THE COMPUTATION OF EQUILIBRIA FOR THE WALRASIAN MODEL: A PERSONAL ACCOUNT

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COWLES FOUNDATION PAPER NO. 1327

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2011

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Some early history

I was a graduate student in the Department of Mathematics at Princeton from September 1951 until June 1954. After graduation I left Princeton for the RAND Corporation. One of the major reasons for choosing RAND rather than a more conventional academic position was my desire to be involved in applied rather than abstract mathematics. I could not have selected a better location for this particular goal. George Dantzig had arrived recently and was in the process of applying linear programming techniques to a growing body of basic problems. Richard Bellman was convinced that all optimization problems with a dynamic structure (and many others) could fruitfully be formulated, and solved, as dynamic programs. Ray Fulkerson and Lester Ford had turned their attention to network flow problems, a topic which became the springboard for the fertile field of combinatorial optimization. Dantzig and Fulkerson studied the Traveling Salesman problem and other early examples of what ultimately became known, under the guidance of Ralph Gomory, as integer programming.[1]

I spent my first year at RAND in the Department of Mathematics, but in 1955 the organization was visited by a budgetary crisis and I was asked if I would mind taking up temporary residence in the Department of Logistics. The Logistics Department was a subgroup of the Department of Economics at RAND, with a much more prosaic mission than that of its senior colleagues. The members of the Logistics Department were concerned with scheduling, maintenance, repair and inventory management and not the deeper economic and strategic questions of the Cold War. I began to work in inventory theory, a topic that I pursued for the next decade.

I was fortunate to meet Samuel Karlin and Kenneth Arrow at RAND. They were both interested in inventory problems and they kindly invited me to spend the academic year 1956–57 with them at Stanford University. My natural home at Stanford would have been a Department of Operations Research, but such a department had not yet been established and I was formally located in the Department of Statistics. My office was in a charming building known as Serra House, sitting in a grove of Eucalyptus trees at the edge of the Stanford campus.

I was on the second floor of the building along with Kenneth, Hiro Uzawa and Patrick Suppes. Richard Atkinson was on the first floor and Leo Hurwicz and Bill Estes were frequent visitors. I began to learn about economic theory from a series of seminars applying mathematics to various topics in the social sciences.

Early research in economics

My involvement in the computation of economic equilibria for the Walrasian model began, somewhat indirectly, with the example of a globally unstable model of exchange that I constructed in the Spring of 1959. This was my first paper in economic theory proper.

I was tutored in the subject of stability by the reigning experts in the field: Kenneth Arrow, Leo Hurwicz and Hiro Uzawa, my neighbors at Serra House. Kenneth and Leo (joined by H. D. Block in the second paper; see Arrow, Block and Hurwicz 1959) had just published their two basic papers in Econometrica and there was much discussion of the topic of stability at seminars and in the halls of the building. The major result of their research was that the Walrasian price adjustment mechanism was globally stable in a model of exchange in which the market excess demand functions exhibited gross substitutability or satisfied the weak axiom of revealed preference. Models with production were not explicitly considered.

To be specific, let there be n goods with associated prices

$$p = (p_1, \dots p_n) \geq 0.$$

The market excess demand functions $(f_1(p), \dots, f_n(p))$ are derived from the summation of individual excess demand functions each obtained from utility maximization subject to the budget constraint—with income equal to the value of that consumer's initial endowment. The demand functions are continuous if none of the prices is zero; they are homogeneous of degree zero, and satisfy the important identity

$$p\,f(p) \equiv 0$$

known as the Walras Law. A competitive equilibrium is a price vector $\hat{p}$ such that $f(\hat{p}) \leq 0$ with $f_i(\hat{p}) = 0$ if $\hat{p}_i > 0$.

The existence of a competitive equilibrium had been established several years earlier by Arrow and Debreu (1954), Gale (1955) and McKenzie (1959) making use of one or another version of a fixed point theorem applied to a variety of different mappings. Their results guaranteed the existence of a price vector for which excess demand was zero, but gave no hint of a dynamic process converging to the equilibrium price vector. The price adjustment mechanism, formalized a number of years earlier by Paul Samuelson, was meant to be a possible description of such a process.

The Walrasian *tâtonnement* can be translated into mathematical form by the system of differential equations:

$$\frac{dp}{dt} = f(p),$$

stating that the change in price, for each commodity, is proportional to its excess demand. With this normalization, the Walras law has the peculiar consequence that any solution $p(t)$ will stay a fixed distance from the origin, since

$$\frac{d\left(\sum p_i^2\right)}{dt} = 2\, pf(p) = 0.$$

If, therefore, the initial price vector lies on the sphere of radius 1 so will all of the prices on the curve $p(t)$. The market excess demand functions therefore define a vector field on the positive part of the unit sphere which is followed by the price adjustment mechanism.

The commodities are *gross substitutes* if

$$\frac{\partial f_i}{\partial p_j} > 0 \text{ for all indices } i \neq j.$$

If this property holds then an elementary argument proposed by McKenzie shows that

$$\max_i [p_i(t) / \hat{p}_i] \text{ is decreasing and } \min_i [p_i(t)/\hat{p}_i] \text{ is increasing}$$

along the path; with some tidiness this property can be converted into an argument for global stability.

The second condition discussed by Arrow, Block and Hurwicz (1959) was that the market excess demand functions satisfy the *weak axiom of revealed preference*, i.e. that for two non-proportional price vectors, p, q the inequalities

$$pf(q) \leq 0,$$
$$qf(p) \geq 0$$

cannot both hold. If the weak axiom is satisfied then

$$\frac{1}{2} \sum (p_i(t) - \hat{p}_i)^2$$

decreases along the path: its time derivative is given by

$$\sum_i (p_i(t) - \hat{p}_i) f_i(p) = - \sum_i \hat{p}_i f_i(p)$$

which is < 0 because $pf(\hat{p}) = 0$ and therefore $\hat{p}_i f_i(p) > 0$.

The conditions have contrasting drawbacks. The weak axiom is satisfied for individual excess demand functions, but, unfortunately it does not aggregate over consumers. Gross substitutability, on the other hand, does aggregate, but it is not satisfied for individual demand functions allowing even a modest form of complementarity. Despite the limited reach of these two assumptions, there was, if I correctly remember, a tentative optimism that market excess demand functions would inherit some property, not yet clearly specified, that would suffice for global convergence of the *tâtonnement*. But for a variety of reasons this was not to be.

My paper, 'Some Examples of Global Instability of the Competitive Equilibrium' (Scarf 1960) contains a series of examples, each globally unstable. They are woven around a first example involving 3 consumers and 3 commodities using preferences based on Leontief utility functions, and therefore exhibiting the most extreme form of complementarity. In this first example, the utility functions and initial holdings possess a curious symmetry that permits us to characterize the solution explicitly as the curve defined by the intersection of the two surfaces

$$p_1^2 + p_2^2 + p_3^2 = 1;$$
$$p_1 p_2 p_3 = c.$$

For this example the solution is unstable in a weak sense: if the initial price vector is not the equilibrium, the path follows a closed periodic curve that does not approach the equilibrium. The subsequent examples of the paper cannot be solved explicitly, but they do display a unique explosive equilibrium.

Of course, the entire discussion of stability was placed in a different light many years later by the Sonnenschein (1973), Mantel (1974), Debreu (1974) theorem asserting that any continuous functions

$$f(p) = \left(f_1(p), \ldots, f_n(p)\right)$$

defined on a closed subset of the positive part of the unit sphere, and satisfying the Walras law, were the excess demand functions arising from the summation of individual demand functions. As a consequence of this remarkable and to some economists, unsettling, theorem, unstable models of exchange could be constructed at will. Simply draw an arbitrary continuous curve on the positive part of the sphere, take its tangents and flesh them out to a continuous vector field on the sphere which, by virtue of this theorem is the market excess demand function arising from a model of exchange. The price adjustment mechanism will follow this arbitrary pre-specified curve. With 4 goods, one can write one's name with a flourish on the 3 dimensional sphere.

