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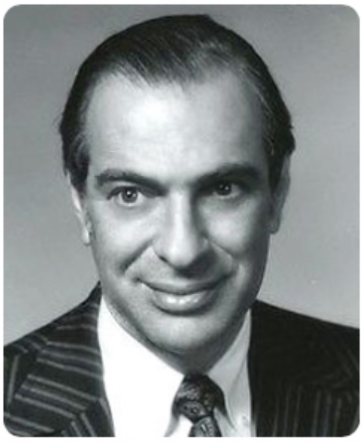
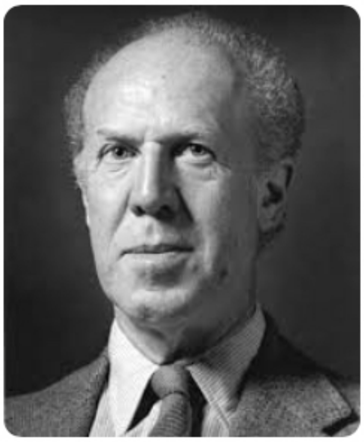

Kenneth Arrow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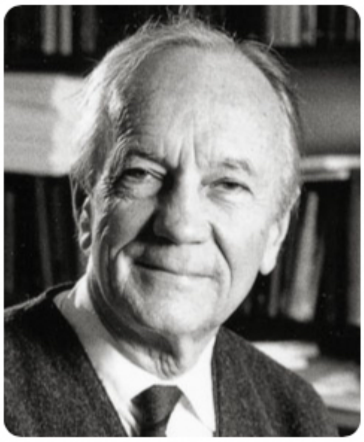
Lionel McKenzie

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Birgit GRODAL, University of Copenhagen, Frank HAHN, University of Cambridge, Werner HILDENBRAND, Universität Bonn, Heracles POLEMARCHAKIS, CORE, Université Catholique de Louvain (organizers)

PROGRAMME

June 3, 1993

Edmond MALINVAUD, Collège de France, Anniversary Lecture.

June 4, 1993

Abstract Economies

Panel members :

Donald BROWN, Stanford University
Bernard CORNET, CERSEM, Université de Paris I
Andreu MAS -COLELL, Harvard University
Wayne SHAFFER, University of Illinois
Karl VIND, University of Copenhagen

Time and Uncertainty

Panel members :

William BROCK, University of Chicago
David CASS, University of Pennsylvania
Chi-fu HUANG, Massachusetts Institute of Technology
Timothy KEHOE, University of Minnesota
Heracles POLEMARCHAKIS, CORE, Université Catholique de Louvain

Strategic Behavior

Panel members :

Claude d'ASPREMONT, CORE, Université Catholique de Louvain
Jean Jaskold GABSZEWICZ, CORE, Université Catholique de Louvain
Birgit GRODAL, University of Copenhagen
Eric MASKIN, Harvard University

Jean-François MERTENS, CORE, Université Catholique de Louvain
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Panel members :

Beth ALLEN, University of Minnesota
Françoise FORGES, CORE, Université Catholique de Louvain
Roger GUESNERIE, Ecole des Hautes Etudes en Sciences Sociales, Paris
Robert TOWNSEND, University of Chicago
Yves YOUNES, CEPREMAP, Paris

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Panel members :

Yves BALASKO, Université de Genève
Egbert DIERKER, Universität Wien
Jean-Michel GRANDMONT, CEPREMAP, Paris
Werner HILDENBRAND, Universität Bonn
Edward PRESCOTT, University of Minnesota

Money and Macroeconomics

Panel members :

Jean-Pascal BENASSY, CEPREMAP, Paris
Truman BEWLEY, Cowles Foundation, Yale University
Jacques DRÈZE, CORE, Université Catholique de Louvain
Douglas GALE, Boston University
John GEANAKOPLOS, Cowles Foundation, Yale University
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Robert LUCAS, University of Chicago

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Anniversary Lecture

Existence proofs of general equilibrium
Looking forty years back

Edmond MALINVAUD, Collège de France

In this anniversary lecture at the eve of our conference, I must remind you how great was the achievement made forty years ago and how lucky were those who, in December 1952 at the American meeting of the Econometric Society, could listen to Kenneth Arrow, Gérard Debreu and Lionel McKenzie. For this purpose I must recall the history that led to the achievement. I must also stress the importance for our discipline of having rigorous existence proofs of equilibrium in the formal models that give foundations to our main theories.

But it also belongs to me to do a little more. The aim of the Conference, we were instructed, is to "assess the accomplishments as well as failures of general equilibrium theory during [the past forty] years and the challenges ahead." We shall have six panels, each one dealing with a particular theme raised by our subject. Something might be missing if we should not also consider the whole subject from a distance. There were undoubtedly accomplishments, perhaps even failures, of a more general nature than those that are likely to spontaneously appear in the discussions of the various panels. Such accomplishments or failures ought to be brought to light at this early stage of our meeting, so as to induce the various panelists to speak also on them. Even my oversights or mistakes will stimulate others who will want to correct me.

I. History

It is common in history to find that different people give different accounts of the facts and this is often where the fun begins. Such is not the case with the history of the existence problem of general competitive equilibrium before 1952. It has been told repeatedly, with no significant contradictions between authors; the first reference I know of consists of a little more than a page in the introduction written by Koopmans (1951) for the famous activity analysis book; the most detailed reference probably is the long article of Weintraub (1983). The story is so well known among specialists that one wonders what to say on this occasion. The content of my text will be just a selection intended to recall the most significant steps toward the achievement of 1952.[1]

Debreu (1987) writes "Walras and his successors were aware that his theory would be vacuous in the absence of an argument supporting the existence of its central concept. But for more than half a century that argument went no further than counting equations and unknowns and finding them to be equal in number." Were Walras and his successors aware that the argument was not sufficient for a mathematician ? Perhaps a close study of their correspondence may reveal something in this respect. But, even if they knew that more was needed for complete rigor, they were not much disturbed by it since they do not mention it in their main works. For instance in "Economie mathématique" Pareto (1911) simply writes at the crucial step: "Les équations sont en même nombre que les inconnues, et le problème est bien déterminé" (p. 627).

1. The 1930's

The difficulty truly emerged after G. Cassel had published in 1918 in German (English translation in 1923) a theory of general equilibrium based on a simplification of Walras'system, i.e. on a mathematical model that was easy to present and discuss. There were two kinds of goods: factors directly used for production with fixed technical coefficients and products that had to meet the demand functions coming from consumers. As Arrow and Hahn (1971) note, that is "an interesting case of work that had no significance in itself, but whose study turned out to be extraordinary fruitful." Reflecting on Cassel's model in the early 30's, three authors, Zeuthen, Neisser and von

[1]References will not be given to the works of all those who will be quoted. A complete bibliography can be found in Weintraub (1983).

Stackelberg, working respectively in Copenhagen, Kiel and Cologne, showed in different ways that the problem of a meaningful equilibrium was deeper than equality of equations and unknowns.

The problem was perceived and discussed in Vienna at the seminar of the mathematician Karl Menger. It was suggested to a young Rumanian refugee, Abraham Wald. On March 19, 1934, Wald presented an existence proof for the equilibrium of Cassel's model in which each demand law determined the price of a product from the production of this single product. On November 6, 1934, Wald relaxed the last feature, proving existence when demand conditions determine the prices of the various products from the productions of all products. About a year later Wald gave a proof of existence of equilibrium in Walras exchange economy, assuming that each individual's marginal utility of a good was independent of the quantities consumed of other goods; but the paper was never published and the manuscript was lost; what remains is a long exposition of the model and its exact hypotheses in a survey article published by Wald in 1936 on his existence proofs; preparation of the survey followed a suggestion made by Morgenstern.

Quoting Arrow and Hahn again: "Wald's papers were of forbidding mathematical depth, not only in the use of sophisticated tools, but also in the complexity of the argument. As they gradually came to be known among mathematical economists, they probably served as much to inhibit further research by their difficulty as to stimulate it." I suppose Kenneth Arrow and Lionel McKenzie can both testify here about this last comment. Lionel attended a course given by Morgenstern at Princeton in 1940-41 on Hicks' *Value and Capital* and then heard about Wald's results, without apparently thinking he would later work on existence proofs. He could then also hear von Neumann presenting his growth model, but did not understand it at the time according to his own later admission. Kenneth was still more explicit in a letter to Weintraub: some time in 1940-42 he read Wald's papers but he wrote in 1982: "the complexity of the argument ... really put me off. I did not believe I was the one capable of really improving on the results."

2. The theory of games

Thus, during most of the 40's no progress seemed to occur and most mathematical economists probably thought the problem of finding general and transparent existence proofs of the competitive equilibrium was intractable. New light was, however, going to come from the theory of games movement. While Cassel's model had been instrumental in showing the nature of the existence problem, the minimax solution to the two person zero sum game would eventually lead people to realize that fixed point theorems were providing a powerful instrument for existence proofs.

In 1924 the French mathematician Emile Borel had shown that minimax was the rational solution concept for two person zero sum games; not all such games had a solution in pure strategies; but Borel conjectured the existence of a solution in mixed strategies (with finitely many pure strategies). One had then to consider two dual systems of linear inequalities between variables, which were constrained to be non-negative. In 1928 von Neumann gave the existence proof, using Brouwer fixed point theorem. Somehow, about at the same time, von Neumann, who was working in Berlin, became interested in linear economic systems. In 1932 after his move to the US, he presented there his model of an expanding economy, which had a good deal of formal similarity with his game model. The final version of the paper eventually appeared in 1937 in the proceedings of Menger seminar; its existence proof used a generalization of Brouwer fixed point theorem, a generalization that was a forerunner of Kakutani's theorem. I shall not comment more on this now well known von Neumann growth model, except to remind you that it was commonly mentioned in the 40's in association with Wald's papers when reference was made to research on existence of economic equilibrium, but again with little hope that it could be useful for proving existence in classical economic specifications.

From our present viewpoint it is still more relevant to refer to the short paper that was written by John Nash in 1949 in order to generalize to n-person games the minimax solution. Nash then gave the definition of what is now known as the Nash equilibrium of non-cooperative games. He used Kakutani fixed point theorem in order to prove that all n-person games have an equilibrium. For imaginative economists this contribution could not but give the idea of a natural approach to the existence problem of general equilibrium: there was some distance to cover in order to go from an n-person non-cooperative game to an $(n + m)$-agent competitive economy, but the route seemed to be open. At least this is how I may venture today to build the history in retrospect, ... with a fair degree of inaccuracy.

To be more respectful of the facts, I must indeed acknowledge the role of activity analysis, which was booming in the late 40's. Activity analysis promoted the mathematical tools that were going to become so familiar and so useful to economists interested in general equilibrium: point sets in general, convex sets in particular.

3. The heroes

Lionel McKenzie had been studying at Princeton in 1940 under Frank Graham. The latter was approaching the theory of specialization in international trade by using a linear model in which each country had access to a set of technologies suited to its own primary factors. Treatment of this model was raising a number of issues that were not solved at the time. When in 1949 Lionel learned about activity analysis, he immediately realized that Graham's model was built exactly with this approach, which was then appropriate for a new look at the unsolved issues. He then decided to spend the year 1950-51 at the Cowles Commission; there he began thinking about proving existence of a competitive equilibrium in Graham's model. The idea of the proof came a little later: considering the space of final output vectors and the set of feasible such vectors, one could first start from a price vector and look at the direction of the final demand vector resulting from this price vector; one could second find the new price vector, or set of vectors, supporting the extreme point of the feasible set in the direction defined by demand. These two successive operations resulted in a mapping on the price simplex. A competitive equilibrium was associated with any fixed point of this mapping. Since Lionel knew

Kakutani's theorem from his stay at Cowles, he could complete the proof and present it at the December 1952 meeting of the Econometric Society in Chicago, McKenzie (1954).

In the fall of 1951, Kenneth Arrow in Stanford and Gérard Debreu in Chicago were independently thinking that the way open by Nash's short paper could lead to a proof of existence of competitive equilibrium. For this purpose it seems that Kenneth was considering the definition of a game in which artificial agents would play together with producers and consumers, whereas Gérard was aiming at a generalization of Nash's result to cases in which the strategy domain of each player is affected by the strategies chosen by other players. In any case it was not long before they became aware of their common interest and joined forces; indeed Kenneth had kept close contact by correspondence with the Cowles Commission group. Gérard succeeded in generalizing Nash's result from a non-cooperative game to what he called a "social system" or an "abstract economy", Debreu (1952). Kenneth and Gérard worked out a precise model of the Walrasian system, a model that is too well known for me to present it today. Existence of equilibrium was then proved by showing that the Walrasian system was an abstract economy with the properties required for existence of equilibrium. Those were the results also presented at the December 1952 meeting of the Econometric Society, Arrow and Debreu (1954).

I shall stop the history at this high point of December 1952. On other occasions than this one, it might appear unfair to others, in particular to those who also proved existence results and published them shortly after 1954; it might also appear to result in an incomplete account of what our three heroes did on the existence problem, since they further worked on it in subsequent years. But this is the anniversary lecture and moreover I must move on to assess the accomplishment.

II. Role of existence proofs

I do not see any other way of doing so than trying to play the devil's advocate and to show that existence proofs are not so important after all. The devil's advocate will be defeated, but you will have to listen to his arguments. They will be spelt out in two stages. First, exploratory theory is more crucial than the mathematical consolidation of the main models. Second, in the consolidation work, existence proofs have no precedence.

1. Exploration and consolidation

When a more rigorous approach to theory spread on economic literature and on the teaching of economics, it happened to me to be somewhat irritated by what I perceived to be an undue emphasis on existence proofs: the author or the student was defining a model and spending most of his efforts in showing how he could apply Kakutani's theorem; but the objective, relevance and content of the model was not enough discussed, according to my taste.

We must indeed recognize that, in our search for explanation of the real world or for solution of economic challenges, we do not know from the start the list of features that could conceivably be related to the point at issue; even if we knew, we should have to decide which features have to be considered and which can be neglected. We do not know exactly from the start how the main features are interrelated; even if we knew, we should have to wonder how the interrelations may work and what kind of answer they may provide to the questions raised. In other words before we have a well defined model on which to concentrate our efforts, there are many preliminaries. The heuristic phase of searching for regularities and for appropriate hypotheses, of feeling around for interesting theoretical models may extend over many years before scientists settle down to concentrate on the study of a particular mathematical system.

