

Title	A SCHOLAR'S PROGRESS
Sub Title	
Author	MCKENZIE, Lionel W.
Publisher	Keio Economic Society, Keio University
Publication year	1999
Jtitle	Keio economic studies Vol.36, No.1 (1999.) ,p.1- 12
JaLC DOI	
Abstract	
Notes	
Genre	Journal Article
URL	https://koara.lib.keio.ac.jp/xoonips/modules/xoonips/detail.php?koara_id=AA00260492-19990001-0001

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A SCHOLAR'S PROGRESS[†]

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My first contact with economics was while attending Middle Georgia College in my home state. I am not sure of the order of events but in one summer I read Adam Smith's *Wealth of Nations* from a set of classics and one semester I attended a Principles course where a revised edition of a very old text by Richard Ely was used. Ely was one of the founders of the American Economic Association and in January of this year I was chosen by Robert Fogel to give the main address, called the Ely lecture, at the annual meeting of the Association. Fogel did his Nobel Prize research at Rochester in the early days. However, I think the book by Adam Smith which was written a hundred years before the American Economic Association was founded played a larger role in turning me to economics. It was 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. I concentrated in economics. I should say that the strongest competition for economics in my life plans was physics to which I had been attracted by popular books on science by Eddington and Jeans. 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.

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. I knew very little about either economics or the character of the Princeton department when I entered there. However I found a number of quite stimulating professors in the program. I would particularly mention Frank Graham and Oskar Morgenstern. Graham introduced me to a multisector, many country model of international trade which many years later played a major role in my research program, leading me to my first theorem

[†] This paper was presented at Keio University on the occasion of the award of the honorary degree Doctor of Economics in June, 1998.

on the existence of a competitive economic equilibrium. Morgenstern taught me advanced economic theory where we read and criticized the new book *Value and Capital* by John Hicks. Many years later I was privileged to lead a conference celebrating the fifty'th anniversary of the publication of this book. I should mention that Morgenstern was not uncritical of Hicks' book. In particular he ridiculed Hicks' use of the equality of the number of equations and the number of variables to conclude that an economic equilibrium existed. 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 led by Karl Menger. However, he did not give us references to these papers in his seminar. I pointed this out to Oskar after he published an account of his collaboration with von Neumann at Princeton in the *Journal of Economic Literature*. I learned about them much later from Tjalling Koopmans. I also benefited from a course in mathematical economics led by our resident statistician, Acheson Duncan. We read Walras and Pareto in the french texts at a time when more modern literature of this type hardly existed. 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 course work and began a project with Morgenstern on futures markets, which was never completed. 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 the theorist, but I don't believe I made any significant contribution to either theory or the war effort. My closest associate in the Office of Civilian Supply was Morris Adelman who later taught industrial organization at MIT. In 1943 I entered the Navy in a noncombatant role as a cable censor in Panama and New York. 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. She has faithfully supported me in my life and scholarship for some 55 years now. As the former Japanese students are well aware, she was also an important part of the life of the economics department during my chairmanship at Rochester, and for many years after that.

Upon leaving the Navy I obtained an appointment in 1946 as instructor at MIT, thanks to the recommendation of my former Princeton colleague Ansley Coale, who became a distinguished demographer. This was after Paul Samuelson had become the star in residence publishing his *Foundations* and preparing his principles text. I had already some knowledge of his work. Indeed, I reported to Morgenstern's business cycles class on his paper that described a model combining the multiplier with the accelerator. Morgenstern liked this paper because it appeared to demonstrate that pump priming was not a feasible policy for defeating an economic depression. 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 do 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 think my

teaching in this unfamiliar field was rather poor, so I suspect MIT was happy to be relieved of my services. James Duesenberry and Guy Orcutt were my junior colleagues there. Samuelson later said, no doubt with tongue in cheek, that they should have kept us and fired the rest of the staff.