I spoke about this material at the Summer meeting of the Econometric Society at Stanford in 1959. In my audience were Peter Diamond, who had just received his B.A. from Yale, and T. N. Srinivasan, who had just finished his Comprehensive Examinations at Yale. Both were at the very beginnings of their distinguished careers.

I was invited by Tjalling Koopmans, whom I had met at RAND, to spend the academic year 1959-60 at the Cowles Foundation, beginning an association with Cowles and Yale University which became permanent several years later. I lectured on my counter-example at a Cowles Foundation staff meeting chaired by Jim Tobin who was then the Director. I cannot say with assurance who the other members of the audience were, but Gerard Debreu, Donald Hester, Alan Manne, Art Okun, Ned Phelps, Bob Summers and Jascha Marschak were all staff members at that time and were likely to be present at my talk. Of course Tjalling was there, as was Martin Shubik who was also a visitor during that year. I do remember raising a tentative question as to whether the instability represented by my example might be related to the phenomenon of business cycles, and I also remember being treated kindly by Jim Tobin as he demurred.

I returned to the Stanford Department of Statistics in the Fall of 1960 and remained there for three years, working primarily on the theory of the core and its relation to the competitive equilibrium of a Walrasian model. I spent more time with economists than I had before visiting Yale; I remember quite vividly being at seminars with David Cass, Karl Schell and Menahem Yaari. I was, in fact, one of the supervisors of Yaari's thesis, which was concerned with optimal consumption plans derived from an infinite horizon, continuous time model. In the Spring of 1963 I accepted an offer to join the Cowles Foundation and the Department of Economics as a Full Professor. Kenneth tried to assemble an analogous appointment involving the Department of Economics at Stanford, but the other members of the department were, I suspect, not quite ready to accept someone with as little formal training in Economics as I possessed. Yaari joined the Cowles Foundation in 1962 and Cass arrived at the same time as I.

Yale and the Cowles Foundation

My teaching responsibilities at Yale were light: I presented a year-long graduate course in Mathematical Economics which was cobbled together during that first year. A number of students audited the course the first time that it was offered, but only a single student, Father Eugene Poirier, was brave enough to take the course for credit. I decided to reverse the customary ordering of topics by presenting the theory of production first, before taking up the discussion of consumer preferences and demand, a mode of organization that I have retained till this day. I feel strongly that production is at the heart of a modern economy, and that the customary emphasis on models of exchange, with production treated casually as an afterthought, provides students with a misleading introduction to economic theory.

So much of economic theory is built upon two extraordinary abstractions neither of which has a tangible representation in our world of everyday experience: the individual utility function and the production function and its generalization—the production possibility set. These two abstractions are crucial for a theory of the determination of market prices and for our ability to examine the consequences of changes in the economic environment. In order to minimize the abstract character of the production possibility set, I would begin my exposition with a study of the transportation problem: that act of production in which a commodity at a specific location is combined with labor, raw materials, some machinery and possibly time, to produce, as an output, the same commodity located elsewhere. The transportation problem, spelled out by Tjalling at a very early stage of his career, has all of the marvellous features of the general activity analysis model of production. Activities can be run at arbitrary non-negative scales, and several activities can be used simultaneously with outputs from one activity possibly appearing as inputs into a second activity. Commodities can be transported from one city to a second, and then from the second to a third, resulting in a net activity in which the intermediary city is not explicitly recorded.

I took the activity analysis model seriously as a pedagogical device to make production possibility sets concrete. Activity analysis models involve numbers, rather than algebraic symbols, to describe inputs and outputs. The resulting production possibility set can be graphed elegantly if there are only two factors of production and a single output, leading to a geometric intuition that permits us to think about general production possibility sets as convex cones in higher dimensional spaces. I must confess to a magical talisman; for 40 years I have drawn precisely the same production possibility set, with precisely the same numbers, whenever I introduce this material for the first time.

If the activity analysis model involves a single output, then the upper surface of the production possibility set is the production function representing the maximum output obtainable from a given vector of inputs. The specific values of the production function can be calculated by solving a linear program. I had been exposed to linear programming by George Dantzig at RAND, and I knew of Tjalling's interest in the transportation problem and the more general activity analysis model. George's paper on the simplex method and Tjalling's discussion of prices that are associated with efficient production plans appear in Cowles Commission Monograph 13 (Koopmans 1951) reporting on a conference on linear programming held at the Cowles Commission in Chicago in June of 1949. The list of attendees at the conference contains many of the leaders in the revolution in economic theory that was led by the Commission in the 1940s: Arrow, Dantzig, Domar, Dresher, Dorfman, Gale, Hildreth, Koopmans, Kuhn, Marschak, Metzler, Morgenstern, Reiter, Samuelson, Scitovsky, Simon, Tucker and Whitin. Most of them were not yet 40 years old.

Tjalling was an elegant writer, concise, lucid and accurate. His introduction to the volume and his paper on the activity analysis model have not lost their freshness more than 60 years later. Tjalling's work was honored in 1975 by his award of the Nobel Prize, jointly with Leonid Kantorovitch. I have written, in a biographical essay on Tjalling (Scarf 1995), about his distress that the committee did not see fit to share the prize with Dantzig as well. I have no reasonable explanation for this lapse; is it possible that George would have been selected if he had been more accommodating towards economists and appreciative of economic theory; if he had used the term 'prices' throughout his work instead of 'simplex multipliers', if he had ever mentioned the terms 'production function' or 'production possibility set'? I doubt it.

In my view, the simplex method was one of the major contributions to applied economics in the twentieth century. I would introduce the simplex method early in my course, work out the details of the proof of finiteness, and use the simplex algorithm to produce constructively those prices which verified that a proposed feasible solution to the linear program was indeed optimal. I would always give a second proof of the existence of these prices using the separating hyperplane theorem and the elements of the theory of convex sets. But I would enjoy stressing the constructive nature of the simplex method to calculate these dual prices in a specific instance and to demonstrate their existence in general.

The actual steps of the simplex method are capable of economic interpretation themselves. Given a proposed feasible solution to the constraints of the linear program, one solves for prices that yield a zero profit for the activities in use. If the remaining activities have a non-positive profit, the proposed solution is optimal; if some of these activities make a positive profit we select one of them and raise its level of use from zero, making compensating changes in the levels of those activities previously used. When one of those levels falls to zero, we obtain a new tentative feasible solution to which the pricing test is reapplied. The production function associated with an activity analysis model is piece-wise linear; the simplex method can be viewed as a systematic search for that facet of the production set that is consistent with the given factor endowment.

In my course, I would follow linear programming with a discussion of non-linear programming, stressing the economic interpretation of the Kuhn-Tucker theorem as an assertion about prices supporting efficient production plans in the production set derived from the objective function and constraints of the non-linear program. I would also demonstrate that these prices were sufficient to verify optimality. As in linear programming the existence theorem makes heavy use of the underlying assumption that the production possibility set is convex. In this realm of discourse no clue is offered for detecting optimality in the absence of convexity.

The Kuhn-Tucker theorem

I am always distressed when economists choose not to present the Kuhn-Tucker theorem as an economic insight about neo-classical production functions but simply as a mathematical tool using the phrasings 'Lagrange multiplier' and 'Lagrangian' instead of 'prices' and 'profits'; these terms should be banished from the vocabulary of economists. Non-linear programming also offers a fine introduction to the theory of competitive markets, under the special assumption of a single consumer. Consider the non-linear program

$$
\begin{array}{rcl}
\max f_0(x) & & \text{subject to} \\
f_1(x) & \leq & b_1 \\
f_2(x) & \leq & b_2 \\
& & \vdots \\
f_m(x) & \leq & b_m \\
x & = & (x_1, \ldots, x_n) \geq 0
\end{array}
$$

with $f_0(x)$ concave and $f_i(x)$ convex for i = 1, … , n. The objective function and constraints define the production possibility set Y consisting of all $y = (y_0, y_1, \dots, y_m)$ with

$$\begin{aligned} y_0 &\leq f_0(x) \\ -y_1 &\geq f_1(x) \\ &\vdots \\ -y_m &\geq f_m(x). \end{aligned}$$

as x ranges over all non-negative vectors in R^n. The convexity assumptions on the functions $f_i(x)$ imply immediately that Y is convex. In this framework, the non-linear program finds that production plan that maximizes output subject to constraints on the endowment of factors.