In the late stages of this heuristic phase, a few alternative models, or even a single model, seem to emerge. But rushing to the study of their, or its, general formal properties is still premature. Before doing so, one has to wonder further why the solutions of the models are likely to be relevant. While already entering the mathematical specification of the theory, one has to discuss why equilibria proposed by this specification are likely representations of what is to emerge in reality, and how the comparative statics of equilibria is likely to work.

During this exploration it is appropriate to proceed to a complete analysis of some simple examples of the models or model under consideration. Each example of course has special features; results found with it may be quite particular. But looking carefully at an example one gets a better understanding of the structure of the theory. It also happens that one stumbles over a serious difficulty that one would have discarded in a purely formal general treatment, then making a convenient hypothesis whose limitation would have been overlooked.

Does it make sense for me to spell out all these warnings on the special occasion of this talk ? Is the devil's advocate now making a good point ? Certainly not. The theories for which existence of equilibrium was proved in 1952 had previously been well explored. The time of consolidation had come.

The theory of specialization in international trade and of equilibrium prices on international and national markets had been widely discussed. Starting with an article published in 1923, Frank Graham had progressively devised a multi-country multi-commodity model that was suited for dealing with national technologies, national productive capacities, national resources and national demand functions. This model, explored at length by Graham and his students, eventually presented in a book, Graham (1949), then provided the best formal instrument for international trade theory. The time had come when it was a pressing question to know whether the general model had an equilibrium. Lionel McKenzie gave the answer just at the right time.

Similarly, the model of a competitive economy had first been formulated by Walras and widely studied for three quarters of a century. The problem of existence of equilibrium had been posed around 1930, as we saw. It was certainly not premature for Kenneth Arrow and Gérard Debreu to improve on the proofs provided by Wald; they did so in two ways: first, they dealt with an integrated model of production, exchange and consumption; second, they greatly simplified the structure of the proof, making it transparent and convincing. As a premium they also provided the first set-theoretic formulation of the full competitive economy; they did it so well that we all now speak of the Arrow-Debreu economy.

2. The existence problem in the consolidation work

"All right," will admit the devil's advocate; but he will add: "Listen further, the existence problem has no precedence in the consolidation work. So, the achievement reported in December 1952 had no more value than many others, reported before or after in the long series of meetings of the Econometric Society."

About the general competitive equilibrium, other well recognized properties to be looked for are unicity, at least local unicity, and continuity with respect to variations in the exogenous elements of the model. Still more important is stability of equilibrium because, when looking for it, one has to specify how the equilibrium is meant to be reached, a specification that ought to be part of the theory. Moreover, the existence problem had not been neglected by Walras and his proximate followers; they had applied to the mathematical system the counting test: the systems had just as many equations as unknowns. The proofs of 1952 were just adding extra rigor in the treatment of the problem.

The devil's advocate is now calling on a few witnesses in order to claim that, according to the modern wisdom, the importance of postwar existence proofs was indeed overrated. The first witness says: "The proof of existence or non-existence of an equilibrium forms just one theorem among others" (quotation from Balasko, 1988, p.22; my translation). The second witness adds: "[The counting test] was good heuristic, and the theory of regular economies can be viewed as a rigorous foundation and elaboration of this [test]" (quotation from Mas-Colell, 1985, p.175). The third witness is Kenneth Arrow who writes: "It is fortunate for the development of existence theorems for general equilibrium that differential topology was unknown in the early 1950's. If the tools had been available to us, we would simply have written down a few appropriate ... conditions [implying transversality] and then said that Walras was really right all the time", Arrow (1991, p.12).

Who is going to dare challenging these three witnesses ? Gérard Debreu, of course. But you should not expect him to embark into a long pleading. His statements will be as concise as possible. First statement: "[General equilibrium] theory would be vacuous in the absence of an argument supporting the existence of its central concept", Debreu (1987, p. 216). Vacuous! Some of our enemies might comment that economists like to indulge in the discussion of vacuous theories. But we do not want to be rightly exposed to the criticism. The first thing to do in the rigorous consolidation of a theory is indeed to establish, as well as we can, that it is not vacuous.

We know cases in which existence of equilibrium does not hold in general unless one restricts the model more than one would have liked, and in which the theory becomes problematic precisely for this reason. Such a case occurs for the sequence of temporary equilibria of a non-monetary economy with adaptive expectations, because of problems about the survival of indebted consumers. Another case occurs for the temporary equilibrium of a monetary economy with liquidity constraints in Grandmont (1983) if price expectation functions are unbounded (section 1.4). Serious difficulties are then revealed, which had escaped attention and go beyond singularities.

About the existence theorem for regular economies, Debreu writes: "The significance of this theorem rests on the fact that under its assumptions almost every economy is regular", Debreu (1987, p.218). Under its assumptions! Debreu does not say more, so far as I know. It is up to us to decide whether we think the assumptions are convincing enough for having made a more general theorem useless, even simply in the first stage of the consolidation work. As for me I do not believe, even today, the assumptions are convincing enough for challenging the value of the 1952 proofs. Remembering the prevailing concerns at the time of activity analysis, I find still more difficult to believe that the assumptions would have been found satisfactory in the early 50's. This is why I tend to read the quotation of Kenneth Arrow as a joke for initiates.

I should not be misunderstood. I consider the theory of regular economies as a major achievement, which provides a strong foundation for many questions raised about the general competitive equilibrium; but not so much so about its universal existence. As a side comment, I may also say that I never shared the extremist position often heard in the early 50's and ruling out differentiability from economic modelling. To analyze why this position was then taken would be an interesting subject; but it is not our subject today.

III. Accomplishments

1. An impressive building

Turning now attention to what was accomplished since the December 1952 meeting, I need not insist on the size and beauty of the edifice that was built through the years. Everybody should agree that the general equilibrium theory, as we now see it, is the central piece in our science, because of both its relevance and its logical robustness. Everybody should agree that it has no serious competitor as a central piece.

But I must try and be more specific. While still remaining at a distance and not entering into the fields of the six panels, I must assess what are the main aims of the theory and how it performs in reaching these aims. I shall identify three main aims. The theory has first to provide a general representation of the functioning of the price system; it has to make this functioning intelligible. The theory has second to be operational, by which I mean that it should predict the effects of exogenous changes and propose normative rules for the organization of resource allocation. The theory has third to provide foundations to other theories that deal with particular aspects or problems of market economies. I shall consider each one of these three aims in turn.

2. A better understanding of the price system ?

Advances were quite important during the past forty years in research about the functioning of the price system. In 1952 one was certainly not ignorant about intertemporal exchange or about insurance and other operations involving risk. One had ideas on how the Walrasian system could take time and uncertainty into account; these ideas were fairly precise about time, more fuzzy about uncertainty. But it is obvious now in retrospect that one had not yet perceived how many issues had to be clarified.

In this respect alone the achievements of the last forty years are so varied, and so substantial for our understanding of market economies, that we may wonder how the panel on time and uncertainty will succeed in doing justice to all of them. Progress was of course also achieved in other respects, as we know.

Were there any failure of a general nature? Thinking about the question I am identifying two deficiencies, which should perhaps be considered as failures. They may be called imperfect competition and price stability.

Looking at actual industrial structures and at actual business or trade union practices, one can hardly be satisfied when admitting that our general theory of the price system assumes perfect competition. One might argue that knowledge derived from this theory extends beyond its formal model and that we gain from it a heuristic perception on how the real, imperfectly competitive, world functions. One might argue that many situations in which competition is not perfect have been carefully studied and that research in this field during the past forty years has given an enlarged scope to what was previously available. One might even argue that general equilibrium models have been built for some idealizations of imperfect competition.

But all that does not amount to anything that could be compared to what is now available for the theory of perfect competition. So much was achieved about the general competitive equilibrium, in its various forms, that we wish we could similarly analyze more realistic cases, which then ought not to be too special. Unfortunately attemps made during the last few decades give little hope that such an achievement could soon occur.

The second deficiency follows from an ambiguity in the teachings of general equilibrium theory about the performance of the market system. The theory explains well that this system greatly helps for efficiency in the normal allocation of resources. The theory explains well that this system does not guarantee that the distribution of levels of living will be satisfactory on ethical grounds and the theory provides the framework for a good analysis of income and wealth inequalities (except for imperfect competition). But the theory is ambiguous about the stability issue: will the price system functions in such a way as to avoid perverse and destabilizing evolutions ? Here I am not referring to the formal problem of studying how the equilibrium is reached, a problem about which, by the way, we shall perhaps be a little too silent in this conference. I am referring to the real problems raised by disturbances in the evolution of our economies. Is there any responsability of the price system in explaining such disturbances ?

Of course, one should not expect any single theory to explain all the troubles that may appear in organisms as complex as modern economies. This is why I am not here thinking of business cycles, inflation and unemployment, which have to be studied by macroeconomic models. But the theory of prices should tell us whether, under ideal exogenous and market conditions, there is a systemic risk in the determination and evolution of the temporary equilibrium. The question may appear a bit narrow; but its importance in price theory cannot be disputed. The question is approached with models assuming rational expectations, or still more simply with models in which no uncertainty occurs and expectations are assumed to be exact. These models accept strong restrictions on the numbers of agents and commodities, or on the forms of utility and production functions.

If I am speaking of a deficiency of our theory it is because of the ambiguity of the conclusion derived from these models about the issue they are meant to address. Perverse evolutions are found with some specifications (see for instance Grandmont, 1991). But I do not know what kind of implication I should derive from these specifications: are they revealing what has a significant chance to happen or are they pure curiosities ? So far as I perceive the situation in our profession, theoreticians seem to split into two groups on this issue: some seem to claim that perverse evolutions so found explain part of what is observed in real economies; others prefer the harmonious picture given by the type of growth models used for instance in the real business cycles movement.

3. A more operational theory ?

Did research on general equilibrium during the last forty years make our theory more operational ? It was definitely intended to do so in the 1950's with the impact of general equilibrium on the study of production by activity analysis, on the management of the public sector, on growth accounting and the like.

But starting in the decade of the 1960's, interest in applications faded away. This may be explained, at least in part, by the inner development of the general equilibrium research program. A rigorous examination of contemplated comparative statics properties, or of contemplated resource allocation rules, led to the unpleasant finding that hardly any of them could be proved to hold true, or to be good, under all conditions. Even the stylized world of the competitive general equilibrium was already too complex.

But operational purposes cannot long be neglected. Recent developments inspired by general equilibrium theory aim precisely at deriving comparative statics properties. This is true of competitive growth theory with its two modern avatars: real business cycles and endogenous growth. This is true of the now familiar product differentiation model. This is true of asset pricing theory. These developments raise serious questions about the generality of the results to which they lead. I gather that none of those results would stand in general if they were closely looked at by pure theorists applying the hypothetico-deductive method that built general equilibrium theory.

Fortunately it is now realized by many among us that, in order to meet operational needs, one has to, and one can, combine rigorous deduction with analysis of available data. We need more specific results than the ones provided by our present general theory; but we do not want to accept them just because they look nice for a general economist or interesting for a mathematician. We must see how the specifications leading to such results stand when confronted with statistical data. We must work on this confrontation at all stages: at the initial stage for the choice of the first hypotheses concerning individual behavior; at the aggregation stage, when more precise hypotheses concern both individual behavior and the structural distribution of individual units; at the final stage when we must check the empirical validity of the consequences obtained. Rigor must be aimed at all along, by application of similar standards to those that emerged in other fields of application of statistical methods. On this approach I hope to have the support of Werner Hildenbrand and others in our panel on aggregation (see for instance Hildenbrand, 1989). Indeed, I believe a recent trend in our research precisely aims at correcting the previous failure of general equilibrium theory to take operational needs into account. It should eventually lead also to a less ambiguous assessment of the stability issue.

4. A theory providing foundations to others ?

The general equilibrium architects may have been seen by some economists as intending to build a universal theory of all economic phenomena. This project, if it was entertained, was not achieved so far and cannot reasonably be expected to be achieved before long. Even such an apparently simple operation as to put real money in general equilibrium has not yet been achieved. But I do not consider those difficulties as failures because the project to have a universal theory was not achievable. The relevant question now is to know whether, by its representations of the price system, general equilibrium provided some useful foundations to other theories concerning phenomena occurring in market economies. It is then easy to answer that much was achieved in this respect.

The clearest case is about economic growth, for the theory of which the competitive temporary equilibrium was widely accepted as the central concept. I may even say that it was accepted with little amendment by John Robinson, Pierro Sraffa and their neo-Ricardian successors. Concentrating their attention on the production sphere, assuming constant returns to scale, and accepting to have an incomplete theory of the full price system, they could think in general equilibrium terms without committing themselves with the neoclassical representation of the household sector, a representation that these authors found to be disputable. They so stood at one extreme in the group of growth theorists, the other extreme being held by those assuming a single infinitely living household.

For the rest of macroeconomics there are two types of microeconomic foundations. Some aim at explaining actual features of our economies that do not perfectly fit with the law of supply and demand; such a feature is price and wage rigidity. Other foundations aim at providing a conceptual framework for the building of macroeconomic models; such a framework is provided, for analysis of the short term, by the fixed price equilibrium. Clearly, general equilibrium theory is more suited for the second type of foundations than for the first. I need not insist in particular on the view according to which the specification of the fixed price equilibrium was a real achievement.

Studies of asymmetric information and incomplete markets provide two well known kinds of foundations of the first type. They led to valuable explanations of a number of features in real economies. What is not clear to me is whether general equilibrium with asymmetric information or with incomplete markets is likely to give any useful framework for other economic theories. It seems to me that this question ought to be somehow considered in our panels.

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My role in this anniversary lecture was to recall how the existence proofs of December 1952 had originated and how important they were. My role was also to try and give some hints as to the broad accomplishments and broad failures of general equilibrium theory during the past forty years. I have been short on each one of these three functions of my talk, particularly so on the last one. But being longer would not have been appropriate since our panels will discuss at length the issues, much better than I could have done.

I am sure they will question some of my too sweeping statements. But I am also confident that they will not challenge the reason why we all are here for this conference. Yes, December 1952 is a date to be remembered in any history of the science of economics.

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Abstract economies

The equilibrium manifold and comparative statics

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One of the most significant developments in general equilibrium analysis in the last forty years has been the application of nonlinear analysis in addressing the questions of existence and uniqueness of equilibria in the general equilibrium (GE) model and the general equilibrium with incomplete markets (GEI) model. In this formulation of the Walrasian model and its generalizations, the theory of differentiable manifolds replaces the theory of linear vector spaces as the framework for equilibrium analysis - an elegant discussion of the GE model from the differentiable viewpoint may be found in Mas-Colell (1985).