When I was in Panama with the Office of Cable Censorship during the war 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 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 was 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 lecture series in philosophy with Gilbert Ryle, and also Friedrich Weizmann, who had been a member of the Vienna Circle of philosophers in earlier years. I was inspired by the large amount of philosophy I had read in Panama during the war. Of course, I attended Hicks' 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 attended Hicks' class, along with Paul Streeten, was also writing on this subject and was my closest associate at Oxford. As it happened the leading economics scholar at the LSE, William Baumol, and the leading economics scholar at Cambridge, J. de V. 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, and we all spoke on this subject. I wrote a draft of a thesis but my examiners quite rightly ruled that it was not a finished product and should be revised. However, I was too pessimistic about its future to comply, so I had to be satisfied with the less prestigious degree of B. Litt. I am happy to report that Little, Baumol, and de Graaff were more successful and their theses eventuated in well known and respected treatises on the New Welfare Economics, which many of you will have read. The indirect outcome of my project was a paper in the *Economic Journal* in 1951, entitled *Ideal Output*. However this paper was not part of the draft of my thesis.

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 Friedrich 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.

At Duke I wrote the *Ideal Output* paper which demonstrated that prices proportional to marginal costs would not guarantee an ideal output if any intermediate products were included. Indeed nothing would succeed except prices equal to marginal costs. I treated primary inputs as supplied independently of prices. In the course of writing this paper I had to deal with a multisector economy and in a footnote I introduced a simple equation system to illustrate the problem of interdependence. At the same time I no-

ticed 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 that I could 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 at Chicago. So after two years teaching at Duke, in the fall of 1950, I went to the University of Chicago as a graduate student in economics.

The time I spent in the Cowles Commission was decisive in setting the character of my research career. 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 MacLane in topology. 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 had encountered in England. Incidentally I was indirectly responsible for calling this work to the attention of von Neumann and Morgenstern by way of my Princeton 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. Irving Siegel, whom I had known from Princeton, suggested that I study differential topology, but I foolishly failed to do so. I withdrew from a statistics course taught by William Kruskal, also not a smart thing to do.

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 Beckmann, and Edmond Malinvaud. Gerard Debreu, Karl Brunner, and Harry Markowitz were also in the Cowles Commission group, as well as Leo Hurwicz for a while. I attended no classes given by a regular member of the economics department. I offered to attend Lloyd Metzler's class in macroeconomics, but he assured me it was not worth my time. Amusingly I later used the text he assigned his students, a little brochure by Marschak, in a course I taught at Michigan as a visitor. I did not pay attention to Milton Friedman while I was at Chicago, although he was already a famous man. I am afraid my Princeton friends were Keynesians, no thanks to our teachers, and regarded the quantity theory of money as belonging to the Dark Ages. Incidentally, while I was in the Office of Civilian Supply, Milton, on the advice of my boss, Al Oxenfeldt, had asked me by telephone whether I thought price control on minerals would discourage their extraction in wartime. As I recall I did him no good.

A piece of research I completed at Chicago was done in Koopmans' class on activity analysis and was based on Grahams' model of international trade that I remembered from Princeton. It was a multisector analysis of comparative advantage which showed that bilateral comparison of comparative advantages was not sufficient to discover an efficient allocation of world production. This led to my article in *The Review of Eco-*

nomic 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 that I should return to Duke, a decision somewhat like my decision not to pursue the Oxford thesis, or the earlier decision not to do physics at Oxford. I had become quite interested in mathematics and if I had stayed in Chicago I might well have transferred to the math department.

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. 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 and read them with my rather weak German. At this time I wrote my paper "On Equilibrium in Graham's Model of World Trade and Other Competitive Systems", published in *Econometrica* in 1954, but delivered to the Chicago meeting 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 at 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 about the Kakutani theorem from a working paper by Morton Slater, the resident mathematician at the Cowles Commission.

I then turned to the problem that Koopmans had suggested to me and succeeded in proving a theorem on the equalization of factor prices by world trade when countries have the same technology (*Econometrica*, 1955). The point of my result was to relate the factor supplies in the different countries to the cone of diversification, so-called by John Chipman. The cones of diversification include the sets of factor inputs consistent with given goods prices and different choices of factor prices. The theorem asserted that if there were a choice of factor prices which put each country's factor supply in the interior of the cone of diversification, then those factor prices would have to prevail in all countries in competitive equilibrium. In this same period I also gave a simple approach to demand theory, avoiding determinant theory and also providing results of greater generality than the classical theory of Allen and Hicks.