If the price of output is unity and the price of the i-th factor is $\pi_i \geq 0$, then the profit associated with the production plan $f_0(x), -f_1(x), \dots, f_m(x)$ is

$$f_0(x) - \sum \pi_i f_1(x).$$

Let x^* be the constrained maximum with the specific factor endowment $b_1, \dots, b_m$. Then under mild assumptions the Kuhn-Tucker theorem asserts the existence of a price vector $\pi^* = (\pi_1^*, \dots, \pi_m^*) \geq 0$ with the properties

- x^* maximizes profit $f_0(x) - \sum \pi_1^* f_1(x)$, for *all* $x \geq 0$, and
- $f_i(x^*) \leq b_i$ with $\pi_i^* = 0$ if the inequality is strict.

For simplicity assume that $f_0(x)$ is strictly concave and each $f_i(x)$ is strictly convex for $i > 0$. Let $x(\pi)$ be the unique activity vector that maximizes profit for factor prices π. The demands for the factors of production are then given by

$$\begin{aligned} &f_1(x(\pi)) \\ &f_2(x(\pi)) \\ &\vdots \\ &f_m(x(\pi)). \end{aligned}$$

In this framework, the Kuhn-Tucker prices π^* are equilibrium prices which equate the supply and demand for each factor (or yield a demand less than or equal to the supply if the corresponding price is zero). In this simple instance the existence of equilibrating prices follows from the separating hyperplane theorem; topological fixed point theorems are not required.

The elementary nature of the discussion reveals itself as well in the behavior of the price adjustment mechanism:

$$\frac{d\pi_i}{dt} = f_i(x(\pi)) - b_i,$$

with an obvious modification if a price becomes zero. For it can be easily seen that this mechanism is always globally stable: starting from an arbitrary initial price vector, if none of the prices become zero along the path, the solution $\pi(t)$ will always converge to the correct equilibrium price π^*. The argument is so charming that I must reproduce it here. I have never seen this argument in any economics text.

Let π and π' be two distinct price vectors. Then from the definitions of $x(\pi)$ and $x(\pi')$ we have

$$f_0(x(\pi)) - \sum\pi_i f_i(x(\pi)) > f_0(x(\pi')) - \sum\pi_i f_i(x(\pi')) \text{ and}$$

$$f_0(x(\pi')) - \sum\pi'_i f_i(x(\pi')) < f_0(x(\pi)) - \sum\pi'_i f_i(x(\pi)).$$

Adding these two inequalities together and simplifying, we obtain the following consequence of revealed preference:

$$\sum(\pi_i - \pi'_i)(f_i(x(\pi)) - f_i(x(\pi'))) < 0.$$

But this inequality is sufficient to verify that $\pi(t) \to \pi^*$, at least if $\pi^* > 0$, by demonstrating that

$$1/2\,\frac{d(\sum(\pi_i(t) - \pi_i^*)^2}{dt} < 0.$$

We have

$$1/2\,\frac{d(\sum(\pi_i(t) - \pi_i^*)^2}{dt} = \sum(\pi_i(t) - \pi_i^*)\frac{d\pi(t)}{dt}$$

$$= \sum(\pi_i(t) - \pi_i^*)(f_i(x(\pi) - b_i) < 0$$

if, in the above inequality, we take $\pi' = \pi^*$ and realize that $f_i(x(\pi^*) = b_i$.

In summary everything works well on the production side alone under neo-classical convexity assumptions. Existence theorems require only the separating hyperplane theorem and local adjustment processes are always globally stable. It is the presence of consumers in the Walrasian model that gives rise to the occasional instability of the price adjustment mechanism and forces us to use fixed point theorems to demonstrate the existence of equilibrating prices. In my course I would break the syllabus into two distinct sections based on the required mathematical topics: the first part using elementary notions of convexity and the second involving fixed point theorems. I had constructive algorithms for the first part, but in the second part I had to rely on the non-constructive proof of Brouwer's fixed point theorem based on Sperner's Lemma.

Sperner's Lemma

Sperner's Lemma deals with a simplicial subdivision of the unit simplex of dimension $n - 1$, say, the price simplex

$$\prod = \{\pi = (\pi_1, \pi_2, \ldots, \pi_n) \text{ with } \pi_i \geq 0, \sum\pi_i = 1\}$$

into a large number of subsimplices that fit together nicely. Each vertex v of the subdivision is given a label $l(v)$ from the set $\{1, 2, \ldots, n\}$. The vertices on the boundary of the simplex have some of their coordinates equal to zero; if v is on the boundary of the simplex, its label $l(v)$ is required to be selected from the set of indices $\{i : v_i > 0\}$. Sperner's Lemma then asserts the existence of at least one subsimplex (in fact, an odd number of subsimplices) all of whose vertices are differently labeled as in Figure 6.1.

Suppose that we are given a continuous mapping

$$\pi \to f(\pi) = (f_1(\pi), f_2(\pi), \ldots, f_n(\pi))$$

of the unit simplex into itself. Sperner's Lemma provides an approximate fixed point of the mapping, and, by means of a standard limiting argument, a proof of the existence of a true fixed point $\pi = f(\pi)$. Take a fine simplicial subdivision of the unit simplex. For each vertex v there will be at least one index i for which

$$f_i(v) \leq v_i$$

and we select one of these indices to be the integer label $l(v)$ associated with the vertex v. We can clearly arrange the labeling so that a vertex on the boundary has a label i for which $v_i > 0$, and the conditions of Sperner's Lemma are satisfied. The completely labeled simplex whose existence is asserted by Sperner's Lemma provides us with a simplex with the property that for each coordinate i there is a vertex of the simplex whose i-th coordinate is decreasing weakly under the mapping. But this is all that we need for a good approximation. If the subdivision is very fine, the vertices of the completely labelled simplex will be very close to each other, and any point in the simplex will be changed by a small amount under the mapping.

I knew of Sperner's Lemma and its application to Brouwer's Theorem in 1963; I taught the subject with great care in my graduate course in Mathematical Economics. I would present the standard proof of Sperner's Lemma, which was

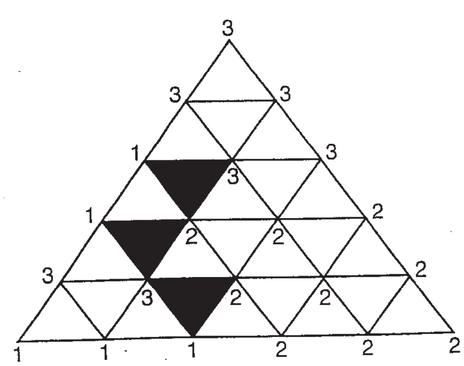

Figure 6.1 Sperner's Lemma.

based on an inductive argument on the dimension of the simplex. It is useful to sketch Sperner's proof, since it has elements that appear in the later simplicial arguments.

Inductive Proof of Sperner's Lemma. Take an $n-2$ dimensional face of Π, say the face Π'' consisting of the vectors in Π whose n-th coordinate is zero. By the assumption of Sperner's Lemma, the vertices on Π'' will all have labels in the set $\{1, 2, \ldots, n-1\}$ and will satisfy the conditions of Sperner's Lemma restricted to that face. Assuming that Sperner's Lemma is correct for a simplex of dimension $n-2$, there will therefore be an odd number of simplices on Π'' bearing the complete set of labels $\{1, 2, \ldots, n-1\}$.

We begin the inductive step by making up a special list, say L, of subsimplicies in Π of dimension $n-2$. We examine each $n-1$ simplex in the subdivision of Π and ask whether the simplex has an $n-2$ dimensional face that happens to bear the labels $\{1, 2, \ldots, n-1\}$. If there is such an $n-2$ dimensional face we include that face in the list L.