Both Smale (1974) and Balasko (1975), independently but inspired by Debreu's seminal paper on regular economies, Debreu (1970), proposed notions of the equilibrium manifold. Smale also considered the manifold of Pareto optimal allocations, which he showed was a smooth manifold of dimension $M - 1$ where M is the number of agents in the given exchange economy. If the economy has a finite number of commodities then this manifold is diffeomorphic to the Pareto frontier, which plays an essential role in Negishi's proof of existence. Chris Shannon (1992) has recently extended Smale's characterization to ℓ_∞ commodity spaces where traders in her infinite horizon exchange economy have additively separable utility functions. Her extension is motivated by the desire to generalize Debreu's results on regular economies to exchange economies with infinite dimensional commodity spaces.

In his survey article, Smale (1981) defines the equilibrium manifold as the set of ordered pairs of equilibrium allocations and distributions of individual endowments which satisfy the first order conditions for a competitive equilibrium. Smale's first order conditions are in terms of the inverse demand functions. In contrast, Balasko defines the equilibrium manifold as the set of ordered pairs of equilibrium prices and distributions of individual endowments such that the market excess demand function is zero. He proves a number of important properties of the equilibrium manifold, e.g. it is diffeomorphic to Euclidean space, which are discussed at length in his important monograph on smooth economies Balasko (1988). We shall see that Balasko's notion of the equilibrium manifold lends itself to a discussion of comparative statics for an exchange economy.

Comparative statics continues to be the primary methodology for deriving refutable propositions from economic theory. This mode of analysis is most highly developed within the theory of the household and the theory of the firm, e.g. Slutsky's equation, Shepard's lemma, etc. As is well known from the Sonnenschein-Debreu-Mantel (SDM) theorem, any attempt to derive Slutsky-like restrictions on the market demand function is doomed to failure - see the Shafer and Sonnenschein (1982) survey article for an illuminating discussion of the SDM theorem. Hence it is sometimes said (and I will argue, incorrectly said) that general equilibrium theory has no testable implications. This "argument" is on occasion used to justify the use of representative agent models in macroeconomics.

It seems to me that if we mean by comparative statics the change in endogenous variables given a change in exogenous variables, then the analogy between individual demand functions and market demand functions, which underlies the invocation of the SDM theorem as "proof" of the empirical vacuity of the GE model, is wrong headed. In the competitive paradigm, consumers are price takers and choose their utility maximizing demands subject to their budget constraints. Hence for the individual consumer prices are exogenous and demands are endogenous. But for the economy as a whole prices are endogenous. What then are the exogenous variables for the economy as a whole ? If tastes are stationary (as we shall assume) then the distribution of individual endowments completely describes the exogenous variables for an exchange economy.

If this argument is correct then it appears that the SDM theorem has nothing to say about the comparative statics of an exchange economy, since in this theorem individual endowments are fixed and prices vary as they would for a price taking consumer - more about this later. The first local comparative statics result, in terms of the equilibrium manifold, is due to Debreu. He showed in his paper on regular economies that for regular economies the equilibrium prices are smooth functions of the distribution of individual endowments.

Global comparative statics analysis of exchange economies begins with Balasko's remarkable paper, Balasko (1975), where he defines the equilibrium manifold as the graph of the Walrasian correspondence. Just as Samuelson (1947) asks if observations on consumer choices, prices and incomes are consistent with utility maximization subject to a budget constraint, we can ask if observations on market prices and distributions of individual endowments are consistent with the GE model. That is, do the observations lie on the equilibrium manifold of some exchange economy ?

This is the research agenda that Rosa Matzkin and I began in BoWo'89. Recently, we derived an axiom on market prices and the income distribution for the two person-two good exchange economy, where there are two observations, Brown and Matzkin (1993). This axiom gives necessary and sufficient conditions for the data to be consistent with the GE model. Charlie Plott has run some experiments for us to test our revealed equilibrium conditions on real data. Sue Snyder, a Ph.D. student at Stanford, has derived an equivalent axiom for this case and shown that the notion of efficiency prices or Pareto supporting prices is not refutable, given market prices and social endowments. That is, one can always find utility functions and allocations such that the allocations are Pareto optimal and are supported by the observed market prices, Snyder (1993).

Now, to return to the implications of the SDM theorem for comparative statics analysis of exchange economies - more precisely - we consider the Hildenbrand and Grandmont program on aggregation, see Hildenbrand (1992) and Grandmont (1991). If we assume that the market excess demand function has the gross substitutability property for all distributions of individual endowments, then the equilibrium manifold is the graph of a smooth function. This suggests that the "Law of Demand" and its variants such as "Wald's Axiom" impose restrictions on the equilibrium manifold and are therefore refutable. Of course, this requires the assumption of stationary preferences - an assumption which Hildenbrand rejects. Since we can certainly guarantee stationary of preferences in the Plott experiments, let us proceed.

Hildenbrand restricts his attention to exchange economies where agents have collinear endowments. This is a w.l.o.g. due to the surprising theorem of Kirman and Koch (1986) which says that any market excess demand function can be represented by an exchange economy where all agents have the same preferences and individual endowments are collinear. Following Hildenbrand, we now consider only exchange economies with this endowment structure and vary the total (or social) endowment, for a fixed family of utility functions. This generates an equilibrium manifold which locally (at least generically) has a structure similar to that of an individual consumer's demand. For individual consumers, we know that globally the Jacobian of the derivative of the Marshallian demand function is the inverse of Jacobian of the derivative of the inverse demand function.

Thorsten Hens is visiting Stanford this year from Bonn. In discussing the Law of Demand, we discovered that on this equilibrium manifold, at regular economies, the derivative of the market demand function w.r.t. equilibrium prices is the inverse of the Jacobian of the derivative of the equilibrium prices w.r.t. the social endowment. Therefore, any assumption on the Jacobian of the derivative of the market demand function implies restrictions (locally) on this equilibrium manifold, hence are refutable in principle. For example, if we assume that the Jacobian of the market excess demand function is negative definite then it is also true that the Jacobian of the derivative of the equilibrium prices w.r.t. the social endowment is also negative definite. A global version of this assumption is that the Walrasian correspondence is a monotone correspondence - see Aubin and Frankowska (1990) for a discussion of monotone correspondences and their derivatives.

After this rather lengthy introduction, we turn to some open questions in the empirical application of the GE model. I think the criticism of applied economists, in particular labor economists, see Heckman and Macurdy (1988), that too little attention has been given to empirical applications of the GE model relative to the time and energy consumed in exploring its "abstract" properties, is well taken. There are, of course, some noteworthy exceptions. I have in mind the pioneering work of Scarf (1973) on the computation of equilibria in the GE model and its subsequent application by his students: Shoven, Whalley, and Kehoe - see Kehoe (1991) for a recent survey; and Hildenbrand's fundamental empirical work on market demand, Hildenbrand (1992). Outside the standard literature of this audience there is the recent work of Townsend (1993) and Udry (1992) who have tested the GE model as a model of markets in villages of LDC's such as India and Nigeria. Finally, there is Chiappori's paper on household labor supply, Chiappori (1988), which comes closest to my work with Rosa. He is asking, albeit implicitly, if observations on household incomes, labor-leisure choices, wage rates, and total consumption lie on the equilibrium manifold for some family of utility functions for the agents living in the household.

Well, what are the open problems ? First, we need an empirically oriented theory of consumer demand which has a random component for those aspects of optimal choice which are not captured by maximizing a deterministic utility function over bundles of quantities of goods in a budget set. The random utility model has been developed with great success in discrete choice models, but no comparable progress has been made in developing a stochastic version of revealed preference theory - but see Varian (1985), Epstein and Yatchew (1985), and Goldman and Rudd (1992) for some imaginative attemps to develop such an econometric theory. Unfortunately, revealed preference theory is easily refuted with real data and a theory of errors more sophisticated than errors in measurement is surely needed.

Second, the basic construct in general equilibrium theory are correspondences not functions. But, to my knowledge, there is no econometric methodology for either estimating correspondences or testing hypotheses about correspondences. Since the equilibrium manifold is the graph of a correspondence and in general not the graph of a function, the econometrics of function estimation and hypothesis testing is of no use in determining econometrically if observations on market prices and income distributions lie on some equilibrium manifold.

In sum, we need a stochastic version of the Afriat inequalities of revealed preference theory and an attendant econometric theory of correspondences before we can econometrically test the GE model of pure exchange in terms of the equilibrium manifold.

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Recents advances in general equilibria with infinitely many goods

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About forty year ago Arrow, Debreu and McKenzie developped the beautiful and useful theory of General Economic Equilibrium. This theory became more and more important with the failures of Partial Equilibrium and Aggregative Economics. Because of this, there has been a great volume of work to improve the theory both in its conceptual and technical aspects. Among the most important conceptual work one could cite the integration of General Equilibrium with imperfect competition, the Assymetry of Information, the computation of equilibria and macroeconomic issues like money, economic fluctuations and growth. This topics are treated elsewhere in this conference. Here, in the abstract economies section, we are more concerned with mathematical generalizations rather than conceptual questions. More presicely, my share on this section is to talk about General Equilibrium for infinite economies: its success and its failures. Since the time is short I will concentrate mostly in infinitely many goods rather than infinitely many agents. First of all, why work such an extension of General Equilibrium ? The answer comes quickly if one thinks of the many applications of economics that uses an infinite number of state of words or an infinite number of time periods. That is the case of Finance, see Duffie (1988), Growth, see Lucas (1988), Macroeconomics, see Stokey and Lucas (1989), International Economics, Monopolistic Competition, see Mas-Colell (1975) and Jones (1984) and many others. Motivated by the example of Growth, Bewley (1972) gave an existence proof of great generality. However, there were a contrast with the finite dimensional economics. As we all know, in a pure exchange economy essentially all we need to get existence of equilibria is convexity of preferences. For an infinite number of agents the work of Aumann, later confirmed by the work in smooth by aggregation, showed that convexity can even be dispensed with. However, with infinitely many goods one has to add Impatience to Convexity to get existence.

After the work of Bewley the question of existence of equilibrium with infinite goods seemed settled. However, many models in economics uses prices or commodities whose log is a Brownian motion whose paths are in L^2 but not in L^∞. This work pointed out the need of conditions of technical nature that require the commodity space to be a Banach lattice, which is the case for the spaces used in economics like L^2 or the space of measures, see Aliprantis, Brown and Bukinshaw (1989). However, in this work there were also conditions on preferences, first introduced by Mas-Colell, called properness conditions. Unfortunately, even when weakned they dont give satisfactory theorems. This is so, because, for the model of finance in L^2, that requires also Inada's condition, the set of initial endowments that would fail to give equilibrium is generic, see Araujo and Monteiro (1981). That is, equilibrium will typically fail in those models.

In the following let me outline a possible solution for the lack of existence in L^2 and $M(K)$, the space of measures – see Araujo and Monteiro (1981). This could be accomplished by considering a subspace of the commodity space and price that is defined on it only and not in the whole space. This approach however might not guarantee prices to be in L^2 anymore. Theorems of this type have been obtained before by many authors, including Peleg and Yaari and Boyd and MacKenzie. More precisely one has the following theorem. For $w \in L^p$ define

$$K(w) = \bigcup_{\lambda > 0} [-\lambda w, \lambda w].$$

Theorem: Let $\varepsilon = \{(w_i, \succeq_i) i = 1, \cdots, I\}$ be an economy in $L^p(\mu)$, such that

(1) $w_i \in L^p_{++}$,
(2) $\succeq_i$ is norm continuous,
(3) $\succeq_i$ is convex,
then, $\varepsilon / K(w)$ has a quasi equilibrium $\pi, \pi : K(w) \to R$, such that, if $x \in K(w)$,

$$\pi(x) = \int \hat{\pi}_t w_t x_t d\mu \text{ some } \hat{\pi} \in L^1_+(\mu).$$

Observations: It might happen that $\pi \notin L^q$; $\succeq$ is called proper at $\overline{x}$ if $\exists v, \exists U(0)$, neighbord of 0 such that $\forall y \in U(0), \forall t \geq 0$ $\overline{x} + tv - ty \succeq \overline{x}$; a similar theorem holds for the space of measure $M(K)$.

The case where markets are incomplete and assets are paid in real terms poses particular difficulties for the existence of equilibria. See Radner (1972), for the concepts involved in incomplete markets, Hart (1975), for non existence, and Duffie and Shafer (1985), for generic existence in the finitely many state of nature case. The problem of existence with infinitely many states of nature and incomplete markets require strong conditions, see Mas-Colell and Zame (1991). However if the possibility of bankruptcy of agents is allowed, see Dubey, Geanakoplos and Shubik (1988), for the finite case, then existence becames easier see Araujo, Monteiro and Páscoa (1994).

So far, I just talked about existence. Another important question posed by General Equilibrium is the local uniqueness of equilibria proved by Debreu for the classical case. However, the phenomena of indeterminacy occurs in incomplete markets see Cass (1985), Geanakoplos and Mas-Colell (1989). This is mostly disturbing since for one side, exploiting smoothness by aggregation ideas, mathematical economists are trying to get global uniqueness of the models while for some models with incomplete markets we do not even give local uniqueness. However with the introduction of nonlinear payoffs, one can restaure local uniqueless Araujo and Páscoa (1993).

To finish this brief note we would like to mention an application of the work with infinitely many goods to the question of convergence of expectations see Araujo and Sandroni (1993). This work says that if one starts with a temporary equilibrium and if one has complete markets and infinite time periods we end up with rational expectations.

The idea exploited here is that sets of measure zero have to be the same to all agents to avoid arbitrage opportunities. But, by the Martingale convergence theorem, that would imply convergence of expectations.

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Mathematical elegance or economic relevance
Three examples

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Introduction

The purpose of these pages is to underline the continued importance of both the mathematical elegance and the economic relevance obtained 40 years ago in general equilibrium theory and in the Arrow-Debreu-McKenzie model of general equilibrium theory.