I also continued to pursue the subject of existence of equilibrium. This led to a paper introducing preferences for consumers that depend on what other consumers buy. It was delivered to a Symposium on Linear Programming sponsored by the Air Force and published in the proceedings of the symposium in 1956. It was at this symposium that Professor Morgenstern asked me if I would like to have a Ph. D. and offered to make the proposal to the Princeton economics department. This is how I got

my degree, by bundling the papers I had written and submitting them to Princeton. When I was given the oral exam on my thesis, William Baumol, who was a member of the committee, moved that the exam be adjourned to the bar of the Nassau tavern, but this was vetoed by Morgenstern who protested that in Vienna thesis exams were taken seriously. I think he was somewhat mollified when I complimented the paper he had written with Kemeny and Thompson on von Neumann models with multiple equilibria. By this time I had also completed my research at Duke for the existence paper published in *Econometrica* in 1959. In this paper I weakened the assumptions used by Arrow–Debreu in their 1954 paper in various ways, dispensing with free disposal of excess goods, the assumptions that activities are not reversible, and the assumption that individual consumers can subsist in isolation. I also gave the proof using the simpler Brouwer fixed point theorem.

In 1956 while still at Duke I spent the spring semester and the following summer again visiting Cowles, now at Yale and renamed the Cowles Foundation. Of the old group, Koopmans, Debreu, and Markowitz were in residence. James Tobin was a newcomer and now the head of the group. I met Richard Rosett there and later recruited him for the Rochester department as an econometrician. My research efforts on this visit were devoted to the Ramsey theory of optimal taxation and a study of matrices with dominant diagonals. My Ramsey result was to extend his theory in a multisector model to taxes on intermediate products. My results were used in a workshop that I gave as part of a Social Science Research Council conference at Stanford in the summer of 1957 to promote the use of mathematical methods in the social sciences and were reported to a summer meeting of the Econometric Society held in Ann Arbor, Michigan, but they were never submitted for publication and I have no idea where my notes are now. An abstract was published in the summary of the meeting in *Econometrica*. I remember that Kenneth Arrow was in the audience and seemed to like the paper. The study of dominant diagonal matrices was my contribution to the conference on Mathematical Methods in the Social Sciences held at Stanford in the summer of 1959. The most interesting result was probably to demonstrate that the Frobenius theorem on positive matrices could be derived by methods that used the theory of dominant diagonal matrices. I also described various applications to economics. My final bit of research at Duke was devoted to the stability of the Walrasian tâtonnement when all goods are substitutes for market demand. I extended the Arrow–Hurwicz theory to the case where some goods might be locally independent for market demand. I believe Hirofumi Uzawa did something like this at the same time. My paper was published in *Econometrica* in 1960. This completed what may have been my most productive period of research, all of it while I held a position at Duke University.

While I was visiting the Cowles Foundation, Bernard Schilling, a professor of English from University of Rochester came by to discuss the possibility that I might be interested in a position there to head an economics department with the aim of developing a Ph. D. program and, of course, to judge whether I was worth looking at for such a post. I later learned that I had been recommended by Samuelson and Solow for this job. The Dean at Rochester had gone to MIT in some desperation since the people they

had approached had apparently not expressed an interest. That fall I visited Rochester and I was very favorably impressed by Dean Alfred Noyes, a distinguished chemist, who had been forced on the administration by the Rochester faculty. Something they probably could not do today. In the event I took the job although I offered to stay at Duke for a salary well below the Rochester offer.

In the first year at Rochester, the academic year 1957–58, the only recruit joining the new department in the tenure track was Ronald Jones. Ron was recommended by Bob Solow who had been his supervisor. He assumed responsibility for the field of international trade where he has had a distinguished career. He is still with us, and he has been a major strength for the department for forty years. As many of you will know he has had many Japanese students who have had distinguished careers in Japanese universities both in Kanto and Kansei. Fortunately he is still active in teaching and research. Also arriving in the first year was our first Ph. D. student, Akira Takayama. Many of you will be familiar with his very successful career in international economics and mathematical economics. His book on *Mathematical Economics* is a standard text and reference work. In the second year we were joined by Richard Rosett and Edward Zabel for econometrics and theory of the firm. In this year we began our doctoral program in earnest. Among the Japanese students who came in the early years should be mentioned Akihiro Amano and Hiroshi Atsumi. Amano worked with Jones and Atsumi with me, though we were all involved in their education. A little later Yasuo Uekawa came and this completed the earliest group of Japanese doctoral candidates. How could we have done better? They gave the department a standard of intelligence and application that has remained its hallmark.