Such an $n-2$ dimensional face can only occur if the simplex containing the face is completely labeled or almost completely labeled in the sense that it bears the labels $\{1, 2, \ldots, n-1\}$. In the first case, a single $n-2$ face of the simplex is added to the list L; in the second case there are two such $n-2$ faces, both of which are added to the list. Therefore the size of the list L has the same parity as the number of completely labeled $n-1$ simplices in Π.

But every $n-2$ face in L has been recorded twice if it is an interior edge of the simplex Π, and once if it is on the boundary, and therefore the size of the list has the same parity as the number of completely labeled $n-2$ simplices on the face Π'', which by induction is odd. This completes the proof.

A beautiful argument for the existence of a completely labeled $n-1$ dimensional simplex, but not remotely an algorithm for finding one.

A digression on the core

During my visit to the Cowles Foundation in 1959–60; I agreed to give a talk at Columbia University on my example of an exchange economy with a unique unstable equilibrium. Martin Shubik, a friend from graduate student days at Princeton, was in the audience. After the talk we took a long walk from 125th street to Martin's apartment in Sutton Place, and Martin spoke to me, with great enthusiasm, about a topic that was much on his mind. He told me about a particular cooperative solution to an n person game, *the core*, and about his conjecture that outcomes in the core of an exchange economy would converge to its set of competitive equilibria if the number of agents in the economy tended to infinity.

I was intrigued by this introduction to cooperative game theory. My previous exposure to game theory was exclusively with two person zero-sum games, and I had not realized that von Neuman and Morgenstern's primary preoccupation was the analysis of cooperative solutions to general n person games. I started thinking seriously about the problem; I read *The Theory of Games and Economic Behavior*;

I read Edgeworth's analysis of the contract curve with two goods and two types of consumers in *Mathematical Psychics*, and Martin's own paper on this subject (Shubik 1959). I discussed the problem with Martin and with Lloyd Shapley, another friend from Princeton, and can remember vividly Lloyd's immediate demonstration that a competitive allocation was necessarily in the core.

The initial difficulty was to pose the problem correctly so as to formalize the concept of a large number of agents. I remember toying briefly with a continuum of agents indexed by the points in the unit interval—a setting of the problem subsequently introduced by Robert Aumann. Edgeworth's treatment seemed natural: one would take a finite economy and replicate each consumer precisely the same number of times, with the number of replications ultimately tending to infinity. The replicated competitive equilibrium would be in the core of the replicated game, in the sense that no coalition, involving arbitrary numbers of consumers of each type, would be able to achieve a higher utility for each of its members. The next problem was to find some analytic device that would permit one to argue that competitive equilibria were the only allocations with this property.

The device that I stumbled on appears in the paper, 'An Analysis of Markets with a Large Number of Participants' (Scarf 1962). The paper, presented at the conference 'Recent Advances in Game Theory', held at Princeton in October 1961, does provide a proof of the conjecture that Shubik had presented to me on our long walk. But the argument was extremely intricate, and I would hesitate to use it today in a classroom presentation of the result. A dramatic simplification of the argument was presented in the joint paper with Gerard Debreu, published in 1963 (Debreu and Scarf 1963). I can do no better than to quote Debreu's Nobel Prize lecture in 1983 on the circumstances leading to our collaboration.

> Associated with our joint paper is one of my most vivid memories of the instant when a problem is solved. Scarf, then at Stanford, had met me at the San Francisco Airport in December 1961, and as he was driving to Palo Alto on the freeway, one of us, in one sentence, provided a key to the solution; the other, also in one sentence, immediately provided the other key; and the lock clicked open.
>
> (Debreu 1984: 272)

The relationship between the core and the set of competitive equilibria was intriguing to me for two reasons: first of all the theorem allowed a competitive equilibrium to be fully characterized by the preferences and endowments of coalitions of agents, with no prior assumption about the role of competitive prices. In this sense the core equivalence theorem is similar to the second welfare theorem, but it is much tighter in that it leads to a competitive equilibrium with no income redistribution at all. While the proof of the theorem requires only the elementary machinery of convexity theory, it leads to an outcome whose existence can be established only by the use of sophisticated fixed point arguments. This is in distinction to a Pareto Optimum allocation whose existence follows simply from maximizing a positive linear combination of utilities.

Cooperative games without transferable utility

The treatment of cooperative game theory by von Neuman and Morgenstern was based exclusively on games with transferable utility. In their volume, they associated with each coalition S, a single number v_s, representing the total utility that could be allocated among the members of the coalition. By the 1950s this approach had become much too restrictive for economists working in general equilibrium theory. There are, of course, several interesting cases of equilibrium models in which utility can be thought of as transferable: in a model of exchange in which all consumers have the same utility function $u(x)$, which is non-negative, concave and homogeneous of degree one, the utilities $(u^1, \ldots, u^n)$ associated with the set of Pareto optimal trades are the vectors u with $\sum u^i = u(\omega)$, where ω is the vector of total assets owned by the consumers prior to trade. But in the purely ordinal framework of equilibrium theory, this, and other special cases like it, are useful primarily as examples illustrating features of the more general model.

At some point in 1964, I was innocently toying with some examples and made the observation that a model of exchange involving three consumers with convex preferences, had a certain property that permitted me to assert the existence of a non-empty core by means of an argument that made no use at all of fixed point theorems. The specific property, which had not previously been remarked on by economists, was the following:

> if $u = (u_1, u_2, u_3)$ is a utility vector such that
>
> the coalition (1, 2) can achieve the pair (u_1, u_2) using $\omega^1 + \omega^2$, and
>
> the coalition (1, 3) can achieve the pair (u_1, u_3) using $\omega^1 + \omega^3$, and
>
> the coalition (2, 3) can achieve the pair (u_2, u_3) using $\omega^2 + \omega^3$

then it is an immediate, but not obvious, consequence of the *convexity of preferences* that the coalition of all *three players* has a way of redistributing their collective initial assets $\omega^1 + \omega^2 + \omega^3$ so as to achieve the utility vector $u = (u_1, u_2, u_3)$.

It was easy to argue, simply by tracing the intersection of certain surfaces in 3-*space*, that a superadditive game with this property would always have a non-empty core. I remember remarking on this fact to Lionel McKenzie who encouraged me to continue thinking about its analogue for games with more than three players. I was much excited by the possibility of extending this result to games with many players and obtaining a proof of the existence of a non-empty core for an economy with convex preferences, without inferring its existence from the fact that a competitive equilibrium was necessarily in the core. In conjunction with my theorem with Debreu, this seemed to be a way of completely avoiding the use of fixed point theorems to demonstrate the existence of a competitive equilibrium. It was an example of a marvellously naive agenda which was useful in spite of itself.

Aumann and Peleg (1960) had provided a formal definition of a cooperative game without transferable utility into which this discussion would naturally fit. Let N be the coalition of all players in the game. For each coalition $S \subseteq N$, let E^S be the Euclidean space whose cardinality is equal to the number of players in S, and whose coordinates are indexed by those players. If $u \in E^N$, we represent its projection into E^S by u^S. Then to describe a game without transferable utility, we associate with each coalition S the set of utilities $V(S)$ in E^S that the coalition can achieve by itself. The sets $V(S)$ are assumed to satisfy some minimal conditions: they are closed, bounded from above and allow for free disposal of utility in the sense that if $u \in V(S)$ and $y \in E^S$ with $y \leq u$, then $y \in V(S)$.

A utility vector u is then 'blocked' by a coalition S if its projection u^S is interior to the set $V(S)$. The vector will be in the core if it is feasible for all players acting collectively, $u \in V(N)$, and if is blocked by no coalition.

Using this notation, the previous condition on a three person game that guarantees the existence of a non-empty core can be stated as follows:

if $\quad u = (u_1, u_2, u_3)$ has the property that

$\quad u^s \in V(S)$ for each two person coalition,

then $u \in V(N)$.