Example I - The Core

Shortly after the breakthroughs in general equilibrium theory 40 years ago the theory of the Core in an economy with many small consumers was added to the theory. Debreu, Scarf (1962) generalized the techniques used by Edgeworth (1881) and proved convergence of the Core in Replica Economies to the set of Walrasian Equilibria. The same technique was used by Aumann (1964) to prove the same equivalence for the case where the set of consumers was the unit interval. In the right mathematical formulation - atomless vector-measures and Lyapunov's theorem - the mathematical elegance of this equivalence theorem was so overwhelming that some naively believed that the classical problems treated most extensively by Edgeworth (1881) of how to formalize "competition" and "many small" now had been solved. It was even suggested to call elements in the Core "Edgeworth-allocations", Vind (1964).

The idea in the definition of the Core, that a coalition of consumers can form and prevent its members from having exchanges outside the coalition can, however, not be found in classical economics from Turgot over Jevons and Edgeworth to Wicksell. Their idea of competion involved specifying the exchanges and not just the net trades, and coalition formation was explicitly excluded.

The elegance of the theory of the Core unfortunately did not mean that we had solved the very relevant problem of how to define competition.

There are still experts on the core who maintain that Edgeworth's concept of competition was the core. One of the reasons for this may be that mathematical elegance has dominated economic relevance.[1]

Example II - Non-total and non-transitive preferences

One of the most important development in general equilibrium theory has been the proofs of the main theorems without total and transitive preferences. This is very elegant and the theory gains generality. In the Arrow-Debreu-McKenzie model of the theory it is often said that nothing will be changed by having markets open after the first point in time. This implicitly assumes that preferences of consumers at time t are just the induced preferences from time 1. In the new generality of the theory, this is a hopelessly special assumption. The natural assumption is now that the preferences for consumption at time t becomes finer over time from 1 to t. The consequences of this may be a less elegant theory, but clearly more relevance and more realism in a model of the theory.

Example III

The elegant result of Savage, that a total preorder on a function space under an independence condition can be represented by expected utility, where both the utility function and the measure is obtained from the theorem, has been used and has been critisized. Some have accepted and used the result despite the critique. Others have given up on elegance and introduced approximately countably many alternative more "relevant" and more "realistic" theories.

Conclusion

The three examples clearly show that insistence on elegance is not enough, insistence on relevance at the cost of elegance will severely limit the progress and applicability of the theory. We have to insist - like Arrow, Debreu and McKenzie 40 years ago - that our work is not finished before the relevant economic problems have elegant solutions.

[1] A related reason may be that "final settlements" in Edgeworth is not a solution concept covered by existing game theory. A third reason may be that the proof of convergence in Edgeworth (1881, 34-38 and 143-144) also proves convergence of the core, but trivially a proof does not determine a theorem.

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Time and uncertainty

Remarks for general equilibrium conference session "Time and uncertainty"

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There is no doubt that the seminal contributions of Arrow, Debreu, McKenzie, hereafter ADM, have built a general equilibrium foundation for economic science of lasting value. All one has to do is skim the Handbook of Mathematical Economics, the New Palgrave, et al., to see what has been accomplished and the essential role of ADM in these accomplishments.

In response to the organizers' instructions to panel members to focus on "open questions, puzzles, and, in general, where they think the challenges lie," I would like to offer the following topics for discussion. Possibly outrageous suggestions are offered to stimulate discussion from the panel and from the audience.

I am working under the assumption that Lucas and others in their session will discuss the impact of Intertemporal General Equilibrium (IGE) models on empirical macroeconomics, Kehoe will discuss computational IGE and its applications, Cass and Polemarchakis will discuss Intertemporal General Equilibrium with Incomplete markets (IGEI), with attendant problems of multiple equilibria, extrinsic (sunspot) uncertainty, and Chi Fu Huang will discuss modern GE continuous time finance. If this assumption turns out to be wrong I would hope that each of these key topics is discussed somewhere on the program. Let me offer several topics, that, hopefully, will add to those raised by others, for my part of the panel's offerings.

Open questions, puzzles, challenges

(I) How common is chaos in IGE and IGEI models ?

(II) How has GE influenced empirical work in intertemporal economics in the past and how will GE influence empirical work in the future ?

(III) What evidence exists for aggregative nonlinearity and how should IGE attempt to model it ?

(IV) What type of empirical evidence would lead the profession towards a macroeconometric view that looks more like that propounded in Guesnerie and Woodford (1993) survey and looks less "log linear" with less dependence upon "exogenous shocks" ?

(V) What evidence is there for nonlinearity in finance and are econometrically tractible noisy rational expectations models with learning the right way to go to capture it ?

(VI) What role does interconnective structure (and "connectionist learning") in the economy play in breakdowns of the Law of Large Numbers and the Central Limit Theorem in large economies which translate into macroeconomic effects such as blowoffs and crashes ? Is there an econometrically usable impact on economic dynamics of intensity of choice of agents across actions, incentives to do high intensity choice, and degree of correlatedness of agent characteristics in a Föllmer type ADM GE setting ?

Topics (I), (II) develop the theme of Routes to Randomness in intertemporal general equilibria but the second stresses Routes to Macroeconomic Randomness. III-VI stress development of classes of econometrically tractible GE models that let data speak to the form of Routes to Randomness.

(I) How Common is chaos ?

In view of the Sonnenschein, Mantel, Debreu theorem for GE and the Boldrin, Montruccio theorem for growth models one should suspect chaos as an "easy" theoretic possibility. The surveys of Boldrin and Woodford (1990), Guesnerie and Woodford (1993) discuss the large theoretical literature on chaotic economic dynamics.

However, to my knowledge, no one has "calibrated" a chaotic model to data to the same level of pursuasiveness as has been done for stochastic models in the Real Business Cycle literature (in the sense of matching moments and cross moments). It *may be worth discussion whether it is likely one can ever do so.* It seems the main obstructions will be consistency with moments and cross moments in bond, and stock returns and data on supply/demand elasticities and cross elasticities, contemporaneous and intertemporal.

While actual empirical testing for chaos in economic and financial data has not revealed much evidence for it, much of this literature has taken the form of new specification tests of "established" models, but constructed to have high power against chaotic alternatives. But this is not a direct test for chaos.

A direct test (carried out by Dechert, Ellner, Gallant, Gencay, Nychka) would (i) estimate the underlying dynamical system using methods to "clean" the noise and (ii) estimate the largest Lyapunov exponent λ and test $H_0 : \lambda > 0$. This type of procedure has the potential to yield a test of chaos with size equal to power under the null. To my knowledge no convincing evidence for chaos has emerged from these methods either. I wish to suggest that chaos may be ubiquitous even though there's little evidence for it in the data. The chaos may be high dimensional, irregular, or observed through too much noise. But I want to entertain another type of argument for it.

It may be possible to formalize the idea that chaos should be likely (close to "probability one") in IGE and IGEI models with lots of degrees of freedom. Incomplete markets models seem like rich breeding grounds for chaos. The ET (1992) symposium contains papers by Cass and others that show that sunspot indeterminacy is extremely likely in two period incomplete markets models and the literature is moving towards exploration of the connection between presence of sunspots and complex dynamics including chaos. Turn to Guesnerie and Woodford's general setup.

Letting x denote this period's state vector and μ' next period's expectations on the state vector, consider Guesnerie and Woodford's "reduced" form linkage between present and future, with μ' equal point mass on x',

$$Z(x, x') = 0, \quad \text{with solution } x' \in \phi(x), \tag{1}$$

for the class of "temporary" IGE models they treat. Note that many of the Chiappori and Guesnerie, Geanakoplos and Polemarchakis, Grandmont, Kehoe, Magill and Shafer settings in the Overlapping Generations (OG), IGEI, and Temporary Equilibrium (TE) traditions generate dynamics of the form $Z(x, x', x'') = 0, x' \in \emptyset(x, \mu''), \mu''$ equal point mass on x''. Hence we use (1) just as an example which suggests more generality. Consider the case where $x' = \phi(x)$. Let us discuss results on the likelihood of chaos on the space of maps.

The physicist Kurten (1988) has done numerical experiments on the "coarse grained" system, $\phi : \{-1, 1\}^N \to \{-1, 1\}^N$ defined by the "neural network"

$$\sigma_i' = \text{sgn}(\sum c_{ij}\sigma_j), i = 1, 2, \cdots, N. \tag{2}$$

Although his statement how he drew the c_{ij} is not precise it appears he drew them Independently and Identically Distributed uniform $[-1, 1]$. He found that as N grew the fraction of "chaotic" systems tended to one, provided connectivity (number of nonzero c_{ij} allowed for each i) was greater than or equal to 5. Here "chaos" means cycle lengths grow exponentially with N rather than linearly or less. Note that $\text{sgn}(x) = \lim_{\beta \to \infty} \tanh(\beta x)$, so I can pass to (2) along a β-arc.

This, of course, does not prove chaos is likely for the space of maps from $[-1,1]^N \to [-1,1]^N$. But it is suggestive of possibilities for some interesting theorems because (i) there is a serious literature in neural networks on density of approximations to general maps by combinations of sigmoids (Hornik, Stinchcombe, White (1990)) (ii) Kurten's results show the importance of c_{ij} not equal to c_{ji} and the drawing from $[-1,1]$ - a space that allows both signs. This last is important because M. Hirsch has shown that dynamic behavior of systems with positive off-diagonal cross partials is very limited and chaos is not possible in such flows.

Physicist J. Sprott (1993) has drawn second order polynomial maps and flows at random from a cubic parameter box centered at the origin. After discarding the ones diverging to infinity, he showed the fraction of chaotic flows increased with the dimension of the state space but the reverse was true for difference equations. It is not known what would happen to these results if the flows and maps were restrained to be "inward pointing" on the boundary of a large compact cube centered at the origin.

The Kurten study suggests ubiqitous chaos, and the Sprott study suggests very common chaos for flows with a puzzle for maps. Let me give another argument for ubiqitous chaos which I have enlisted Mukul Majumdar's aid to begin to attempt to formalize.

Impose enough "inward pointing" conditions to keep the dynamics $x(t+1) = \phi(x(t))$ away from infinity. First, consider a matrix question: Induce a measure ν_n on the space of $n \times n$ real matrices. Consider the limiting probability of getting an n by n matrix with at least one eigenvalue greater than one as n goes to infinity. Just about any way of inducing a probability structure $\{\nu_n\}$ will lead to the probability of getting at least one eigenvalue bigger than one going to one as n goes to infinity so long as you place *some* mass on parts of the parameter space with eigenvalues bigger than one.

With this in mind, turn to Ruelle (1990). Ruelle examines $(A^*A)^{(1/2T)}$, $A = D^T(\phi)$, i.e. A is the derivative of the Tth iterate of ϕ starting from some initial condition x_0. Ruelle's A^* is the adjoint of A. He shows for ρ (where ρ is invariant measure for ϕ) almost all initial conditions x_0, $(A^*A)^{(1/2T)}$ converges to a limit matrix L which is independent of x_0 if ρ is ergodic. If for each n, we now consider an induced measure on the space of limit matrices L, induced by some probability measure on the set of ϕ's that map R^n to itself, then the matrix result hints that the probability of getting chaos, i.e. at least one eigenvalue of Ruelle's limit matrix bigger than one, must tend to one as n tends to infinity.

I realize there is a lot of work to turn this kind of loose idea into any kind of theorem. At the minimum controlling the relationships among each map, its invariant measure, and the induced probability structure on Ruelle matrices, and, hence, their eigenvalues, is going to be hard. But I think such types of theorems would change the way we look at how the economy takes inputs many of which we don't see and turns them into time series output observed by econometricians and others.

Suppose we could get such a theorem for the space of maps. Then one could "back out" the conditions needed on the primitives generating Guesnerie and Woodford's $Z(\cdot,\cdot)$ to get a result on economies.

I believe IGE research on dynamic equilibrium systems with a large variety of cooperative and competitive relationships between this period's state and next period's state may show a substantial percentage of such systems to be chaotic. Even though empirical testing has failed to detect evidence of chaos, I believe theoretic arguments on commonality of chaos will cause increased focus of research on the connectivity structure of the economy and what economic forces cause connective links to be increased and decreased over time.

Durlauf's recent work (1993) should play a role here. Earlier work like Allen (1982) and Follmer's seminal piece (1974) should be helpful. This is so because the new aggregation theory, Hildenbrand, Jerison, and Grandmont, has taught us the importance of agent characteristics distributions upon aggregate quantities in generating, for example, "gross substitute" type behavior for aggregate demand, Grandmont (1992). Hence, conditions on correlatedness of agent characteristics would appear to play a key role in generating "complex" dynamics in aggregates. More will be said about this below.

Another topic which I believe should be discussed by the panel and audience is this:

(II) How has GE influenced empirical work in intertemporal economics in the past and how will GE influence empirical work in the future ?

A general puzzle under this heading is the ubiquity of results on nonlinearity, chaos, extrinsic uncertainty (sunspot indeterminacy) and other "complex" dynamic phenomena in the IGE literature, but the puacity of empirical work which addresses these results. Why have empiricists been so slow to embrace this literature ? Is it because "Anything Goes" (You give me a map and I will give you an Economy ...) theorems and "Logical Possibility School" (If it happens on an "open" set of economies, it must be important) type of results lead econometricians to believe that IGE theorists bear nothing but free parameters and refuse to state theorems in terms of econometrically usuable restrictions on data ? Is it because econometricians believe that a lot of this kind of behavior gets "washed out" by something like the Law of Large Numbers (LLN) during the process of aggregation ?

What can we do to increase the influence on working econometricians of this type of theoretical work ?

(III) Evidence for aggregative nonlinearity and IGE attempts to model it

Aggregative IGE models used in much of current macroeconometrics appear to be well approximated by (log) linear stochastic processes. This squares well with the ability of VAR's to fit vectors of aggregative time series after appropriate detrending and deseasonalizing. This research raises the issue of whether there is any evidence for nonlinearity in aggregative data and, hence, whether the aggregative nonlinear modelling surveyed in Guesnerie and Woodford (1992) will ever have much influence on the branch of macroeconomics that specializes in the analysis of aggregative data.

There is some good (weaker) evidence against Wold type moving average representations driven by IID (Martingale Difference Sequence) innovations. Econometricians such as Granger, Hamilton, Pesaran, Potter, Terasvirta, etal. have had some success fitting nonlinear functional forms to macro data. Asymmetries appear to be a main cause of nonlinearity findings. Some abrupt changes imputed to "regime changes" may actually be evidence of "endogenous" abrupt change that could be captured by a Föllmer and Durlauf type of IGE model.