Shortly after arriving in Rochester I read the book by Dorfman, Samuelson, and Solow (DOSSO) on *Linear Programming and Economic Analysis* which led me to decide to turn my research efforts to the theory of optimal growth, in particular, to the problem of a turnpike which describes, when it exists, the asymptotic course of an optimal growth path from whatever initial position it starts. The DOSSO book had devoted some attention to the problem without reaching a very satisfactory solution. I think it was in the course of teaching my first graduate class in economic theory, what is now the first semester of Value Theory, that I discovered a way to prove the turnpike theorem for a simple Leontief model with variable production coefficients, using an old theorem of Samuelson and Solow. This was presented to the winter meeting of the Econometric Society held in St. Louis in December, 1960. Morishima had proved a parallel result, unknown to me, by a very different method. The papers of Morishima, Radner, and me, all completed around 1960 gave the first complete proofs of a multi-sector turnpike theorem. However they had the defect that they took maximization of the accumulation of capital goods in certain ratios over a finite period as their objective. This was the problem DOSSO had posed for the von Neumann model, which did not have an objective in the form in which it was presented by von Neumann.

I noticed a weak point of the Radner theorem, elegant though it was. This is the fact that it assumes strict convexity of the production set at least near the turnpike. This is inconsistent with the neoclassical production model which has a number of independ-

ent industries. The variation of the levels of production in these industries generates a flat section of the boundary of the production set which typically contains the von Neumann equilibrium in its interior. The consequence is that the convergence implied by the Radner argument leads to the flat sector that contains the von Neumann equilibrium, not to the equilibrium itself. However I saw that a further convergence might be proved since the technology that spanned the flat containing the von Neumann equilibrium could have stability properties itself. This led to my proof of a turnpike for a generalized Leontief model with variable coefficients and capital goods. In the proof of this theorem the methods used by Radner were supplemented by an analysis of the difference equations implied by the technology that was active on the flat containing the von Neumann equilibrium. This analysis was extended to a von Neumann model where multiple equilibria are possible in a paper given to a conference on Activity Analysis and the Theory of Growth and Planning in Cambridge, England, in 1963.

These turnpike theorems were of doubtful economic relevance since they did not respect the satisfaction of consumer preferences. The next step was to marry the von Neumann model with its activities model of production to the Ramsey model of optimal growth, where the maximization of consumer utility is the objective. The transition to this more relevant problem was begun in two conferences on optimal economic growth held in 1964 and 1965, the first at the University of Rochester and the second at Stanford. I led the first of these and Kenneth Arrow led the second. Among the people present at the Rochester conference were several Japanese, Ken-ichi Inada, Hukukane Nikaido, Jinkichi Tsukui, and Hiroshi Atsumi. Atsumi was finishing his Rochester thesis at the time. Other members of the group included David Gale, Dan McFadden, and Maurice McManus. It was at this conference that, as I remember it, I introduced the reduced form approach to modelling optimal economic growth. In this approach the von Neumann practice of describing the production as a transformation of initial stocks into terminal stocks over the production period is adapted to the Ramsey style optimal growth model by, in effect, including the utility achieved over the period as an output along with the terminal capital stock. The papers of Gale, McFadden, and me begun in the conference all took this approach. Papers begun there were continued in the second conference. Many of them were published in the January, 1967, issue of *The Review of Economic Studies*. However, the first turnpike theorem using a method that would generalize to any number of goods was proved by Hiroshi Atsumi as part of his thesis project. He also, simultaneously with Weizsäcker, introduced the overtaking criterion to allow an objective to be defined in terms of a sum of utility even though utility is not discounted and the horizon is infinite. His paper was given to the Boston meeting of the Econometric Society in December, 1963, and published in *The Review of Economic Studies* in April, 1965. Atsumi's theorem was extended to any number of goods by me and Gale. The generalization was used by Gale in the course of proving the existence of the infinite optimal program, and by me as part of an elaborate analysis of turnpikes when strict convexity assumptions are not made. My paper was published in the Hicks festschrift, *Value, Capital, and Growth* (1968), edited by J. N. Wolfe (who incidentally had visited Rochester for our first year). Gale's paper ap-

peared in the issue of *The Review of Economic Studies* just mentioned. If we consider the Samuelson turnpike with the objective of maximal capital accumulation as the first phase of the turnpike research program, these papers effectively ended the second phase of the program, where an objective is to maximize a sum of undiscounted consumer utility over an infinite horizon.