This condition, in conjunction with superadditivity and some minor technical assumptions is sufficient to guarantee the existence of a vector in the core of the game. What is the generalization of this theorem to general cooperative games? What replaces the collection of the three two person coalitions for games with more than three players? The answer is given by the concept of a *balanced collection of coalitions*:

Definition 1: Let $T = \{S\}$ be a collection of coalitions. The collection is balanced if $\exists$ weights $\delta_s \geq 0$ such that

$\sum_{S \ni i} \delta_s = 1$, for each player i.

Definition 2: The game defined by the sets $V(S)$ is defined to be balanced if, for every balanced collection $T = \{S\}$, a utility vector

$u = (u_1, u_2, \ldots, u_n)$ is in $V(N)$ if

$u^s \in V(S)$ for every $S \in T$.

Balanced games have a non-empty core

The main result of my paper, 'The Core of an N-Person Game' (Scarf 1967c), is *Theorem 1*, stating that a balanced game has a non-empty core. I knew that this theorem was true by some point in the Spring of 1964; in fact I had worked out a formidable proof based on Brouwer's theorem, which I never did publish. My hope was to provide an argument for the general case, which made no use of fixed point theorems, and could conceivably be converted into a numerical algorithm that would permit me to compute a point in the core.

In order to compute a point in the core of an arbitrary balanced game, I would certainly have to develop an algorithm for games in which each set $V(S)$, with $S \subset N$, consisted of those utilities that could be obtained from a *finite number* of utility vectors by the free disposal of utility. Such games would be as far removed as possible from the class of games with transferable utility, and might therefore require the use of algorithms which were quite different from the simplex method for linear programming. Moreover, a general balanced game could be approximated arbitrarily closely by a balanced game in which $V(S)$ was finitely generated for all proper coalitions S. If I could effectively obtain a point in the core for such finite games, then I would be able to approximate a point in the core of a general balanced game as closely as required.

Such a game would be characterized by a finite list of vectors: each specific $V(S)$ would contribute a finite set of utility vectors lying in the Euclidean space E^S. In order to place these sets in the same space E^n, I filled out the vectors in E^S by adding arbitrary, large numbers for the coordinates corresponding to players not in S. This permitted me to describe the game by a large finite list, say C in the paper, of utility vectors in E^n. In my paper I work with a specific example with the list

$$C = \begin{bmatrix} 0 & M_2 & M_3 & 12 & 3 & 2 & 9 & 5 & 4 & M_{10} & M_{11} & M_{12} \\ M_1 & 0 & M_3 & 6 & 7 & 9 & M_7 & M_8 & M_9 & 5 & 2 & 8 \\ M_1 & M_2 & 0 & M_4 & M_5 & M_6 & 3 & 8 & 10 & 6 & 9 & 4 \end{bmatrix}$$

The algorithmic question was to find a utility vector in $V(N)$ which could not be blocked by any vector in this list. The device of introducing large numbers for the coordinates of players not in S simply translated this search into that of finding a utility vector $v \in V(N)$ such that no $u \in C$ dominated v in the sense that $u > v$. Efficient vectors v in the set obtained from C by allowing free disposal of utility would certainly have this latter property, but how was one to guarantee that such a vector would necessarily lie in $V(N)$?

At this point, I came upon a construction that I have been thinking about in one form or another for more than four decades. I discovered a very natural, canonical way of constructing what mathematicians call a *simplicial complex*: My complex, $C(X)$, was based on a finite set of points X in general position in E^n, each point augmented by the possibility of free disposal. In the paper on balanced games, each such simplex in the simplicial complex was called an '*ordinal basis*'. In the problem of computing equilibrium prices for a general Walrasian model of the economy, the name given to these simplicies was changed to '*primitive sets*', and in my subsequent work on indivisibilities in production, a third terminology '*maximal lattice free bodies*' was introduced to describe precisely the same concept, when the finite set of points is replaced by a lattice. When I first came upon this structure in the context of cooperative game theory, I was, of course, unaware of its possible uses in the analysis of other, quite different areas of inquiry. If I had been prescient, I would have introduced the construction in a more leisurely way and maintained terminological consistency.

This informal history is not the proper place for a detailed discussion of this abstract simplicial complex. An exposition can be found in the video of a talk presented in Jerusalem at the conference 'The Economics of Kenneth J. Arrow' (Scarf 2008).

In each application of this construction: to the core of an n-person game; to the problems of computing equilibrium prices or fixed points of a continuous mapping; or to the description of discrete production possibility sets, the simplicial complex $C(X)$ is used in quite a different fashion. In the game theoretic application, the set X consists of the vectors in $V(S)$ which are enlarged to n dimensional vectors by adding arbitrary large numbers for the coordinates corresponding to players not in S. A simplex in $C(X)$ will consist of a set of n such vectors $u^{j1}, \ldots, u^{jn}$ with the property that

$$u = \min\,[u^{j1}, \ldots, u^{jn}]$$

cannot be blocked by any coalition. Figure 6.2 displays the complex associated with the above example.

Each local minimum on the surface in this drawing defines a simplex in the simplicial complex. The point in the core is the black dot in the centre of the figure.

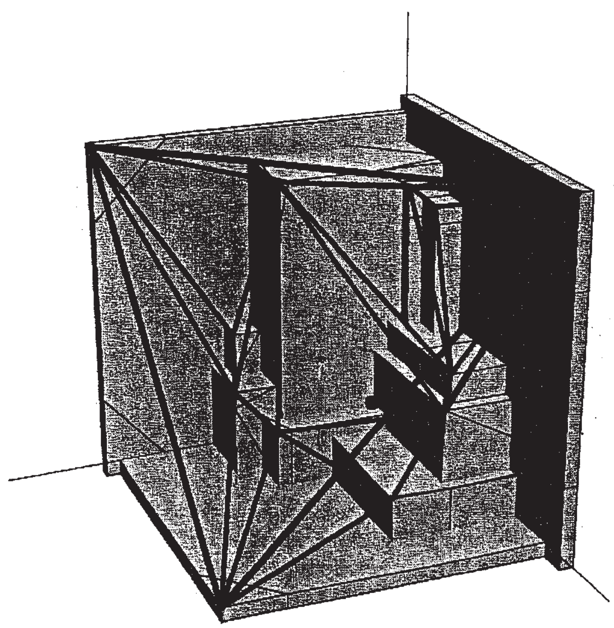

Figure 6.2 The simplifical complex.

The major theorem of the paper is demonstrated by showing that there is at least one such simplex whose corresponding vector u lies in $V(N)$. This will be true if the n vectors $u^{j1}, \ldots, u^{jn}$ are derived from $V(S^{j1}), \ldots, (S^{jn})$, respectively, with the n sets $S^{j1}, \ldots, S^{jn}$ forming a *balanced* collection T. For then the vector u would lie in each such $V(S)$ and from the fact that the game is balanced, the vector u would lie in $V(N)$ as well.

I had reached this point in my analysis by the Spring of 1965. I realized that the existence of a point in the core of a balanced game could be reduced to a purely combinatorial theorem involving two structures: the feasible bases of the incidence matrix of players versus coalitions, and the simplicies (referred to in the paper as 'ordinal bases') in the complex derived from the sets $V(S)$. A generalization of this combinatorial result appears as Theorem 2, and as I remark in Section 4, I could derive Theorem 2 quite readily by a limiting argument from the theorem asserting the existence of a Nash equilibrium for an arbitrary two person non zero-sum game. But as far as I knew, the existence of a Nash equilibrium required precisely those non-constructive fixed point arguments that I had been trying to avoid.

Lemke's House

But I had an enormous stroke of good fortune: Robert Aumann was visiting the Cowles Foundation during the academic year 1964–65, and when I described my problem to him, he suggested that I take a look at a recent paper by Lemke and his student Howson (Lemke and Howson 1964), which provided an algorithm for computing a Nash equilibrium for a finite two person non zero-sum game. In a single evening, I realized that my limiting process would directly translate Lemke's procedure into an algorithm for computing a point in the core of a balanced n person game in which each $V(S)$ was generated by a finite number of utility vectors. The translation of Lemke's algorithm through the limiting process becomes the algorithmic proof of the main theorem in the paper, 'The Core of an N Person Game' (Scarf 1967c).