(IV) *What type of empirical evidence that would lead the profession towards a macroeconometric view that looks more like that propounded in Guesnerie and Woodford's survey and looks less "log linear" with less dependence upon "exogenous shocks" ? Is there an "implicit view" in the minds of many macroeconometricians that corresponds to potential extensions of the "new aggregation theory" (Hildenbrand, Jerison, Grandmont, etal.) to IGE models that argues for intertemporal "gross substitutes" behavior like that in Grandmont, (1992) ?*

Steven Durlauf has written a series of papers which apply Interacting Particles Systems (IPS) ideas (cf. Föllmer and B. Allen) to produce models which are potentially econometrically tractible. If interconnective structure is strong enough, multiple limit measures appear and the cross sectional LLN fails. While these models have influenced his and others empirical work on "convergence/divergence clubs," unit roots, time series persistence, long memory in time series, and low activity (poverty) traps, I have not seen direct econometric estimation of models of this type of GE model that parallels the econometric literature on homogeneous IGE models or their modifications for external effects along more aggregative Romer/Lucas lines.

This may be due to the inherent complexity and econometric intractibility of IPS probability structures. But use of recent results in the IPS literature on Mean Field Approximations (MFT) to these probabilities and the connection between the structural form of IPS probabilities and discrete choice probabilities in the discrete choice econometric literature may produce an econometrically tractible class of IPS models (cf. Brock (1993)). In this class the data can speak to "Hebbian-like" rises and falls (due to past economic activity along the links) in the strength of links in the interconnective structure, choice intensity of technology, and takeoff/atrophy of parts of the economy.

It is not clear what the advantage of this kind of model is over the versions of GE used in the computible GE literature (cf. Kehoe). Perhaps they may have some advantage in the endogenization of "emergent structure," abrupt change, and choice intensity. Maybe this will help econometricians build models that will allow data to speak to crashes, blowoffs, "bubbles," emergent extrinsic uncertainty, and other phenomena which are treated extensively in theoretical GE but do not seem to have the impact on empirical work that they deserve.

(V) Evidence for nonlinearity in finance and possible econometrically tractible noisy rational expectations models with learning to capture it

The October crashes and the "excess" volatility literature has increased interest in nonlinear GE modelling in finance. Noisy Rational Expectations models have been used by Gennote and Leland (1990) to explain crashes. Noisy RE negative exponential, conditional Gaussian, (mean variance asset demands) GE models have been examined from an empirical point of view by Lang, Litzenberger, Madrigal, (1992). LLM show that the equilibrium returns stochastic process does not depend upon uncorrelated personal characteristics in the large economy Hellwig (1980). But the equilibrium volume stochastic process does depend upon such characteristics. Their evidence supports an information role for price when the noisy RE model is tested against a "Walrasian" model where price is not conditioned upon.

It is important to recognize a problem for such models in explaining the high contemporaneous correlation between volume and volatility. Precise signals lead to large trades but low returns volatility. To put it another way, large conditional volatility leads to small demands which lead to timid trading which leads to small volume. Ideas how to fix this conflict between data and theory are worth some discussion.

(VI) What role does interconnective structure (and "connectionist learning") of the economy play in breakdowns of the Law of Large Numbers and the Central Limit Theorem in large economies which translate into macroeconomic effects such as blowoffs and crashes ? Is there an econometrically usable impact on economic dynamics of intensity of choice of agents across actions, incentives to do high intensity choice, and degree of correlatedness of agent characteristics in a Follmer type ADM GE setting ?

(VI.1) "Strategic 'Backing Off' from Rationality"

Parameterization of the intensity of choice may be a useful way to "back off" from rationality which allows access to the large econometric literature on Random Utility Models. Sargent's Arne Ryde Lectures have stressed a strategic backoff from rationality may be necessary to explain facts such as the large observed stock trading volume and the strong persistence of such volume. The literature on common knowledge and no-trade theorems suggests that "too much" rationality may conflict with explanatory power of the theory. As Sargent shows, strict adherence to rationality and common knowledge dries up trading volume. Admati and Pfleiderer type work can explain volume bunching but all the models have a difficult time explaining blowoffs, crashes, and volume "bursting" unless one adds something like a Gennote/Leland "outside hedging function."

Perhaps Mean Field Theoretic ideas from the Interacting Particles Systems stochastic process literature can be used to parameterize correlated trader characteristics in such a way data can speak to whether their strength is strong enough to break a large economy central limit tendency (cf. Brock, 1993, supra). Breaking the LLN may be a fruitful way to study blowoffs, crashes, and volume bursting in this class of models. Furthermore a nesting framework can be set up to test whether the data are consistent with such correlations. A brief illustration of such techniques to "endogenize" such "discontinuous" phenomena is given below.

(VI.2) "Evolutionary Learning in Noisy RE Models"

Versions of noisy RE models can be built where $2^S = K$ beliefs/signal packages can be coded as bit strings and an adaptive evolutionary walk induced by performance of these beliefs can be studied in a temporary equilibrium setting. Common knowledge of the parametric structure of the system can be assumed but backoff from pure rationality can be done by (i) forcing agents to make forecasts of the number purchasing each type of belief/signal (ii) parameterizing choice intensity along the lines of the IID extreme value distributed Random Utility Model (RUM).

Example I: Let the global measure of each belief be something like conditional expected utility of each belief given expectations of each agent on the next period equilibrium number of each belief type. This induces a "temporary equilibrium reduced form" dynamics on the state space of fractions of each belief type. Since belief types are coded as bit strings this leads to use of genetic algorithm ideas popularized by Holland and the Santa Fe Institute.

Example II: Let a signal on next period's asset fundamental be available at cost $C > 0$. Let $+1$ code "signal purchase," n_+ the fraction that are $+1, -1$ code "no signal purchase," n_- the fraction that are -1. Given point expectations, n_+^e, n_-^e for next period's fractions, one can write for LLM (JPE, 1992) models, the goal functions (expected net utility or expected net profit) as $G_+(\cdot), G_-(\cdot)$.

Putting $m \equiv n_+ - n_-$, the RUM dynamics $n'_+ = \exp[\beta G_+(\cdot)]/Z, n'_- = \exp[\beta G_-(\cdot)]/Z$ lead to a dynamics, $m' = \tanh[\beta(G_+(m^e) - G_-(m^e))] \equiv F(m^e, \beta)$ which are closed once one sets $m' \equiv m_{t+1} m^e = m'$ for forward dynamics, ($m^e = m_{t-1}$, backwards dynamics). Note that there is no equilibrium in pure strategies ($\beta = \infty$) because if the signal makes a profit over cost everyone will buy it; but then it won't make a profit over cost, so no one will buy it. But there does, typically, exist $m^*, -1 < m^* < 1$, such that $G_+(m^*) = G_-(m^*)$ but this is a "mixed strategy."

Note that under plausible assumptions on the dissipation of signal rent the backwards tanh dynamics, $m' = F(m, \beta)$ decreases in m and has a fixed point $m^*_\beta \to m^*, \beta \to \infty$. The second iterate backwards dynamics, $m_{t+1} = F^2(m_{t-1}, \beta)$ increases in m_{t-1}. For $0 \leq \beta$ small enough there is just one fixed point for the second iterate dynamics, $m_{t+1} = F^2(m_{t-1}, \beta)$, but two new fix points appear for $\beta > \beta_c$, and F^2 converges to a shifted signum function as $\beta \to \infty$. By analysis of even and odd iterates the dynamics can be characterized for both forward and backwards dynamics in this simple case.

Hence, we see that simple versions of these models suggest that high intensity of choice across the bit strings can induce "freezing" into poor states which can be escaped by a reduction of rationality in the sense that intensity of choice β is lowered. When intensity of choice is infinite, equilibria will fail to exist in this model for the usual reasons stressed by Grossman and others. When one backs the intensity of choice away from infinity one gets existence of an equilibrium. As intensity goes to infinity the sequence of equilibria goes to the "mixed strategy" equilibrium even though no pure strategy equilibrium exists. For choice intensity large enough each of these equilibria near the mixed strategy equilibrium is (i) unstable in the backwards dynamics on type fractions but (ii) stable in the forwards dynamics on type fractions.

An interesting byproduct of this approach is another potentially useful way to study the well known instability problem of mixed strategy equilibria in games (e.g. V. Crawford (1989)).

If one introduces K components of the fundamental with ± 1 signal choices on each, one gets a "bit-string" model where an "ecology" of traders can evolve in this type of model. A rich dynamics should be available where results on "coarse grained bit dynamic" systems like Kurten's are suggestive for the high choice intensity cases.

Introduction of a Durlauf/Follmer type of lattice structure of "correlated" costs of signals on each of the K-components should create even richer dynamic behavior but the above appears rich enough to lead to endogenization of discontinuous behavior like blowoffs, crashes, and volume bursting. This kind of model can be rendered analytically and econometrically tractible by use of Mean Field Theoretic approximations to the lattice choice probabilities. In lattice type (Follmer/Durlauf IPS) models discontinuous, i.e.

"phase transition," phenomena tend to occur when the product of strategy choice intensity and "lattice" correlatedness is greater than unity.

"Ecologies" of trader types can be studied in this kind of model. For example, one can add "predatory strategies": One trader type "learns" by predating on a high quality/high cost signal type by getting a low cost signal on the high quality signal. If strategies are coded into bit strings and performance measures posited then evolutionary/IPS methods can be used to study their evolution. This kind of learning model may be more attractive than received learning models in economics.

The modelling of learning behavior has been criticized as "ad hoc." Someone once said that good economic learning models should contain no agent behavior that the modeller wouldn't be proud to call their own. Modeller egos would insure the extinction of dumb learning dynamics in journal articles. Movement towards using adaptive beliefs and adaptive learning strategies in a bitstring "Digital Darwinism" setting should blunt some of this criticism. Bernard Huberman's recent emphasis on differences between asynchronous and synchronous updating may play an important role in this kind of research. There are some blocks to empirical applications of this kind of research, however.

Many fields in economics do not have a rich enough data base to deal with theories that are profligately parameterized. "Anything Goes" theorems tend to deter empirical researchers from drawing on the theoretic work on GE. The literature on applied equilibrium (Kehoe, Shoven, Whalley, etal.) imposes discipline in their parameterizations.

The neural net literature (c.f. White et al. reviewed in Sargent's Arne Ryde lectures) has produced results on ability of sigmoid families to be "optimal" flexible functional forms. I.e. such families can approximate arbitrary functions in an economical way. Even with these tools the attachment of measures of precision of estimation of parameters and the execution of hypothesis testing becomes problematical. Turn now to one way to ameliorate this problem.

Dynamic versions of the bootstrap (e.g. Brock, Lakonishok, LeBaron (1992)) may be used in extensions of applied empirical GE to IGE/IGEI models. For example let goal directed agents inside the fitted model generate statistical tests with power against model alternatives that they have an economic interest and relative expertise over econometrician at finding. This is a way to find, those potential departures from the null model, that econometricians have a difficulty finding. In the Brock, LeBaron, Lakonishok, 1992 paper the statistics were gleaned off of technical trading rules for stock market trading. The goals of agents were trading profits. The bootstrap was used to generate null model distributions. I believe this kind of bootstrapping methodology may be useful in empirical work with IGE/IGEI models.

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Time and uncertainty

David CASS, University of Pennsylvania

[Like many others on these panels, I suspect, found it difficult to try and put "big think" down on paper; it has a tendency to appear fatuous, silly or pompous. Nonetheless, given the nature of this gathering, I have felt it incumbent to have something prepared beforehand. My comment is intended to be purely provocative.]

Several years ago I participated in a conference honoring John Hicks on the occasion of the 50th anniversary of the publication of *Value and Capital*. At that time I emphasized that his monograph was perhaps the first serious treatment of the implications of time and uncertainty for market allocation, and that many of the difficulties he had grappled with in working out that analysis were ameliorated by the innovation - in general equilibrium theory - involving the notion of states (or date-event-location), and state contingent actions and outcomes. I also had to confess that this powerful approach had been developed almost exclusively in the context of what Hicks called "perfect equilibrium" (a term later co-opted for one of the second-order refinements of Nash equilibrium), what we now call perfect foresight or rational expectations - and that I simply wasn't going to address the appropriateness of such methodology. Here, however, I find a very natural setting for opening this particular can of worms.

Frankly, I don't have anything especially profound or novel to say about the enormous informational requirements imbedded in the perfect foresight hypothesis. So all I will do is provide a framework for discussion, by sketching what you all already know, the simplest (general equilibrium) model in which the hypothesis can be formulated, and let its almost palpable implausibility speak for itself.

What is the most basic setup ? There are two periods, today and tomorrow, with uncertainty about tomorrow. This uncertainty is represented by the familiar catch-all, possible states of the world. There are also a number of households who have endowments of goods today and anticipate (correctly) endowments of goods tomorrow, and who trade on spot goods and credit or financial markets today and anticipate (given their previously undertaken financial obligations) trading on spot goods markets tomorrow. Finally, these households observe (and hence know) spot goods prices today, but only anticipate spot goods prices tomorrow, contingent on the realized state of the world. (Notice that here the sense of what one means by "anticipate" shifts somewhat; for example, tomorrow's endowments are essentially a part of the description of the states of the world, the possible spot goods prices are not.) The ultimate question is, what spot goods prices should (do?) households anticipate ? The answer under the perfect foresight hypothesis is, spot goods prices such that

- today's markets clear, and
- given the realized state of the world, tomorrow's spot goods market would (will ?) clear at the anticipated prices.

But how in the world can households come up with such fantastically accurate predictions ? Are there less implausible (more acceptable ?) but equally tractable hypotheses ?

Time and uncertainty

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One important application of the general equilibrium theory that would be completely uninteresting if it had neither uncertainty nor the passage of time is its application to financial markets. The past two decades witnessed a tremendous advancement in our understanding of financial markets. This advancement has led to an explosion of financial innovations and has coined the term "financial engineering." The insight that underlies this tremendous progress is attributable to the observation made by Black and Scholes (1973) and Merton (1973) that when financial markets are utilized in a dynamic way, they become production facilities capable of manufacturing financial payoffs that are highly nonlinear functions of the inputs. Thus, a higher frequency of trading in a fixed number of securities is a substitute for a larger number of tradable securities. The root of this insight, however, is found in Debreu (1959) and Arrow (1964). The former provides us with a framework to think about and a paradigm to theorize time and uncertainty. The latter was the first to point out the role of securities markets as vehicles through which resources are transferred across time and states of nature.

