papers submitted after yours was submitted before publishing yours, provided, of course, that your paper is found to be acceptable."

Strotz's hopes were forlorn. He wrote McKenzie on August 6: "I have given up. Letters have gone to both referees requesting the return of your manuscript to this office right away. I hope to God I can have better luck with the next people. I don't know whether this is a matter of concern to you, but let me assure you that it is my intention not to publish the paper by Arrow and Debreu (which has also been submitted) before the publication of your paper (if both are found acceptable). I think this would only be fair to you."<sup>24</sup>

There is one last bit in this sequence of what must have appeared to McKenzie to be calamitous: Strotz writes to McKenzie on August 17 1953 that "as might be expected, a recent demand that your manuscript be returned by the two tardy referees has brought a brief comment from one of them, with the promise of more detailed comments within a few days. I have already arranged for someone else to take over the refereeing of your paper from the other laggard and hope that we can make some speed from here on in."

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<sup>24</sup> Strotz was to be scrupulous about this promise. The McKenzie paper appeared in the April 1954 (vol.22, no. 2) issue, while the Arrow-Debreu paper appeared in the July 1954 (vol. 22, no. 3) issue.

McKenzie's desire to get something in print, on the record, quickly to establish his claims for joint priority on the existence question appeared then to have depended upon the publication of an *Econometrica* abstract of both his Chicago paper and his Kingston paper. In this he was only half successful. In the July 1953 issue (vol. 21, no.3, pp.463-490) in the "Report of the Chicago Meeting, December 27-29, 1952", it was recorded that in the session on Saturday afternoon, December 27, in a session on the Theory of Games chaired by Harold Hotelling, the "Abstract of paper by Arrow and Debreu and of discussion by Savage not available" (p. 473). Thus the Arrow-Debreu paper had neither been published nor abstracted in the open literature by July 1953. That same "Report" however noted that in the Selected Papers session of Monday afternoon, December 29, 1953<sup>25</sup>, chaired by Martin Bronfenbrenner, there is a four paragraph abstract of McKenzie's paper titled "The Existence and Uniqueness of Equilibrium in Graham's Model of International Trade." The last paragraph of that abstract reads: *"From the generality of the proof it is clear that the special nature of Graham's model is irrelevant. The proof actually constitutes a substantial generalization of the results achieved by A. Wald for Cassel's [general equilibrium] model. Also, the proof does not have the rather intricate nature of that used by Wald. The basic source of the added generality and simplicity is the exploitation of the convexity of the set of outputs and the use of a fixed point theorem."* (p.484, emphasis added)

McKenzie's hopes for a double mention, before the Arrow-Debreu paper would appear, rested on a second abstract in *Econometrica* from the Kingston Summer Meeting. Those hopes though were dashed as he received a letter from Strotz on September 29, 1953 which told him that "As

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<sup>25</sup> This is two days after the Arrow-Debreu paper presentation by Debreu, not the next day as McKenzie had remembered.

for your abstract, you will be grieved to know that present plans, which are, however, still tentative, are to cease publishing abstracts of papers given at meetings. A main reason for this is to economize on space in the journal. I believe this will mean that the abstracts of papers given at the Kingston meetings will not appear, although this is not yet quite definite. It was good to see you again [at Kingston] and I look forward to the next time.”

We need to be clear here about the chronology. McKenzie was concerned about establishing a claim of priority for his work. He saw his claim for priority to be the equal of Arrow-Debreu's in the specific sense that they each presented their work on the existence of equilibrium problem at the December 1952 Econometric Society Meeting. He saw his special claim to be weaker in that he appeared to be modeling not a general competitive equilibrium system but a specific world trade. But his claim was stronger in its use of the Kakutani fixed point theorem which immensely simplified the structure of the existence proof, and made far better connection to the underlying economic theory. Moreover, he had specifically pointed out in his *published* abstract of that paper that the restriction to Graham's trade model was irrelevant to the larger issue of existence of a competitive equilibrium. How, then to solidify at least an equal claim to priority to that of Arrow-Debreu? His strategy was to give the expanded paper, making absolutely clear that its generality was far more than just the Graham model, at the Summer Meetings of the Econometric Society in Ontario in September 1953. The published abstract of the paper, he hoped, would itself appear before either of the published papers by him or Arrow-Debreu. The note from Strotz had to be crushing to those hopes, as it meant that there would be no recognition of his powerful theorem in print before both of the original papers appeared.