The major theorem of the paper has been much taken up by computer scientists under the name 'Scarf's Lemma'.

It was a terribly exciting moment for me. I immediately took a course in Fortran programming and wrote my first computer code for an arbitrary game with four players. I can remember submitting a stack of punched cards to the attendant at the Yale computer center, wondering whether the answer would be returned in a minute or two or whether the calculation would require weeks to complete. The program, in spite of its primitive inefficiencies, executed quite rapidly. I realized that I had taken a tentative first step in my project to convert general equilibrium theory from its role in providing existence theorems for the Walrasian model into a practical tool for economic analysis.

Lemke's algorithm has some features in common with the simplex method; a sequence of pivot steps are carried out until the desired solution is reached. But the argument for termination is dramatically different from that used in linear programming. The simplex method terminates finitely because (in the absence of degeneracy) the objective function is strictly increasing from iteration to iteration. The problem being studied by Lemke has no objective function and termination is demonstrated by a topological argument—essentially a homotopy argument—that I call the parable of

> **Lemke's House:** Imagine a house with a finite number of rooms with the property that each room has precisely two doors. Suppose that there is a room one of whose doors leads to the outside of the house. Then there is a second door leading outside.
>
> The second door can be found by the following algorithm, which also demonstrates that the second door exists: Enter the house through the known door. Whenever a room is entered, exit through the other door in the room. Termination follows from the fact that no room is revisited. Why?

The previous section on the Core discusses the sequence of steps that led, in the Spring of 1965, to an algorithm for finding a utility vector in the core of a *balanced n—person* game. The Walrasian model leads to a balanced game if the neoclassical convexity assumptions hold and each coalition has access to the same convex cone as a production possibility set. If the number of consumers is large, a point in the core will be close to a competitive equilibrium and this algorithm is therefore a candidate for calculating an equilibrium price vector. But it does not seem to be an attractive algorithm in practice.

Back to fixed point theorems

In November of 1965; I finally realized that the techniques used to calculate a vector in the core of a balanced game could be carried over directly to approximate fixed points of an arbitrary continuous mapping of a compact convex set into itself. The translation of Lemke's algorithm becomes the algorithm of my paper 'The Approximation of Fixed Points of a Continuous Mapping', which appeared as a Cowles Foundation Discussion Paper in February 1967 (Scarf 1967b).

The innovation involved in approximating fixed points is actually a simplification of the earlier methods. The algorithm used in calculating a vector in the core started with a large, finite set of points X in R^n, and constructed a simplicial complex $C(X)$ whose vertices were the points in X.

In order to approximate the fixed points of a continuous mapping of the unit simplex Δ into itself, we take the points in X to lie on Δ. The simplices in the complex $C(X)$ will also lie on Δ, and in this paper they are given the new title of '*primitive sets*'. As in Sperner's Lemma, we associate with each point $x \in X$ an *integer* label $l(x)$ from the set $\{1, 2, \ldots, n\}$ rather than the *vector* label used in the core algorithm. If the boundary is treated correctly, we have the analog of Sperner's Lemma: there exists at least one simplex whose vertices carry all of the labels. Again, if the labels are appropriately selected from the mapping itself, and the points in X are generously sprinkled throughout the simplex, the completely labelled simplex will provide an approximate fixed point of the mapping.

But, of course, the important feature of this approach is that the completely labelled simplex that approximates the fixed point of a continuous mapping is found by an algorithm rather than having its existence simply asserted in a non-constructive fashion. The paper contains three numerical examples of exchange economies whose equilibrium prices are calculated using this algorithm, augmented by some minor numerical analysis to improve the precision. These were, I suspect, the first examples of general equilibrium models, too large to solve by hand and too complex to solve analytically, that were successfully addressed by a general purpose computer code which was, simultaneously, capable of demonstrating existence. This early algorithm was quite clumsy, but I remark in the paper on the small time that was required to solve all three problems and express my hope that really large models, involving perhaps fifteen or twenty commodities, might ultimately be within the scope of the computational procedure. I still have copies in my files of the original code.

The algorithm can be applied directly to a model of exchange, without using Brouwer's theorem as an intermediary device. The algorithm produces a sequence of small subsimplices on the price simplex Π, that terminates at a subsimplex yielding an approximation to the equilibrium price vector. In this sense the algorithm is always globally stable in contrast to the Walrasian price adjustment mechanism discussed at the beginning of this section. The evolution of the Walrasian path depends simply on the value of excess demand at the current price vector. In contrast, the current state of the simplicial method is given by the n vertices of the current simplex and the value of excess demand at each of these vertices. When the simplex is small, we possess, therefore, information about the excess demands, not at a single price vector, but in an entire full dimensional region. For me, the major insight is that in order to ensure global convergence, an algorithm must be based on something like the Jacobian of the excess demand function as well as the value of excess demand at a point.

An early paper

Irving Fisher, Yale's preeminent economist, was born on 27 February 1867. A celebration was held in the Spring of 1967 to commemorate the 100th anniversary of his birth. Paul Samuelson presented an address entitled, 'Irving Fisher and the Theory of Capital', and nine members of the Department of Economics were asked to prepare papers, bearing on Fisher's work, to be collected, along with Samuelson's address, in a volume entitled *Ten Economic Studies in the Tradition of Irving Fisher*. I took my cue from Irving Fisher's thesis.

Fisher's thesis, submitted in 1891 was remarkable in several respects. It contained a fully articulated presentation of the general equilibrium model, assembled by Fisher without any knowledge of the simultaneous contribution of Walras. But in addition, Fisher constructed an ingenious hydraulic machine that would solve for market clearing prices in a model of equilibrium. Fisher's model had several special features tailored to his construction: there were 3 consumers and 3 commodities, the utility functions were separable and the initial endowments of the consumers were specified in terms of money rather than our customary presentation by the ownership of physical commodities. But in spite of these specializations, Fisher's examples probably require a fixed point theorem to demonstrate existence; they certainly may have multiple equilibria. Fisher used his machine as a teaching device in his classes at Yale. Two variants of the machine were actually constructed, but unfortunately, they have been lost.

My contribution to the volume, 'On the Computation of Equilibrium Prices' (Scarf 1967a), described my recently discovered algorithm for calculating equilibrium prices when production was given by an activity analysis model. The paper contains a numerical example with 6 commodities and 8 activities. The example, which was meant to be vaguely realistic, was solved and then compared to the solution arising when the production set is augmented by an additional activity. The example was meant to suggest to my colleagues, at Yale and elsewhere, that these novel numerical techniques might be useful in assessing the consequences for the economy of a change in the economic environment, or in a major policy variable—to engage in comparative statics when the equilibrium model was too large to solve graphically or by hand.

But it was some time before this suggestion was taken seriously. We were in the 1960s, in the era of large Keynesian macro models in which specific scarce resources and relative prices were not included; there was, in the air, a suggestion that the economy could actually be fine-tuned by prescient economic advisors. The oil shocks of the subsequent decade presented a severe challenge to this way of doing economics.

At the end of the paper I remark that the time required to solve the problems was quite small and that I looked forward to the possibility of solving problems with perhaps 20 variables. In order to discuss subsequent algorithmic improvements, I have to be somewhat specific at this point about some mathematical details. The algorithm works with a large finite set X on the unit price simplex Π, satisfying the non-degeneracy assumption that no two vectors in X have a common i-th coordinate for any i. The set is enlarged by the addition of n special vectors that play the role of the $n-1$ dimensional faces of Π, and the members of X are given integer or vector labels, depending on the particular application. A simplicial complex $C(X)$ is then defined in a canonical fashion, and a path through the simplices in $C(X)$ is followed, using some variation of the basic Lemke algorithm.

The algorithm involves alternating between linear programming pivot steps and replacement operations carried out on the simplices. More specifically, at each iteration we are given a simplex S with n vertices, and a particular vertex x which is to be removed. The remaining vertices of S define an $n-2$ dimensional face of S; we then move to the unique other simplex S' that shares this face with S.