Indeed, Arrow (1964) shows that the existence of reconvening spot markets for consumption commodities, together with financial markets at the initial date, can reduce the number of time-state contingent commodity contracts needed to allocate resources efficiently when there are more than one commodity. The insight of Black and Scholes (1973) and Merton (1973) goes on step further and suggests that there can still be a dramatic reduction of the time-state contingent commodity contracts needed for efficiency, even when there is only one commodity if there are reconvening financial markets for "long-lived" securities. This insight was later formalized by Duffie and Huang (1985) in which all time-state contingent contracts paying the numeraire can be dynamically "manufactured" or "duplicated" by trading in a fixed set of long-lived securities. Consequently, even though the markets are not Arrow, Debreu and McKenzie complete, they can be completed by dynamic trading. This notion of completeness is termed "dynamic completeness."

In applications of the notion of dynamic completeness, financial firms have built models of dynamic securities markets under uncertainty that imply dynamically complete markets. Complicated financial payoffs that suit corporations' or investors' needs are

* I would like to thank Ho-Mou Wu for helpful discussions.

created using dynamic trading rules. One issue arises: How does one calculate the prices and duplicating strategies over time of these complicated financial payoffs most efficiently ? Cox and Ross (1976) and Harrison and Kreps (1979) are the two most important papers in this endeavor. They show that the prices of financial payoffs over time can be made into martingales by a normalization and a change of probability. Once the probability under which normalized prices are martingales is found, one can systematically calculate the prices of all financial payoffs over time by either evaluating expectations or by solving differential equations. The duplicating strategy for a financial payoff turns out to be directly related to certain derivatives of its price, and can be gotten once the price is calculated. This procedure is the aforementioned "financial engineering."

The idea that reconvening markets are substitutes for a larger number of markets is certainly a powerful one. But it comes with costs. When all time-state contingent commodity contracts are available, there is no need for the markets to reopen (provided that agents' preferences are time consistent). Thus, agents do not need to have correct expectations for future prices. Reconvening markets are substitutes for a larger number of markets when agents have rational expectations about future prices. In the case of Arrow (1964), agents need to have rational expectations for the commodity prices in the spot markets in the future. In the context of Black and Scholes, and Merton, in addition to what is required in Arrow (1964), it is also demanded that agents have rational expectations about the price processes of long-lived securities. One can substitute the frequency of trading for the number of markets if at the same time one demands more rationality from agents. Essentially, agents will have to know the price processes of the long-lived securities in that they will have to know the prices of the long-lived securities in every state of nature and at any future time.

In practice, however, the kind of rationality we require for reconvening markets to substitute for the number of markets is difficult to come by, even for the most financially resourceful institutions. These institutions understand that they don't fully comprehend the relation between future prices and the time-state contingencies. Indeed, they are aware that they don't even know what the complete menu of future contingencies is. Nevertheless, these institutions utilize whatever statistical techniques they have at their disposal to "estimate" their incomplete models of security prices for the dynamic production of financial payoffs. They also take precautions to guard against contingencies unforeseen by these models.

Interestingly, these institutions have learned a great deal from the unbounded rational theories of financial markets, and at the same time have understood their own limitations in applying these theories. On the one hand, these unbounded rational theories of financial markets have significantly changed the practice of the financial industry; on the other hand, their success in practice also exemplifies their limitations.

In the context of dynamic equilibrium theory under uncertainty, we should ask: Can one build models where agents don't fully comprehend the relation between the prices and the time-state contingencies and they use statistical techniques to try to delineate this relation the best way they can ? And, going one step further, can one build models where the set of future contingencies is not completely known ? These two questions are certainly related. If one does not know the set of future contingencies, then of course one does not know the price processes !

The first question seems easier than the second as at least it lies in the realm of classical decision theory. For example, in some very (maybe too) stylized models, one can use linear filter to best estimate the relation between prices and the time-state contingencies. Then the optimal decision of an agent will be made by substituting this best estimate for the true and unknown relation. To address the second question, one would need a decision theory that accounts for the possibility that one does not know the complete menu of future contingencies. Some exploratory work has been done in this area but more will be needed (see Kreps (1992)).

In sum, research during the past four decades on the dynamic general equilibrium theory under uncertainty has profoundly changed the way we think about financial markets, and the way the financial industry conducts its business. This success also points out the direction where future improvement may lie, for which some foundational work in the theory of choice will be needed.

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Time and uncertainty in applied general equilibrium

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In February 1992 the U.S. International Trade Commission invited all economists who had constructed models to analyze the impact of the North American Free Trade Agreement (NAFTA) on the economies of Canada, Mexico, and the United States to present their work at a public conference in Washington, DC. Summaries of the research presented at this conference are contained in U.S.I.T.C. (1992). Of the twelve modeling efforts presented, all but one of them employed applied general equilibrium methods. (The exception was a hybrid input-output/macro model.)

Over the past twenty years static applied general equilibrium models of the sort pioneered by Johansen (1960) and Shoven and Whalley (1972) have become increasingly popular as a tool for policy analysis. For the most part, models of this sort have stressed issues involving sectoral disaggregation and have ignored issues involving time and uncertainty. Parallel to the development of these disaggregated static models has been the development of highly aggregated models that have stressed what were traditionally identified as macroeconomic issues, issues that crucially involve time and uncertainty. Probably the most well known branch of this sort of research utilizes what has become known as real business cycle models. See Danthine and Donaldson (1993) for a recent survey of this research, which was pioneered by Kydland and Prescott (1982).

This paper assesses the usefulness of applied general equilibrium models for evaluating the impact of a major policy change like the NAFTA. Drawing heavily on Kehoe (1994), it stresses the need to add modeling time and uncertainty to the disaggregated static models, or alternatively the need to add sectoral disaggregation to aggregated dynamic, stochastic models.

Although a large amount of energy and resources that have gone into constructing applied general equilibrium models and using them to perform policy analyses over the past two decades, little effort has gone into evaluating the performance of such models after such policy changes have actually taken place. Only by showing that a model can replicate and, to some extent, predict the principal developments that occur in the economic system that it intends to represent can we justify the effort put into a large-scale quantitative model.

One approach to empirically validating a model is to investigate how well it can do in tracking the impact of policy changes and exogenous shocks after these shocks have occurred. Another approach is to compare predictions with actual outcomes. The problem with this latter approach is the actual data can be significantly affected by unforseen exogenous shocks that occur concurrently with the foreseen policy change. Since applied general equilibrium models have very explicit structures, however, it should be able to disentangle the impacts of different shocks and policy changes using the model.

Kehoe, Polo, and Sancho (1992) take a step in this direction. They assess the performance of a model of the Spanish economy built in 1984-85 to analyze Spain's 1986 entry into the European Community. Table 1 shows the percentage changes in relative prices that actually took place in Spain between 1985 and 1986 in its first column. The second column shows the model predictions. In each case the prices have been deflated by an appropriate index so that a consumption weighted average of the changes sums to zero: these sorts of models are designed to predict changes in relative prices, not those in price levels. Notice that the model fares particularly badly in predicting the changes in the food and nonalcoholic beverages sector and in the transportation sector. There are obvious historical explanations for these failings: in 1986 the international price of petroleum fell sharply and poor weather caused an exceptionally bad harvest in Spain. Incorporating these two exogenous shocks into the model yields the results in the third column in Table 1, which correspond remarkably closely to the actual changes.

Kehoe, Polo, and Sancho perform similar exercises comparing model results, both with and without the exogenous shocks, with the actual data for changes in industrial prices, production levels, returns to factors of production, and major components of GDP. In general, the unadjusted model does somewhat better in predicting the actual changes in these variables, and the adjusted model does somewhat worse. Overall, however, the exercise shows that this sort of model can do a good job in predicting the changes in relative prices and resource allocation that result from a major policy change.

The major policy change that occured in Spain in 1986 was a tax reform that converted most indirect taxes to a value added tax, in accord with EC requirements. The process of trade liberalization began in 1986 and is captured in the model. The work on Spain did not, however, concentrate on trade issues. Consequently, the results from the Spanish model do not help us much to discriminate among the various model structures currently being used to analyze the impact of the NAFTA. One way to evaluate these different modeling strategies would be to modify the Spanish model to incorporate alternative assumptions about product differentiation, returns to scale, and market structure. Alternative versions of the model could then be used to "predict" the impact of the trade liberalization that has occurred in Spain in recent years and the results compared with the data. Similarly, and more to the point, the different models used to analyze the impact of the NAFTA could be evaluated by using them to "predict" the impact of the policy changes and exogenous shocks that have buffeted the three North American economies over the past decade. In any case, now that NAFTA has been is implemented, it will be possible in less than a decade to go back and see which models performed better in predicting its effects.

As I have stressed elsewhere, Kehoe (1994), the dynamic impact of the NAFTA is likely to dwarf the impact analyzed by static applied general equilibrium models such as those of Brown, Deardorff, and Stern (1991), Cox and Harris (1992), and Sobarzo (1992). Perhaps the major impact of the entry into the EC on the Spanish economy, for example, has been a sharp increase in foreign investment closely related to increases in GDP and imports. From 1980 to 1985 investment in Spain actually fell by 1.0 percent per year, as shown in Table 2. In contrast, since entry into the EC in 1986, investment has grown by 10.0 percent per year on average. Similarly, GDP growth has increased from a 1.4 percent average in 1980-1985 to 4.1 percent in 1986-1991, and import growth has increased from 1.3 percent to 14.2 percent. A similar pattern can be seen to emerge in Mexico with the *apertura*, or openness policy, that began to take effect in 1988 and 1989.

The NAFTA is expected to reinforce this pattern, with substantial increases in GDP fueled by foreign and domestic investment and with even more substantial increases in imports leading to large trade deficits. In both Spain and Mexico, many, if not most, of the current discussions of economic openness in the press, among academic analysts, and in policy circles concentrate on the sustainability of these investment booms and the corresponding trade deficits. It is worth stressing two points about investment flows, however: First, differences in capital-labor ratios between Mexico and its northern neighbors cannot be the sole explanation of the large differences in output per worker between these countries. (See Lucas (1990) for a discussion and calculations similar to those below.) Consequently, simply equalizing capital-labor ratios cannot be the solution to the problem of eliminating income differences. Second, when modeling the savings and investment decisions that determine capital flows, we need to take into account the significant differences in age profiles of the population between Mexico and its neighbors. This point is discussed further below.

To illustrate the point that differences in capital-labor ratios cannot explain the differences in output per worker observed in Mexico and the U.S., we carry out come simple calculations using aggregate production functions. Suppose that each economy has the same production function

$$Y_j = \gamma N_j^{1-a} K_j^a$$

where Y_j is GDP, N_j is the size of the work force, and K_j is capital. In per capita terms, where $y_j = Y_j/N_j$ and $k_j = K_j/N_j$, this becomes $y_j = \gamma k_j^a$. The net return of capital is

$$r_j = a\gamma k_j^{a-1} - \delta$$

where δ is the depreciation rate. Using the World Bank's (1992) estimates of real GDP per capital and Summers and Heston's data for workers per capita, we can calculate real output per worker in Mexico in 1990 as \$17,500 and that in the U.S. as \$43,800. Suppose that $a = 0.3$, which is roughly the capital share of income in the U.S. Then to explain this difference in output per worker, we need capital per worker to be larger than that in Mexico by a factor of 21.3,

$$\frac{k_{\text{us}}}{k_{\text{mex}}} = \left[\frac{y_{\text{us}}}{y_{\text{mex}}}\right]^{1/a} = \left[\frac{43,800}{17,500}\right]^{1/0.3} = (2.5)^{1/0.3} = 21.3.$$

Suppose that $\delta = 0.05$ and $r_{\text{us}} = 0.05$, which are roughly the numbers obtained from calibration. Then the net interest rate in Mexico should be 16 times that in the U.S.,

$$r_{\text{mex}} = (r_{\text{us}} + \delta)\left[\frac{k_{\text{us}}}{k_{\text{mex}}}\right]^{1/a} - \delta = 0.10(21.3)^{0.7} - 0.05 = 0.80.$$

During the period 1988-90 the real return on bank equity in Mexico (and banks are the major source of private capital in Mexico) averaged 28.2 percent per year, as compared to 4.7 percent in the U.S. (see Garber and Weisbrod 1991). Since 28 percent is far less than the 80 percent that we would expect if the difference in capital-labor ratios were the principal determinant of the difference in output per worker between Mexico and its neighbors, we must look elsewhere for this determinant.

There are at least two objections that can be raised to the above calculations. First, a comparison based on per capita GDP in U.S. dollars using the exchange rate to convert pesos into dollars would suggest that $y_{\text{us}}/y_{\text{mex}}$ is much larger than 2.5, about 5.4. Second, calibrating the capital share parameter a using Mexican GDP data would yield a larger value, about 0.7. These two objections work in opposite directions, however, and our calculations can be defended as being in a sensible middle ground: income comparisons based on exchange rate conversions neglect purchasing power parity differentials; per capita comparisons rather than per worker comparisons neglect demographic differences; much of what is classified as net business income in Mexico is actually returns to labor, and so on.

Moreover, that differences in capital per worker cannot be the sole explanation of differences in output per worker across countries in a more general point. It is supported both by historical evidence, such as that of Clark (1987), and by even more extreme examples of differences in output per worker: Real GDP per worker in Haiti in 1990, for example, was 4.6 percent of that in the U.S. The same sort of calculations as those above would suggest that interest rates in Haiti should be over 13,000 percent per year if differences in the capital-labor ratio were the sole explanation of the differences in output per worker. Furthermore, historical evidence does not indicate that Mexico has always been starved of funds for investment. The problem has often been that investments abroad, particularly in the U.S., have been more attractive. Between 1977 and 1982, for example, \$17.8 billion of private investment flowed into Mexico while \$18.7 billion flowed out (see Garcia-Alba and Serra-Puche 1983, p. 45).

Although capital flows cannot provide all of the answers to Mexico's problems, they are important. If capital flows could lower the net interest rate in Mexico from 28 percent per year to 5 percent, we would estimate that the capital labor ratio in Mexico would increase by a factor of about 5.5

$$\frac{k'_{\text{mex}}}{k_{\text{mex}}} = \left[\frac{0.28+\delta}{0.05+\delta}\right]^{1/(1-a)} = 5.5.$$

This would increase Mexican output per worker to about \$29,200, which would close the current gap with the U.S. level by about 44 percent.