Finally, on December 14, 1953, eight months after his original submission, McKenzie received a letter from Managing Editor Strotz which read:

“At last I can report to you on your manuscript entitled “On Equilibrium in Graham’s Model of World Trade and Other Competitive Systems”. The paper is favorably refereed and I am today writing to Professor Frisch [Editor of *Econometrica*] to recommend it for publication. I feel quite confident that he will concur in this recommendation. At the same time, it appears that a fairish amount of revision is desirable, although it is thought that the desired revision would not take a great deal of time to effect. What has happened is that your paper has actually been read thoroughly by only two persons. Let me call them referees number one and number two. Number two, in addition to making his own comments, read the comment of number one and commented on the comments. I’m enclosing copies of all this material, properly labeled.... My goal is to get your paper into the April issue if at all possible. This means that I really ought to get your revision along about the middle of January if this can be done.”

On January 18, 1954, approximately nine months after McKenzie’s original submission, Strotz encloses some comments received on that original McKenzie manuscript. Strotz next writes to

McKenzie on January 26<sup>th</sup> letting him know that he has marked up the paper for the printer. He also mentions that “Frisch has written me about the exposition of the mathematical material in your paper and I am enclosing an excerpt from his letter dealing with this subject with the thought that you, better than I, might take a stab at changing a few things so as to satisfy him.” The excerpt that Strotz enclosed from Frisch’s letter begins with the sentence “The Lionel McKenzie MS on Equilibrium in Graham’s Model on World Trade and Other Competitive Systems is accepted.”

There is, finally, one more issue to bring forward. Since the work by Weintraub and Gayer (2001) we had known that the original referees for McKenzie’s paper were Leo Hurwicz and John Nash. They did not do their jobs. Eventually Strotz himself as Managing Editor took the referee problem out of the hands of Nicholas Georgescu-Roegen. He asked for another referee even as one of the original ones, almost certainly Hurwicz, sent in a short positive report. But that summer Georgescu-Roegen had been replaced as Associate Editor in charge of the McKenzie paper by Robert Solow. This fact explains a remarkable letter of September 13, 1966 from Debreu to Solow.

“Dear Bob: I may have been responsible in 1953 for a misconception which I find to be spreading. I believe I should endeavor to dispel it.

On October 5, of that year, you asked me to referee for *Econometrica* the article by Lionel McKenzie that eventually appeared in the April 1954 issue. Your request put me in an

awkward situation, for on June 9, 1953, Kenneth Arrow and I had sent to Robert Strotz for publication in *Econometrica* the joint paper that was published in the July 1954 issue. The results of our joint paper were more general than those of Lionel in several ways and the main mathematical result on which our work was based was also a fixed point theorem for set-valued functions. The difference between the two papers in this respect was that Lionel used Kakutani's theorem whereas we used the theorem I had published in *Proceedings of the National Academy of Sciences*, October 1952 which rests on the generalization of Kakutani's theorem due to Eilenberg and Montgomery. We hoped thereby to be preparing the way for a theorem on the existence of a competitive equilibrium which would not depend on convexity assumptions. My *Proc. Nat. Acad. Sc.* article was sent to John von Neumann on May 29, 1952. The idea of using a fixed point theorem for set-valued functions to obtain an existence proof for the equilibrium of a competitive economy had occurred to Ken and me several months before<sup>26</sup>.

In my referee's report of December 17, 1953, I leaned away from the temptation to tell you all this and tried to evaluate Lionel's paper on its merits denying myself use of the information that I

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<sup>26</sup> Debreu is thus claiming that the idea for the proof had occurred to him, and Arrow, in early 1952.

have just imparted to you. As a result, my report was undoubtedly confusing.

I began to wonder whether I should write to you about this matter some seven or eight years ago when I read footnote 1, p. 374, of Dorfman-Samuelson-Solow [*Linear Programming and Economic Analysis*. New York: McGraw Hill, 1958]<sup>27</sup>. I hope I am not too hasty in writing today.”