This mathematical step has to be carried out over and over again in the course of the numerical calculation, and for numerical efficiency it must not be too difficult to find the adjacent simplex S'. In the first application of these ideas—to finding a point in the core—the set X was defined by the characteristic function of the game, and the replacement required a complete search over the members of the set X. In applications to the calculation of economic equilibria, the points on the price simplex had no intrinsic qualities other than being sprinkled quite regularly throughout the simplex. The most natural choice for X was the set of all rational vectors on the price simplex with a fixed, large denominator, which could ultimately be made larger if we wished to improve the accuracy of the calculation. But this set of points violates the non-degeneracy assumption which was required for the definition of the simplicial complex. This difficulty needs a resolution which is not too complex; the two papers referred to above use a resolution which requires a search through the prices that have examined, but not the remaining price vectors. It is not an elegant or efficient way to proceed.

Terje Hansen

In 1966–67 my class was attended by a gifted young Norwegian graduate student named Terje Hansen with whom I was to have a close working relationship during the next several years. Terje became very involved in the topics being discussed in the course and began to think of improvements and modifications of the material. In March of 1967 he published a Cowles Foundation Discussion Paper, entitled 'A Note on the Limit of the Core of an Exchange Economy' (Hansen 1967), in which he demonstrated that the allocations, in a replicated economy, that could not be improved upon by the small number of coalitions consisting of all consumers of type i and all but one of the other types, for each i, would converge to a competitive equilibrium.

Terje then made a very dramatic contribution to the design of fixed point computational methods. I was planning to visit Bob Aumann in Israel in May and early June of 1967, and before I left, Terje asked me for some computer time to try out an idea that would remedy the particular flaw in the algorithms that I have just described. I suggested that he work out the theoretical details of his improvement first and I would then provide him with the necessary time on the computer. When I returned, Terje greeted me with a glowing smile and informed me that he had been successful. What had Terje done?

Terje proposed that the algorithm work with a system of matrices of a special form. For each $1 \leq j \leq n$ let

$$e^j = \begin{matrix} 0 \\ \vdots \\ 1 \\ -1 \\ \vdots \\ 0 \end{matrix},$$

with 1 in row j, -1 in row $j+1$ mod (n) and zeros elsewhere. Then each of Terje's matrices

$$C = (c^1, c^2, \ldots, c^n)$$

is defined by its first column c^1, the base column, and a permutation ϕ of the integers $1, 2, \ldots, n$ such that

$$c^{j+1} = c^j + e^{\phi(j)}$$

with $c^{j+1} = c^1$ if $j = n$. For example, if $n = 4$; $c^1 = (3, 5, 1, 7)'$ and $\phi = 3, 4, 1, 2$ then

$$C = \begin{bmatrix} 3 & 3 & 2 & 2 \\ 5 & 5 & 5 & 6 \\ 1 & 2 & 2 & 1 \\ 7 & 6 & 7 & 7 \end{bmatrix}$$

The collection of matrices has precisely the right features for fixed point computations:

- The column sums are identical, so that the columns, divided by their common sums, lie on the unit simplex,
- If column c^j is removed, its unique replacement is given by the matrix $(c^1, \ldots, c^{j-1}, c^{j-1} + c^{j+1} - c^j, \ldots, c^n)$ with the indices taken mod(n). The new matrix has the same base column c^1 and the permutation in which $\phi(j)$ and $\phi(j+1)$ are interchanged.
- If the entries in C are ≥ 0, then so are the entries in its replacement unless a row of C has the form $|(0, 0, \ldots, 1, 0, \ldots, 0)|$ and we are removing the column associated with the 1, that is, we are trying to move out of the price simplex.

Each column is given an integer or vector label, the boundary is properly attended to, and Lemke's algorithm is used to provide a completely labeled matrix. Terje's innovation worked perfectly: it permitted a rapid movement through the collection of matrices without a complex search. In my paper, 'An Example of an Algorithm for Calculating General Equilibrium Prices', published in the *American Economic Review* in 1969 (Scarf 1969), I used Terje's variant with vector labeling, and, of course, I continued to use his algorithm in all future lectures and applications.

Terje's thesis, 'On the Approximation of a Competitive Equilibrium' was presented to Yale in the Spring of 1968. Tjalling was also an advisor on the thesis and, as usual, he took his responsibilities seriously. I have many letters from Tjalling to Terje with detailed suggestions about mathematical arguments and the general presentation of the thesis. Tjalling and Terje subsequently collaborated on a research project applying fixed point methods to computing a stationary equilibrium of a disaggregated optimal growth model. Their results appeared in the paper, 'On the Definition and Computation of a Capital Stock Invariant Under Optimization', published in the *Journal of Economic Theory* in 1972. Terje returned to Norway in April of 1968. By the end of that year we had accumulated a variety of applications of our basic methods, and we had to give some thought to a suitable form of publication. In April of 1969, we published a Cowles Foundation Discussion Paper entitled 'On the Applications of a Recent Combinatorial Algorithm' (Scarf and Hansen 1969), that contained a discussion of Brouwer's Theorem, Kakutani's Theorem, a fixed point theorem originally suggested by David Gale, the calculation of equilibrium prices when production is described by an activity analysis model and an application to concave programming suggested by Terje.

In the Fall of 1969, my wife, three daughters and I embarked on a sabbatical year at Cambridge University. Our host was Frank Hahn, who had arranged for us to be guests at Churchill College. We saw much of Frank and other Churchill Fellows: Roy Radner, Joe Stiglitz, the distinguished humanist George Steiner and the poet Octavio Paz. I remember, with great pleasure, sitting in on lectures by James Meade, and meeting Nicky Kaldor, Joan Robinson and Richard Stone. We frequently visited Ronald and Betsy Dworkin at Oxford, and dined with James Mirrlees and his wife. I have a lovely photograph of Kenneth lunching with us in the court behind our flat.

I talked to Frank Hahn about the best mode of publication of our research and he was insistent that a scholarly monograph was preferred to publication in an academic journal. And so I began work on the Cowles Foundation Monograph, *The Computation of Economic Equilibria* (Scarf and Hansen 1973), which took several years to compose, revise, organize and shepherd through the publication process; it appeared in 1973; with Terje listed as a junior author. I recommend the monograph; it contains many mathematical results and numerical examples which cannot be found elsewhere.

As time went on, Terje's interests moved elsewhere. He became an economic analyst and commentator on issues of public policy in Norway and elsewhere. Terje and I had lunch together in New Haven several years ago. He told me about the details of his life and professional career and described his many publications. I suggested that I might send him an email message about an issue that we were discussing. But Terje said, 'No, I never use email. In fact I never use a computer at all. I write all of my books and articles by hand. I gave up the computer completely after writing my thesis.'

Daniel Cohen's argument

In January of 1968, I received a letter from Bob Aumann telling me about a paper by Daniel I. A. Cohen entitled 'On the Sperner Lemma' that was published in the *Journal of Combinatorial Theory* in June of 1967 (Cohen 1967). In this brief note, Cohen presents an inductive argument for Sperner's Lemma that uses the Lemke sequence of almost completely labelled simplices at a crucial point. Here is a sketch of Cohen's argument:

Cohen's Inductive Proof of Sperner's Lemma: The first step is as before. We take an $n-2$ dimensional face of the simplex Π, say the face Π'' consisting of the vectors in Π whose n-th coordinate is zero, and remark on the fact that, by induction, there will be an odd number of simplices, with $n-1$ vertices, on that face bearing the complete set of labels $\{1, \ldots, n-1\}$.

Take one of these simplices with vertices, say, $v^1, \ldots, v^{n-1}$. It will be the $n-2$ dimensional face of a unique simplex in Π, with vertices $v^1, v^2, \ldots, v^n$. If v^n

bears the label n then we have found a completely labeled simplex in Π; if not we have an almost completely labeled simplex which initiates a path of almost completely labeled simplices. Cohen presents the argument, identical to Lemke's, that no simplex is revisited and therefore we terminate with a completely labeled simplex or return to the same face Π^n, with a different simplex from $\{v^1, \ldots, v^{n-1}\}$.