A dynamic applied general equilibrium model would be an ideal tool for analyzing the capital flows that would result from the NAFTA. With some sectional disaggregation, such a model could account for differences in total factor productivity in the U.S. and Mexico that differ widely across sectors. Total factor productivity in some sectors in Mexico is similar to that in the U.S., in others it is much lower. The calculations of the impact of capital inflows reported above are based on aggregate production functions and ignore these differences. A disaggregated model would enable us to capture the various impacts that capital inflows have on different sectors.

If a model is to explain the impact of large capital inflows on the Mexican economy, it should be able to answer the question, if the post-NAFTA interest rate in Mexico is to be so much lower than the pre-NAFTA interest rate, why is the pre-NAFTA rate so high ? One possible answer is that a high interest rate in Mexico is the result of closed capital markets and of inefficient, oligopolistic financial intermediaries. If this is the case, we would want to model imperfect competition in the financial services sector and to explicitly model the way in which the NAFTA would lower the interest rate.

Another potential answer is that the gap between the pre-NAFTA interest rate in Mexico and the U.S. interest rate represents a risk premium: international investors demand a higher rate in Mexico because they fear that a financial collapse and maxi-devaluation like that which occured in 1982 would wipe out much of their investment. By locking Mexico and its two northern neighbors into policies that would help guarantee economic stability in Mexico, the NAFTA would lower this risk premium and thereby lower the interest rate. Foreign investment in Mexico has increased dramatically in recent year, as seen in Figure 1. Some of this increase has been due to the liberalization of Mexican laws regarding such investment, and some has undoubtedly been due to improvements in expectations about Mexico's economic future.

It is possible to model the process by which the NAFTA would lower the risk premium in a simple way. Figure 2 depicts an event tree for a dynamic, stochastic general equilibrium model in which there is a probability π_{ct} of a financial collapse in period t and a probability $1 - \pi_{ct}$ of no financial collapse. In simulations, we can concentrate on the path in which no financial collapse actually occurs. Even so, in principle, we have to model that would occur at every node of this event tree. This subjects us to the "curse of dimensionality" associated with an expanding state space typical in this type of model. To simplify the analysis, however, we can model what happens if a financial collapse occurs in a simple enough way so that we do not have to move further out on branches in which a financial collapse occurs to compute the equilibrium outcomes. Even though we do not need to model in great detail what happened if a financial collapse occurs, lowering its probability π_{ct} can have a significant impact on equilibrium outcomes along the branch of the tree where there is no collapse. To make this approach useful, we need to model the interaction of π_{ct} and the NAFTA in a way that is tractable but also captures the impact of the NAFTA on economic stability in Mexico.

To successfully account for capital flows, a dynamic general equilibrium analysis of the NAFTA needs to model consumers' saving decisions. In modeling savings decisions in Canada, the U.S., and Mexico, we must take into account demographic differences among the countries. Figure 3 illustrates the stark contrast in the population growth experiences of the U.S. and Mexico. These differences in population growth manifest themselves in differences in age structures of population: While the populations of Canada and the U.S. are currently aging as the postwar baby boom generation reaches middle age, half the population of Mexico is currently age 19 or younger.

These differences would be very important in an overlapping generations context in which life-cycle consumers dissave when young and build up their human capital, save during the middle of their lives, and dissave again when old during retirement. An example of an applied general equilibrium model with overlapping generations is that of Auerbach and Kotlikoff (1987), who model a single country with a single production sector. A model with similar dynamic structure but with several countries and multiple production sectors could be used to capture the impacts of the NAFTA on capital flows in North America.

Modeling demographic differences in an overlapping generations framework would be especially important in a model in which the accumulation of human capital, as well as that of physical capital, plays an important role. The alternative modeling strategy is to assume that a bequest motive links generations in such a way as to produce families that act as if they were infinitely lived consumers. Empirical evidence does not seem to clearly favor one approach over the other: On one hand a large percentage of wealth seems to be the result of bequests, Kotlikoff and Summers (1981). It would be essential to account for this phenomenon in Mexico where wealth is very concentrated. On the other hand, while bequests may be important in some families, they are not important in others, Altonji, Hayashi, and Kotlikoff (1992). Consequently, an applied dynamic model should be able to incorporate both families linked by bequests and other families who engage in life cycle savings. A theoretical version of such a model has been developed by Escolano (1992).

As we have seen, a low capital-labor ratio cannot be the only factor in explaining the low level of output per worker in Mexico compared to that in a country like the United States. We must look elsewhere for explanations for the differences in levels of output per worker. It is here that the new, endogenous growth literature, which follows Romer (1987) and Lucas (1988) and focuses on endogenous technical change, is able to provide potential answers. Although this literature is still at a tentative, mostly theoretical level, we can use preliminary empirical work at an aggregate level to estimate the impact of free trade on growth rates in Mexico.

Although our calculations are fairly crude, they suggest that, like the capital accumulation effects, the growth effects of the NAFTA could dwarf the static effects found by more conventional applied general equilibrium models. Similar kinds of suggestive calculations are done to estimate the dynamic gains from the European Community's 1992 Program by Baldwin (1992). Unlike Baldwin's analysis, however, the results presented here are based on theories and empirical estimates that deal with trade directly. Baldwin obtains his numbers by multiplying estimates of static gains from trade obtained by other researchers by a multiplier derived from a highly aggregated growth model with dynamic increasing returns but without any explicit role for trade.

Although endogenous growth literature is still at a tentative stage, the intuition behind it is fairly simple: economic growth is spurred by the development of new products. New product development is the result of learning by doing, where experience in one product line makes it easier to develop the next product in the line, and of direct research and development. On the final product side, increased openness allows a country to specialize more, achieving a larger scale of operations in those industries in which it has a comparative advantage. On the input side, increased openness allows a country to import many technologically specialized inputs to the production process without needing to develop them itself.

Consider the following simple framework, as presented by Backus, Kehoe, and Kehoe (1992): Output in an industry in some country depends on inputs of labor and capital, country and industry specific factors, and an experience factor that depends, in turn, on previous experience and output of that industry in the previous period. Keeping constant the rates of growth of inputs, the crucial factor in determining the rate of growth of output per worker is the rate of growth of the experience factor. Output per worker grows faster in industries in which this experience factor is higher. The level of growth of output per worker nationwide is a weighted average of the rates of growth across industries. One way increased openness promotes growth is that it allows a country to specialize in certain product lines and attain more experience in these industries.

Increased openness also allows a country to import more specialized inputs to the production process. Stokey (1988) and Young (1991) have proposed models in which new product development is still the result of learning by doing, but where the primary impact of learning by doing is in the development of new, more specialized inputs. Trade allows a country to import these inputs without developing them itself. Aghion and Howitt (1992), Grossman and Helpman (1989), Rivera-Batiz and Romer (1989), and others have proposed similar models where it is research and development that leads to the development of new products. (Here, of course, the relationship of trade and growth is more complicated if one country can reap the benefits of technological progress in another country by importing the technology itself without importing the products that embody it.)

The most interesting aspect of this theory is the perspective it gives us on trade and growth. The natural interpretation of the theory that emphasizes specialization in final products is that technology is embodied in people and is not tradeable. Trade may influence the pattern of production, including both the scale of production and the pattern of specialization, and in this way affect growth. In the model with specialized inputs, technology is embodied in product variety, and there is a more subtle interaction between trade and growth: increases in the number of varieties of intermediate goods raise output. If these varieties are freely traded, a country can either produce them itself or purchase them from other countries. By importing these products a small country can grow as fast as a large one. When there is less than perfectly free trade in differentiated products, we might expect to find that both scale and trade in differentiated products are positively related to growth.

Using cross-country data from a large number of countries over the period 1970 to 1985, Backus, Kehoe, and Kehoe (1992) analyze the determinants of growth. Various other researchers have used similar cross-country data sets to estimate the parameters of endogenous growth models; see Levine and Renelt (1992) for a survey. Typically, researchers in this area find that their results are very sensitive to the exact specification of the model and the inclusion or exclusion of seemingly irrelevant variables. Backus, Kehoe, and Kehoe find, however, that, in explaining rates of growth of output per worker in manufacturing, results related to the theory sketched out in this section are remarkably robust. Using their methodology, we can estimate some parameters for a model in which both specialization in final output and the ability to import specialized inputs foster growth. Details concerning the data sources and methodology can be found in Backus, Kehoe, and Kehoe.

Consider a relationship of the form

$$g(\overline{y}^j) = a + \beta_1 \log \overline{Y}^j + \beta_2 \log \sum_{i=1}^{I} (\overline{X}_i^j / \overline{Y}^j)^2 + \beta_3 \log \overline{GL}^j + \beta_4 \log y^j$$
$$+ \beta_5 \,\mathrm{PRIM}^j + \varepsilon^j .$$

Here $g(\overline{y}^j)$ is average yearly growth of manufacturing output per worker in percent form from 1970-1985; $\overline{Y}^j$ is 1970 manufacturing output; $\sum_{i=1}^{I}(\overline{X}_i^j/\overline{Y}^j)^2$ is a specialization index for exports at the three digit S.I.T.C. level; $\overline{GL}^j$ is the 1970 Grubel-Lloyd index of intra-industry trade, Grubel and Lloyd (1975); y^j is 1970 per capita income; and PRIM^j is 1970 primary school enrollment rate. Bars above the variables indicate that the variable deals with the manufacturing sector only; the specialization index and the Grubel-Lloyd index, for example, are computed for manufacturing industries only.

We include total manufacturing output and the specialization index to account for the impact of specialization in production of final goods. One motivation for using export data is that specialization is most important in the export sector. Another motivation is purely practical: the trade data permit a more detailed breakdown of commodities, and the export specialization index can be thought of as a proxy for the total production specialization index: if exports are proportional to outputs, then $\overline{X}_i^j = \varepsilon \overline{Y}_i^j$ and $\sum_{i=1}^{I}(\overline{X}_i^j/\overline{Y}^j)^2 = \varepsilon^2 \sum_{i=1}^{I}(\overline{Y}_i^j/\overline{Y}^j)^2$, and the two indices are proportional. The Grubel-Lloyd index is included, because it captures, in a loose way, the ability of a country to trade in finely differentiated products, which our theory implies is important for growth. We include initial per capital income and the primary enrollment rate partly because they are widely used by other researchers in this area, such as Barro (1991), and partly because they may be relevant to our theory: the inclusion of per capita income allows for less developed countries, which are playing catch-up, to face different technological constraints. The inclusion of the enrollment rate allows for differences in countries' ability to profit from learning by doing because of differences in levels of basic education.

A regression of the above relationship yields

$$g(\overline{y}^j) = \underset{(5.686)}{2.602} + \underset{(0.259)}{0.743}\log \overline{Y}^j + \underset{(0.113)}{0.309}\log \sum_{i=1}^{I}(\overline{X}_i^j/\overline{Y}^j)^2$$

$$+ \underset{(0.410)}{0.890}\log \overline{GL}^j - \underset{(0.799)}{0.172}\log y^j + \underset{(2.271)}{2.421}\text{PRIM}^j$$

$$\text{NOBS} = 49 \qquad R^2 = 0.479.$$

(The numbers in parentheses are heteroskedasticity-consistent standard errors.) Notice that in this regression the coefficients all have the expected signs, and that the first three variables, total manufacturing output, the specialization index, and the Grubel-Lloyd index, are statistically significant.

To illustrate the dramatic impact of trade liberalization possible in a dynamic model that contains the endogenous growth features discussed in this section, let us suppose that the NAFTA allow, Mexico to increase its level of specialization in production of final manufactured goods and imports of specialized inputs. The average values over 1970-85 of the specialization indices and Grubel-Lloyd indices for the three North American countries are listed below. The values of the same indices for South Korea, a country with about the same output per worker as Mexico, are also included for comparison.

	$\sum_{i=1}^{I}(\overline{X}_i^j/\overline{Y}^j)^2$	$\overline{GL}^j$
Canada	7.10×10^{-2}	0.638
Mexico	5.93×10^{-4}	0.321
U.S.	1.92×10^{-3}	0.597
Korea	5.43×10^{-2}	0.362

Suppose that free trade allows Mexico to increase its specialization index to 1.0×10^{-2} and its Grubel-Lloyd index to 0.6. Dramatic increases of this sort are possible: in 1970, for example, Ireland had a Grubel-Lloyd index for manufactured goods of 0.150; in 1980, after having joined the European Economic Community in 1973, this index was 0.642.

Over the same 1970-1980 period earnings per worker in Ireland grew at a 4.1 percent annual rate.

Using the above regression results, we would estimate the increase in the growth rate of manufacturing output per worker of 1.43 percent per year

$$
\begin{aligned}
1.430 &= 0.309 \log \left[\frac{1.00 \times 10^{-2}}{5.93 \times 10^{-4}}\right] + 0.890 \log \left[\frac{0.600}{0.321}\right] \\
&= 0.873 + 0.557.
\end{aligned}
$$

It is clear that much is at stake in the issues discussed here. Suppose that Mexico is able to increase its growth rate of output per worker by an additional 1.43 percent per year by taking advantage of both specialization and increased imports of specialized intermediate and capital goods. Then after 30 years, its level of output per worker would be more than 50 percent higher than it would otherwise have been. By way of comparison, if Mexico's output per worker were 50 percent higher in 1988 than it was, then output per worker in Mexico would be about the same as that in Spain (this comparison uses Summers and Heston's 1991 data). Our earlier calculations suggested that Mexico could increase its output per worker by about 66 percent by increasing its capital per worker until the rate of return on capital is equal to that in the U.S.

Admittedly, these calculations are very crude, but they suggest that there is a significant impact of increased openness on growth through dynamic increasing returns. Furthermore, the dynamic benefits of increased openness dwarf the static benefits found by more conventional applied general equilibrium models. Obviously, this is an area that requires more research, and even a crude disaggregated dynamic general equilibrium model of North American economic integration would make a substantial contribution.

Table 1

Comparison of Spanish model's prediction with the data

(Percentage change in relative price)[1]

Sector	Actual 1985-86	Model	Adjusted Model
1. Food and nonalcoholic beverages	1.8	-2.3	1.7
2. Tobacco and alcoholic beverages	3.9	2.5	5.8
3. Clothing	2.1	5.6	6.6
4. Housing	-3.2	-2.2	-4.8
5. Household articles	0.1	2.2	2.9
6. Medical services	-0.7	-4.8	-4.2
7. Transportation	-4.0	2.6	-6.6
8. Recreation	-1.4	-1.3	0.1
9. Other services	2.9	1.1	2.8
Weighted correlation with 1985-1986[2]	1.000	-0.079	0.936

Source: Kehoe, Polo, and Sancho (1992).