The next step was to prove a turnpike theorem over the infinite horizon where the discounting of consumer utility is allowed. An early contribution was made by Atsumi in the context of a Leontief model with capital goods and fixed coefficients. His paper "The Efficient Capital Program for a Maintainable Utility Level" appeared in *The Review of Economic Studies*, July, 1969. A simpler proof in a closely related model was given in 1994 by Majumdar and Mitra, the latter also one of our graduates. These results were used to good effect very recently by Kaganovitch in a way that I will describe later. It took a remarkably long time to extend this theorem to a general model. But I am happy to report that this step was also taken at Rochester by my student José Scheinkman. There were also two conferences at this time in which the third phase of turnpike theory was featured. They were held at Squam Lake near Hanover, New Hampshire, and at the University of Pennsylvania. Scheinkman's paper was presented at one of these and appeared along with other papers from the conferences in the *Journal of Economic Theory* in February, 1976. It might be mentioned that these conferences as well as that earlier at Stanford were supported by the Mathematical Social Sciences Board, in which I served as the economics member.

My period as department chairman ended in 1966. I was succeeded by Richard Rosett who also chaired the department for nine years. Our doctoral program in economics at Rochester continued to prosper. Over the years we have had a great many students from Japan, the largest contingent from a foreign country. I believe there are some fifty odd former students resident in Japan. However, we have also received many students from India, Greece, and Taiwan. In recent years many students have come from Korea and China, as well as Mexico, Italy, Portugal, and Sweden. Of course a smattering of students have come from all over, perhaps not excluding any major country. I have sometimes boasted that we had a former student teaching in every major Japanese University. Perhaps a proposition like that should not be examined too closely! However very few people would guess that the University where the largest number of Rochester graduates teach, at least at the time when we looked some years ago, is the University of Calcutta. This emphasis on bringing in students of quality wherever they might live was firmly established in the start of our program. It has never been given up although our somewhat reduced resources prevent us from supporting as many students from very poor countries as we would like. Fortunately Japan has increased so greatly in wealth since our program began that it does not fall in the set of countries notably poorer than ourselves. I could go on for a very long time telling you stories about our program but I fear that would try your patience.

My research continued to be concerned with optimal economic growth and existence of equilibrium. In my Fisher-Schultz lecture to the European meeting of the Econometrics Society in Grenoble I gave an account of Turnpike Theory, which fea-

tured an extension, to the case of undiscounted utility, of the proof that support prices exist for optimal programs with discounted utility, given by Weitzman in 1973. In my Presidential address to the Econometric Society given in Ottawa and Vienna in 1977 I gave an existence theorem which dispensed with both the assumption that individual consumers could survive in isolation and the assumption that consumer preferences are transitive. A few years later I succeeded in proving a turnpike theorem for the optimal growth model with discounting without the assumption, made by Scheinkman and by Cass and Shell, that the social production set has a smooth boundary. This theorem introduced the notion of a neighborhood turnpike theorem in which the convergence requirement was further reduced by describing a convergence to a neighborhood of the von Neumann facet, the name that I gave to the flat on the social utility function that contained a Golden Age, rather than to the facet itself. And similarly for the further convergence to a Golden Age if the facet was stable. These results were included in my contribution to the *Handbook of Mathematical Economics* (1986). Its publication had been long delayed. My chapter was entitled "Optimal Economic Growth, Turnpike Theorems and Comparative Dynamics". It also contained joint work with my former student, Swapan Dasgupta, on the effects of variations in the discount factor on the optimal paths. To complete the account of my contributions to optimal growth theory I found a way to prove the theorems without using price supports for the optimal path by exploiting the characterization of a convex set as a set whose chords do not rise above the boundary rather than as a set supported by a hyperplane at every boundary point. This was published in the *Journal of Economic Theory* in 1981. Finally I gave a comprehensive account of my theory allowing for von Neumann facets and discounted utility in a paper published in the *Journal of Economic Theory* in 1983. This completed my contribution to the subject of optimal economic growth which had occupied me most of the time since I came to Rochester.