Each completely labelled simplex on the face Π^n is therefore paired with another such simplex on the same face, or it initiates a path leading to a completely labelled full dimensional simplex in Π. Since there is an odd number of completely labelled boundary simplices, there must be at least one path leading to a full dimensional completely labelled simplex in Π—in fact an odd number of such paths. This completes the inductive argument.

Cohen's argument is presented for a general simplicial subdivision. If a special subdivision were used, with the ease of replacement possessed by Terje's construction, the path used by Cohen could be traversed rapidly, starting from an arbitrary completely labelled simplex on the boundary face. But such a path might return to the boundary face and we would need another completely labeled boundary simplex to begin a new path. This is computationally unacceptable since in the worst case we must find, inductively, *all* of the completely labeled simplices on the boundary.

The best way to initiate the algorithm

Let me illustrate the procedure with the 2 dimensional simplex previously drawn.

Begin by adding 3 additional vertices, with their own labels, and new triangles connecting these new vertices to the boundary intervals of the previous simplex as shown in Figure 6.3. These new vertices are labeled so that no new completely labelled triangles have been introduced. We start with an almost completely labelled simplex on the boundary of the enlarged simplex and follow the sequence of simplicies until a completely labeled simplex is reached.

Homotopy methods

A dramatic set of improvements in fixed point methods was made by Curtis Eaves in a series of papers published in the late 1960s and the early 1970s. I met Curtis in the Fall of 1971 when he presented a lecture in the Department of Administrative Sciences at Yale. We met again at a workshop in fixed point computations held at Dartmouth College in the summer of 1972 that was also attended by Terje Hansen and two young graduate students from the Department of Economics at Yale, John Shoven and John Whalley, who played an extremely important rôle in applying these computational techniques to problems of serious economic significance, along with Tim Kehoe, Jaime Serra Puche, Marcus Miller, John Spencer and a number of other remarkable graduate students at Yale.

Curtis visited the Cowles Foundation for the academic year 1974–75. Curtis has a supple and creative geometrical sense. Concepts and mathematical formulations that were somewhat cramped and formal for me appear in a much clearer geometric

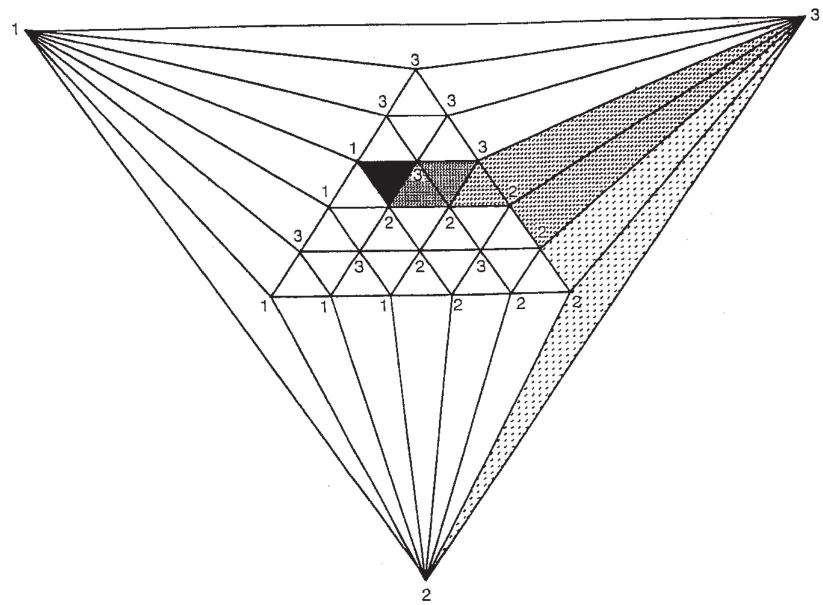

Figure 6.3 The augmented simplex.

setting for Curtis. In one of his papers, for example, Curtis talks about a piece-wise linear mapping of a convex polyhedron into itself. The regions of linearity are themselves convex polyhedra; in each such region the mapping is fully defined by its action on the vertices. This is a geometric rendition of my more formal notion of vector labelling.

Curtis also offered his own *homotopy* method in order to obtain increased accuracy without fully restarting the fixed point algorithm. Define a cylinder $\Pi \times [0,1]$ as the product of the underlying simplex Π, and a homotopy interval $[0,1]$. The mapping of concern, $x \rightarrow f(x)$, is placed on the face with parameter 0, and an elementary mapping with a single known fixed point is constructed on the face with parameter 1. The cylinder is subdivided, and the mappings on the two faces are extended to a piecewise linear mapping of the entire cylinder into the unit simplex. For each value of the homotopy parameter $0 \leq t \leq 1$, the corresponding mapping, $x \rightarrow f(t, x)$, will have its own set of fixed points. The computational procedure proposed by Curtis is to follow the piecewise linear path of fixed points from the upper face until it reaches the bottom face with a fixed point of the original mapping.

Our paper, 'The Solution of Systems of Piecewise Linear Equations' (Eaves and Scarf 1976), is a blending of Curtis' homotopy approach and an elementary notion of an *index* associated with simplicial fixed point methods. I remember remarking to Curtis that I knew which of the two possible directions to take if I were presented with an almost completely labeled simplex in the middle of a path of such simplices. Based on the sign of a particular permutation I could avoid moving in the direction that returned to the original starting position of the algorithm. Curtis immediately realized that this observation was capable of the far reaching generalization presented in our paper. For me, this joint work with Curtis completed a decade long evolution of the early simplicial methods for computing a vector in the core of a balanced game to their final form as homotopy algorithms that follow the solutions of a system of piecewise linear equations.

Applied General Equilibrium Analysis

My good friend and colleague of many years, T. N. Srinivasan, organized a conference entitled Frontiers in Applied General Equilibrium Analysis, which was held at Yale University in April, 2002. The conference was attended by many of my former students and a goodly number of scholars involved in Applied General Equilibrium Analysis in one way or another. It was suggested, at various times during the conference, that my algorithm for computing equilibrium prices had initiated this active and important field. The papers presented at the conference were collected in a volume edited by Timothy J. Kehoe, T. N. Srinivasan and John Whalley, which was published in 2005 (Kehoe, Srinivasan and Whalley 2005). I quote from the introduction to the volume written by the editors:

> As applications of AGE modeling progressed, they were taken up by governments and international organizations around the world. The World Bank, the World Trade Organization, the International Monetary Fund and government agencies in the United States, Australia, Canada, Mexico, the United Kingdom, the Netherlands, and many other countries all had general equilibrium models. The field of AGE modeling as an operational tool in government and policy circles was launched.
>
> (Kehoe, Srinivasan and Whalley 2005: 5)

The dramatic increase in computational power during the last four decades has made it possible to replace the original Scarf algorithm, based ultimately on Sperner's Lemma, by a variety of alternative techniques that perform effectively on problems of substantial size. Computational ability is no longer a constraint in solving large examples. As Kenneth Arrow states in his paper in this same volume:

> What the Scarf algorithm did was to give a license to develop CGE models according to economic logic and empirical validity. The author did not have to modify the model to consider tractability or computability. Once completed, the model might be solved in some way that, if it worked, would be less demanding computationally. But the author or authors knew that there was a way that would always work and that, with steadily and rapidly increasing computational power, would likely be practically feasible. It was

> this knowledge that generated the subsequent explosion in applied general equilibrium models.
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> (Arrow 2005: 22)

I met Professor Sperner at a conference on fixed point computations held at the University of Southhampton in July of 1979: I knew that he would be at the conference, and after I alighted from the train, I approached an elderly gentleman who was standing on the platform and asked whether he was Professor Sperner. I introduced myself and during the long ride to the University, we discussed the consequences of the result that had appeared in his thesis, written some 50 years earlier. He had never worked in applied mathematics himself, but he was, I believe, fully enjoying the widespread consequences of his early research.

Note

1 In this paragraph, and several others, I have modified some material that has been previously published.

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