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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 Klein, my advisor, sent me to read material on the stability of general equilibrium, and so I became familiar with 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 semi-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 that, 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 in my references to this, 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 (p. 27, footnote 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, something for which I have felt quite pleased about 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 sent me to historical work. My colleagues, Craufurd Goodwin and Neil DeMarchi 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 DeMarchi 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-Roegen, 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 both 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 25th, 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. This progression of ideas led Hicks, over the next period of time, to describe himself as
quite opposed to 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 how would it be possible to talk about Hicks and Arrow 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” of 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 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 or 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 lead to my stay at Chicago for one full year (12 months), in 1949-50…”

On his return to Duke following the year at Cowles, he 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 my 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 possibility 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 not dependent 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 proof closely!” In other words, McKenzie is shocked that Arrow, and presumably Debreu, were not aware that Arrow-Debreu’s use of an assumption tantamount to the weak axiom of reveal 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, suggested that Arrow was unaware of that issue.

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 piece written by Robert Dorfman at Harvard 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, following a discussion of my 1983 paper itself, 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 contribution, something I would never knowingly do. The truth is that, until I read your 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 New York Times was 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 slight anyone. Of course, I don’t believe Dorfman intended to slight 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 Manuscript, Rare Book and Special Collections department. Following this, I began working with Craufurd Goodwin, Neil DeMarchi, and A. W. ‘Bob’ Coats, with the then 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 Econ* conference, each of which resulted in special supplementary hardback 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* 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 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 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. 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, (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 that a 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 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 Gayer-Weintraub (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 elsewhere (Weintraub and Gayer, 2001). There are though 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 new availability of the McKenzie and Solow papers at Duke. Telling this story, which involves not only re-shading and reemphasizing and retelling this story that had earlier been developed, but incorporating some very startling and disturbing new material related to the Arrow-Debreu and McKenzie papers, will in fact require my going back to rewrite even those past attempts. It is this narrative that I shall now engage.