#### **Part V: Simultaneous Discovery, Priority, and the Matthew Effect**

In his letter to the Montezuma high school student, Langdon York, McKenzie concludes

“Congratulations on your selection as star student. May I wish you lots of luck as your life develops. I’m sorry to have to say that luck is needed but the truth is that it is. I hope you have plenty of it.” (p. 2)

The several references in his own letters (e.g Morris Abram) to the Nobel Prize suggest that the bad luck he was himself recalling had to do with the difficulty he had publishing his 1954 paper

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<sup>27</sup> That footnote begins: “The use of the Kakutani theorem to prove the existence of an equilibrium is McKenzie’s idea. See his study of Graham’s international-trade model ...”

quickly enough to establish real priority for his own work on the existence of equilibrium, work that would result in those prizes for both Arrow and Debreu<sup>28</sup>.

The issues of priority and simultaneous “discovery” intrude in this narrative in several places. First, there are issues of accident and contingency. Consider the independent presentation of the existence proofs at the December 1953 Chicago meeting. Debreu twice noted in his letters to me that his paper with Arrow had been presented before McKenzie’s. This apparently, in Debreu’s view, meant that McKenzie’s paper could not have any claim to priority, else why did Debreu focus on the order of the presentations? There is no particular reason why one session chaired by Hotelling should have taken place on a Saturday, and one by Bronfenbrenner should have occurred on Monday. The program could have had those sessions reversed. Since the abstracts of both papers would have had to have been approved for presentation at the meeting, we may assume that both abstracts were sent in to meet the same deadline. Thus with respect to which paper was read, in public, first, they were simultaneous. We do know, however, that the Arrow-Debreu paper was in fact submitted with a cover page stating its origin as an Office of Naval Research Technical Report (Weintraub and Gayer 2001, 428;

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<sup>28</sup> The McKenzie papers include a copy of a mailgram sent on 10/17/83 to Debreu: “Dear Gerard. Blanche and I are very happy that you have received your deserved award. Congratulations to you and our warm regards to you and to Françoise. Lionel.” The response to McKenzie on November 2 is a form letter sent to several hundred people, which concludes “To each of you I owe a personal note of thanks. Since an endless list of commitments will not permit me to write these notes, I shall remain totally in debt to all of you for the extraordinary support that you are giving me. Gerard Debreu”

Weintraub 2002, 192). It is plausible that the paper, in that form, was in the semi-public domain before it was submitted to *Econometrica* even though that claim would be unlikely to bear much scrutiny. We have no evidence, nor were any ever proffered from Arrow, Debreu, or McKenzie that there were comments made on the papers prior to the Chicago meeting that might be traced through third party files still extant.

The contingency of the referee process was crucial. The Arrow-Debreu paper, submitted at least two months after McKenzie's submission, was refereed by Baumol and Phipps, both of whom did their work quickly, with no prodding needed from either Strotz or Georgescu-Roegen. McKenzie's referees, selected before those for the Arrow-Debreu paper, were Leonid Hurwicz and John Nash. Whether Hurwicz had then the reputation that he later developed, of not reading his mail in a timely fashion, or replying in a timely fashion, is not clear. Nor is it clear that those not immediately connected with John Nash knew of his schizophrenia, and the ravages it was making of his previous life. Had a Princeton mathematician been asked about Nash, that information which would have been privately known might have been shared, but at that time mental illness was not as widely discussed as it is today. In any event it was McKenzie's bad luck to have drawn the referees he did.

Thus the referee process continued (or did not!) through the summer of 1953. By the end of the summer, Strotz as Managing Editor had stepped in after having prodding Hurwicz and Nash, to no avail, several times. Finally in probably late September Hurwicz sent in a report. That report was sent by Strotz on October 5 to Debreu together with a request by Strotz that Debreu himself referee the McKenzie paper. Debreu apparently was unaware that the McKenzie

paper had been submitted before his own, namely in early April, and wrote as if he believed that the McKenzie paper was a new submission of the paper read in Chicago the previous December.