My work on optimal economic growth has been carried forward very effectively by my former students and some closely related colleagues. The current state of their researches is well represented in the conference led by Kazuo Nishimura which ended here just yesterday. The former student who has continued to develop the theory in closest harmony with the line of my research is Makoto Yano. Along with Truman Bewley and Robert Becker, also a former student, Yano has introduced yet another phase of the research program in which the analysis is applied to the competitive equilibrium by showing that the competitive equilibrium will in fact realize an optimal program for a properly chosen social welfare function. Of course this development brings the optimal growth theory into contact with descriptive economic development and practical fiscal policies. Yano has illustrated this connection by using the theory to provide a firmer basis for a claim of Milton Friedman on the effects of temporary fiscal policy in a recent article in *Econometrica*. Harutaka Takahashi has also explored implications of the von Neumann facet. Michele Boldrin, Kazuo Nishimura, Makoto Yano, Tapan Mitra, Jess Benhabib, and Mukul Majumdar, the first four former students and last two having connections to our program, have studied the possibilities of chaotic motions of optimal, and therefore, competitive paths. These studies are relevant to business cycle

research. Other former students and colleagues, in particular, William Brock and Leonard Mirman, the former a colleague and the latter a student, have studied the complications that arise when uncertainty is introduced into technology or consumer preferences. It would be tedious for you to have to listen to the full account of all these developments. Nor am I the best person to describe them.

With mathematical help from my colleague, John H. Boyd III, I made one further essay into the theory of existence of equilibrium. A major development in existence theory since the fifties has been the treatment of the case of infinite horizons. This case has special interest for growth theorists since the modern growth model uses an infinite horizon and the theorist is often interested in competitive equilibria where he does not have a social utility given to him apart from the competitive equilibrium. Thus it is rather important to have some handle on the question, does the competitive equilibrium exist? When I wanted to include this case in my class notes, which I was expecting to prepare for publication, I found that all the proofs I knew about contained a disabling assumption, that the possible consumption set of the individual consumer was equal to the positive orthant. There are two difficulties with this assumption. First it ignores the fact that consumers cannot survive if they consume nothing. Second it ignores the economics of substitution in consumption on the boundary of the possible consumption set. I proposed a way of introducing a realistic consumption set into the model and with John Boyd's help a rigorous argument was developed. The paper was published in the *International Economic Review*, February, 1993. This journal, incidentally, is jointly sponsored by the University of Pennsylvania and Osaka University.

To complete this history, the most recent growth theory, continuing the line of development of the other phases I have described, is the so-called endogenous growth theory in which investments in technology and human capital are introduced to provide a basis for continued growth, possibly at undiminished rates, despite the limitations on natural resources and labor supplies. This development was begun by Paul Romer in his Chicago thesis which he worked up into articles while teaching at Rochester. His thesis was supervised by the Rochester graduate, José Scheinkman. Most of the work on endogenous growth has been done in models with basically no more than two capital goods, physical capital and human capital. However Michael Kaganovich combined the approach of my turnpike theorem on the simple Leontief model with variable coefficients and the theorem on optimal growth in a simple Leontief with variable coefficients of Atsumi, as modified by Majumdar and Mitra, to prove a turnpike theorem for the simple Leontief model with variable coefficients where the objective is to maximize a discounted utility sum over the infinite future. This theorem applies to the models of endogenous growth where the technology converts initial stocks of physical capital and human capital into terminal stocks of these goods and utility over the period. In addition, it allows for any number of capital goods. In the paper that I prepared for the Ely Lecture to the Chicago meeting of the American Economic Association I gave an overview of turnpike theory and concluded the paper with references to the recent results of Yano on fiscal policy and Kaganovich on Leontief models. Kaganovich is an immigrant from the Soviet Union whose placement in the

United States owes something to Rochester people.

These are the academic things I have had something to do with. In concluding I would like to express my deep gratitude to the Rochester Japanese students who have so much enriched the lives of me and my wife as well as the intellectual life of the department of economics. I apologize that I could only mention those who happened to be closest to my own research program. There were many others of great importance to us and to Rochester economics. The tradition of a Japanese presence at Rochester continues with the many Japanese students of Ronald Jones and of William Thomson, who has brought game theory to our program. Next year we are joined by Larry Epstein who should be attractive to prospective microeconomic theorists.