Added to this mixed up collection of misunderstandings, on October 8, 1953 Georgescu-Roegen, in transmitting the two referee reports by Baumol and Phipps, and his own views about, the Arrow-Debreu paper on to Strotz recommending its publication, made a plea for simplifying the paper:

“Would it not be possible either to make the proof more elementary and simpler or to present it as elaborated consequences of other well-known theorems? I heard at Kingston the paper given by McKenzie and was impressed by the very small place occupied by the technical mathematical proof in the argument.”

*Thus Georgescu-Roegen, who had heard the McKenzie paper in Kingston, and knew that McKenzie had submitted his Chicago paper to Econometrica before the Arrow-Debreu paper was submitted, was asking Strotz to ask Arrow and Debreu to simplify their proof along the lines of McKenzie's, using a simple exposition of the Kakutani theorem.*

Thus chronology is complicated by the occurrence of the Summer Meeting of the Econometric Society in Kingston Ontario from August 31-September 4, during which time the Arrow-Debreu and McKenzie papers were still being refereed. Robert Solow was chairman of the program committee, but both McKenzie and Debreu were listed as members of that committee. McKenzie submitted his paper directly to Solow; there is no evidence that Debreu

was aware of it before the meeting. McKenzie gave his paper on September 3<sup>rd</sup>, in a session chaired by Strotz, with Koopmans the discussant. There is no evidence that either Arrow or Debreu attended those meetings in Kingston, the program for which (but with no abstracts) was published in *Econometrica*, Vol. 22, No. 4 (Oct., 1954), pp. 511-512. Finally, the McKenzie paper was published, honoring Strotz's promise to McKenzie, the following April 1954, with the Arrow-Debreu paper being published in the (very next) issue, of July 1954.

The fact that the final referee on the McKenzie paper was Gerard Debreu opens a Pandora's Box of serious issues which go beyond the contingent. We know that, in 1953-1954, there was a concern about priority expressed by both McKenzie and Debreu. We see it in the letters between McKenzie and Strotz, and in the recollections of both McKenzie and Debreu in their letters to me in the early 1980s concerning the Chicago meetings. What is difficult, at this distance, is to tease out the evolving memories and sensitivities of the participants in this drama. Solow, for instance, was unaware when he wrote to McKenzie about the paper to be given in Kingston that McKenzie had given his major paper earlier in Chicago. Thus Solow, representative of most scholars at the time, only knew that there was an Arrow-Debreu paper that had been in the works that year.

By the 1960s it was fairly clear to mathematical economists that the Kakutani theorem was the preferred method of demonstrating the existence of an equilibrium for a competitive general equilibrium model. This view likely resulted from the 1959 volume *Theory of Value* by Debreu, which introduced the Kakutani Theorem in the introductory chapter, and employed it in the existence proof of Chapter 5. The Preface makes no mention of McKenzie's work in the

narrative of the development of the theory up to 1959, a narrative that credits Wald, von Neumann and Morgenstern, and Koopmans, and of course Arrow. Yet the Dorfman-Samuelson-Solow volume, of 1958 clearly attributes<sup>29</sup> the use of Kakutani in the direct proof to McKenzie, a fact that led to Debreu's letter to Solow in 1966 asking that he, not McKenzie, be credited with that move in the sequence. Of course Debreu was quite wrong to suggest in that letter that his paper with Arrow had been submitted prior to McKenzie's, a belief apparently based on the fact that he himself only saw McKenzie's paper as a Fall 1953 submission. Thus his conviction was formed on incomplete information.

Yet Debreu's letter to Solow also claims that he was thinking about using the Kakutani Theorem in 1952 apparently, in his own opinion, before McKenzie had thought of using the theorem. But McKenzie had returned to Duke in 1951 to work on his existence paper, using Kakutani's theorem as developed in the notes written by Morton Slater he obtained in his year at Cowles in 1950-51. And when McKenzie was at Cowles that year, he recalled asking Debreu what he was working on, and Debreu's refusing to tell him!

Certainly the priority issue rankled. McKenzie felt that he had been marginalized in the division of credit, while Debreu believed, for a variety of reasons some of which were in fact false, that McKenzie had been second through the door. I had not, of course, realized that Debreu was sensitive to my employing the ADM acronym. The Matthew Effect<sup>30</sup> seemed to be

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<sup>29</sup> See footnote 27, above.

<sup>30</sup> Vide "For unto every one that hath shall be given, he shall have abundance: but from him that hath not shall be taken away even that which he hath" from the Gospel According to St. Matthew

lurking in the background. And in the early 1980s Debreu was incredulous that McKenzie was claiming in his letters to me that he and Arrow had misread Wald. Arrow however agreed with McKenzie that he had gotten Wald wrong. With the Nobel Prize given to him, Debreu could be more generous, and in his Nobel Prize speech in 1983, Debreu said “In addition to the work of Arrow and me, begun independently and completed jointly, Lionel McKenzie at Duke University proved the existence of an ‘Equilibrium in Graham’s Model of World Trade and Other Competitive Systems’ [1954] also using Kakutani’s theorem.”

Finally, we must not let this narrative end without calling attention to the most distressing set of professional journal practices revealed by the editing and refereeing of both the Arrow-Debreu and McKenzie papers at *Econometrica*. *Econometrica* in the early 1950s was still a coterie journal, not widely read by professional economists. It was created in conjunction with the Econometric Society in the 1930s, and had few subscribers compared to *The American Economic Review* (published by the AEA), the *Journal of Political Economy* (from the University of Chicago), and the *Quarterly Journal of Economics* (from Harvard University). Its articles include citations to other highly sophisticated mathematical and statistical work. The economics profession was indeed changing in the early 1950s, but the fact that McKenzie was intellectually isolated at Duke University exemplifies the lack of change in most institutions, since prewar training in economics would not enable economists to read *Econometrica* with ease.

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noted in Chapter 20 (p. 445) of Robert Merton’s *The Sociology of Science* (1973), reprinted from Merton’s (1968) paper in *Science*. Or is there sensitivity on Debreu’s part to my own possible awareness of Stigler’s Law of Eponymy : “ No scientific discovery is named after its scientific discoverer.” (Stigler 1999, Chapter 14).

As a consequence perhaps, the more sophisticated readers of the journal formed a small group meeting at conferences, circulating papers to one another, and generally aware of work being done by like-minded economists around the world<sup>31</sup>. Unlike today, when placement of an article in *Econometrica* nearly secures tenure for a North American Assistant Professor of Economics, in the early 1950s such a publication would have been seen as a contribution to a field journal which few read. The small number of submissions compared to the *AER*, and the small number of individuals who might be called upon to edit and referee papers for the journal, must have led to the kind of informal practices that characterized the journal's treatment of the Arrow-Debreu and McKenzie papers, though surely this case is egregious. As Associate Editor Georgescu-Roegen wrote (October 8, 1953) to Strotz concerning the submission of the Arrow-Debreu paper:

“the mathematics and the economics are so much inter-woven in the argument that I found it difficult to think of many referees who would be at the same time economists and mathematicians so that the critical reading of the paper would not impose upon them a tremendous task.”

He then went on to note that Baumol's and Phipps's referee comments were unhelpful, and that he himself was thus providing comments to Arrow and Debreu saying

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<sup>31</sup> See Gary Wersky's *The Visible College*, London: Free Association Books, (1988 [1978] )

“I have the highest opinion of the authors and I trust Debreu’s mathematics, yet I recommend that somebody check the mathematics. This could be done while the authors revise the present version, thus saving considerable time.”

Thus the Arrow-Debreu paper was not really refereed<sup>32</sup>, and the McKenzie paper was only lightly read by Hurwicz before it was “refereed” by Debreu, who under no circumstances should have been allowed to even read the paper without McKenzie’s approval. That Debreu did not believe, at the time, that he should have recused himself from that process, and that no editor expressed discomfort with the practices apparently in place, creates a curious picture of an intellectual community about to come into prominence in economics. That picture, at the distance of more than a half century, is not pretty.

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<sup>32</sup> Although I have not stressed the point in this narrative, it should be clear that I regard Nicholas-Georgescu Roegen’s role in the editing process as nothing short of scandalous. His own narcissism led him astray, and his actions did real damage to the high professional standards of *Econometrica*, in both his treatment of the Arrow-Debreu paper and the McKenzie paper.

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