[1]Change in sectoral price index deflated by appropriate aggregate price index.

[2]Weighted correlation coefficients with actual changes 1985-86. The weights used are 1) 0.2540, 2) 0.0242, 3) 0.0800, 4) 0.1636, 5) 0.0772, 6) 0.0376, 7) 0.1342, 8) 0.0675, 9) 0.1617; these are the consumption shares in the model's benchmark year, which is 1980.

Table 2

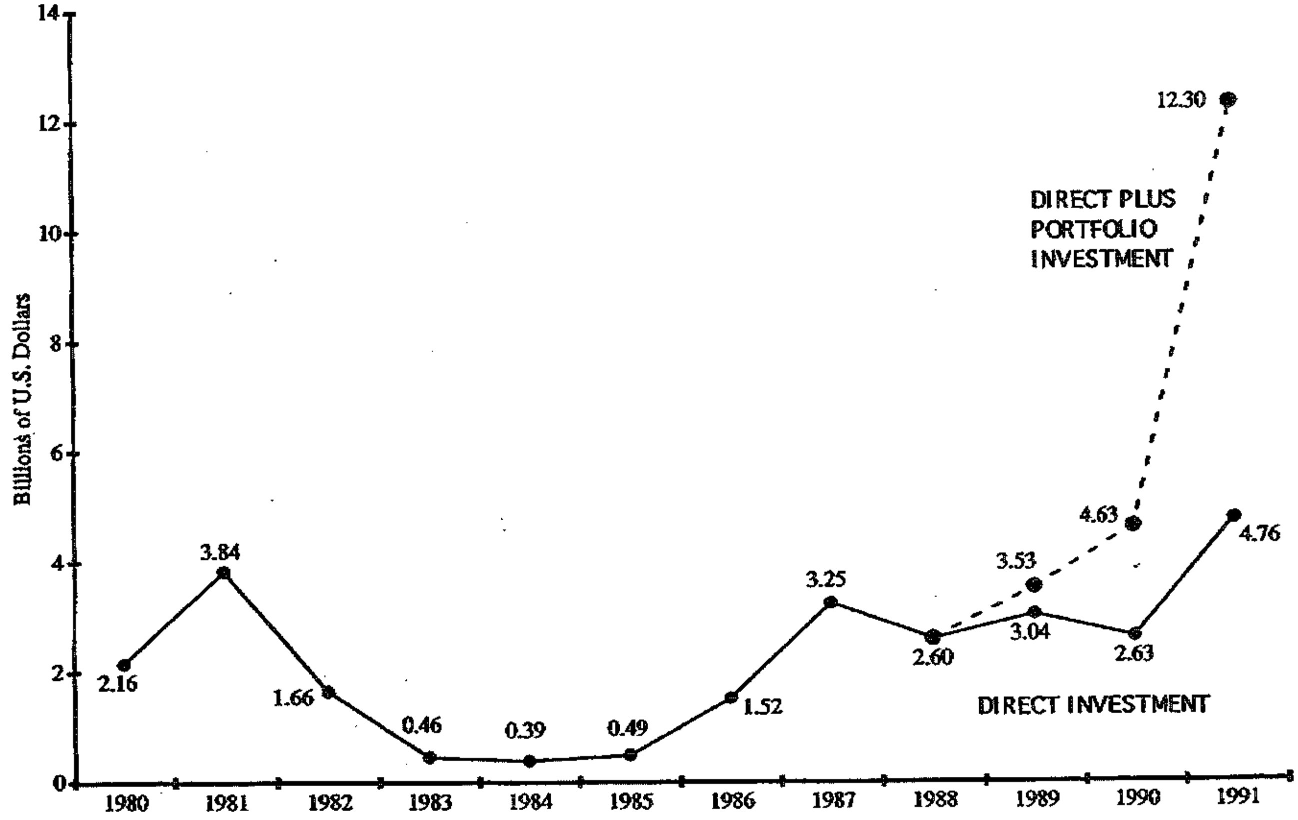
Growth rates of GDP in Mexico and Spain 1980-1991

(Real change in percent per year)

	Mexico				Spain			
Year	GDP	Investment	Exports	Imports	GDP	Investment	Exports	Imports
1980	8.3	14.9	6.1	31.9	1.5	1.3	0.6	3.8
1981	8.8	16.2	11.6	17.7	-0.2	-3.3	8.4	-4.2
1982	-0.6	-16.8	21.8	-37.9	1.2	0.5	4.8	3.9
1983	-4.2	-28.3	13.6	-33.8	1.8	-2.5	10.1	-0.6
1984	3.6	6.4	5.7	17.8	1.8	-5.8	11.7	-1.0
1985	2.6	7.9	-4.5	11.0	2.3	4.1	2.7	6.2
1986	-3.8	-11.8	5.3	-12.4	3.3	10.0	1.3	16.5
1987	1.7	0.1	10.1	2.0	5.6	14.0	6.1	20.2
1988	1.4	5.8	5.0	37.6	5.2	14.0	5.1	14.4
1989	3.1	6.5	3.0	19.0	4.8	13.8	3.0	17.2
1990	3.9	13.4	5.2	22.9	3.6	6.9	3.2	7.8
1991	3.6	8.5	5.1	16.6	2.4	1.6	8.4	9.4

Source: Instituto Nacional de Estadística Geografía e Informática, Mexico and Instituto Nacional de Estadistica, Spain.

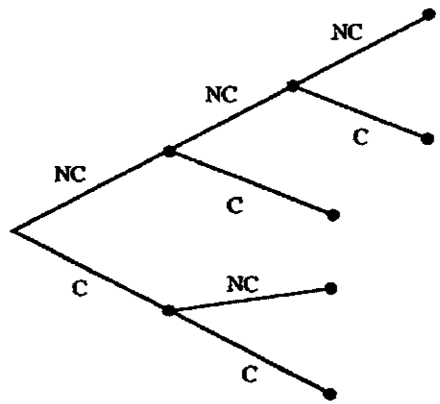
Foreign Investment in Mexico



Source: Banco de Mexico

Figure 1

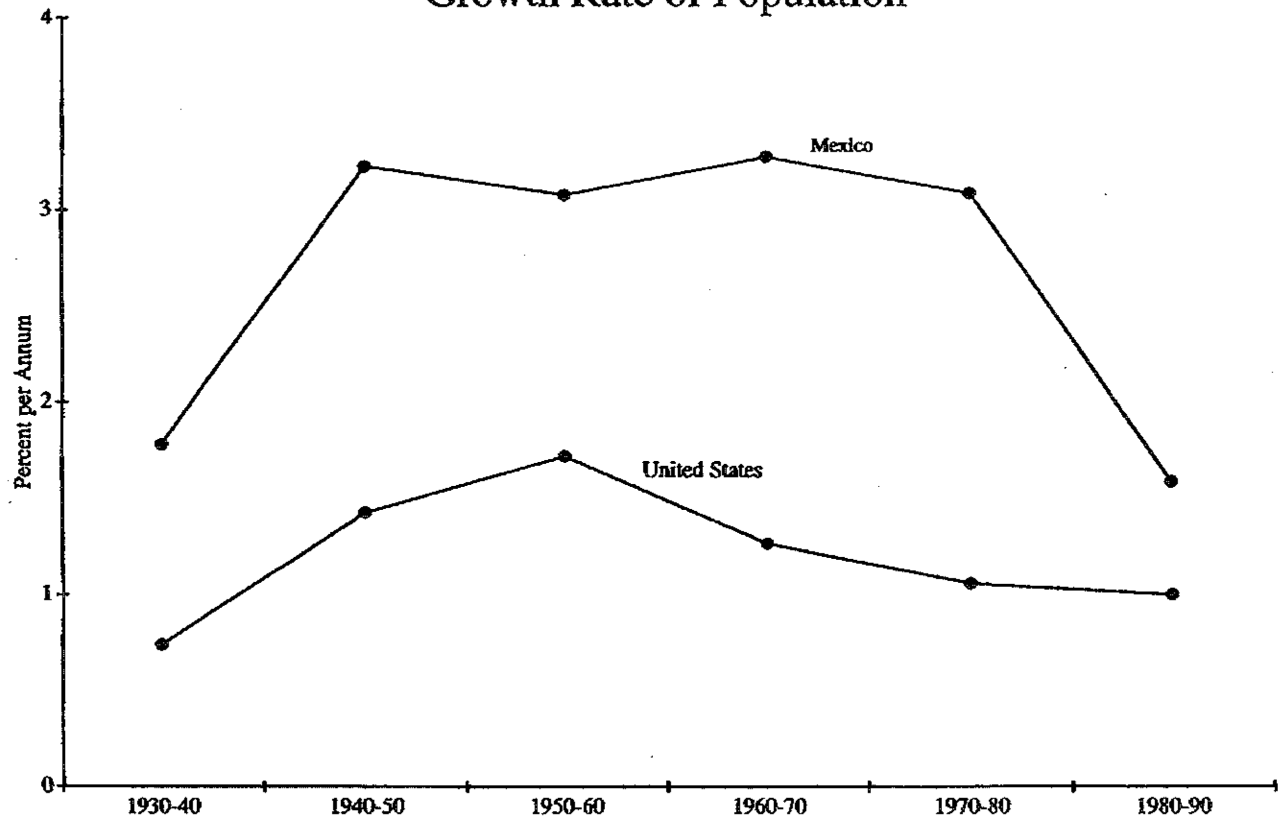
Introduction of Uncertainty



C = Financial Collapse
NC = No Collapse

Figure 2

Growth Rate of Population



Sources: NAFINSA and USDC, Bureau of the Census.

Figure 3

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General competitive equilibrium: time and uncertainty

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A market economy is a collection of individuals who produce and exchange commodities and assets according to their preferences.

The work of Arrow and Debreu (1954) and McKenzie (1954) established conditions for the existence of competitive equilibrium allocations.

The existence of competitive equilibrium allocations is complemented by their desirability, Arrow (1951), Debreu (1951), the identification of competitive equilibrium allocations with the pareto optimal ones, and by their determinacy, Debreu (1970), the local uniqueness of competitive equilibrium allocations and their continuous dependence on the structural characteristics of the economy.

Existence, optimality and determinacy are the three fundamental properties of competitive equilibrium allocations, which abstract economies satisfy under conditions which are well understood.

The abstract economies in which the fundamental properties obtain indeed encompass economies which extend over time and under uncertainty, Arrow (1953), Debreu (1960) and Radner (1972), but they do so under restrictive conditions:

i) the economy is essentially finite, it does not comprise a double infinity of individuals and commodities;

ii) the asset market for the reallocation of revenue across time and realizations of uncertainty is essentially complete, all contracts for the transfer of revenue across date - events are priced;

iii) information is essentially public, individuals honor their contractual obligations even when these involve a payment, a reduction in revenue, contingent on information private to them.

With a double infinity of individuals and commodities, in particular in economies of overlapping generations, Samuelson (1958), prices, which value unambiguously the endowments of individuals, even competitive equilibrium prices, need not value the aggregate endowment and, as a consequence, competitive equilibrium allocations need not be optimal. The failure of market clearing at infinity may, Geanakoplos and Polemarchakis

(1986) allow for a continuum of distinct competitive equilibrium allocations and hence for indeterminacy.

With an incomplete asset market, prices acquire a role in addition to conveying the aggregate scarcity of commodities. Together with the asset structure, they determine the attainable reallocations of revenue across date-events. When asset payoffs are denominated in multiple commodities, the attainable reallocations of revenue may vary discontinuity with spot commodity prices and competitive equilibrium allocations may fail to exist, Duffie and Shafer (1985, 1986), Hart (1975). When multiple commodities are traded in commodity spot markets, a reallocation of assets typically leads to variation in spot commodity prices at equilibrium and, hence, in the attainable reallocations of revenue, which in turn may lead to a pareto improvement in welfare, Geanakoplos and Polemarchakis (1986). The market, typically, makes inefficient use of the available assets, there exist improving variations in the allocation of assets, and, thus, competitive equilibrium allocation typically fail to be even constrained pareto optimal. With asset payoff denominated in abstract units of account, nominal assets, variation in the level of spot commodity prices, which is indeterminate, affect nontrivially the attainable reallocations of revenue and are, thus, typically, associated with distinct competitive equilibrium allocations which inherit the indeterminacy, Balasko and Cass (1989), Cass (1985), Geanakoplos and Mas-Colell (1989).

When information is private, and incentive compatibility constraints are imposed on individuals, their optimization problems are not convex. Competitive equilibrium allocations thus need not exist, while pareto optimal allocations need not be competitive equilibrium allocations for any redistribution of revenue, Prescott and Townsend (1984a,b).

Consider an economy which extends over two periods under uncertainty. One of finitely many states of the world, $s \in \mathbf{S}$, is realized in the second period.

Commodities, at each state of the world are $\ell \in \mathbf{L}$, a finite set. A commodity bundle at state s is $x_s = (\cdots x_{(s,\ell)}, \cdots)'$, while, across states of the world, $x_{\mathbf{S}} = (\cdots, x_s, \cdots)$ is a commodity bundle.

Individuals are $h \in \mathbf{H}$, a finite set. An individual is characterized by his utility function, u^h, defined for consumption bundles, nonnegative commodity bundles, his initial endowment, w^h, a consumption bundle, and by his information partition $\mathcal{S}^h$, a partition of the set of states of nature.

Assets, $a \in \mathbf{A}$, a finite set, are traded in the first period and payoff in the second. A portfolio is $y = (\cdots, y_a, \cdots)'$. Assets are nominal, their payoffs are denominated in abstract units of account. The payoff of asset a in state s is $r_{(a,s)}$ while $r_a = (\cdots, r_{(a,s)}, \cdots)'$, and $R = (\cdots, r_a, \cdots)$ is the matrix of payoffs of asset structure.

For simplicity, for any choice of information sets across individuals, $\mathbf{S}^h \in \mathcal{S}^h$, $\cap_{h \in \mathbf{H}} \mathbf{S}^h = \{s\}$, for some $s \in \mathbf{S}$.

Commodities are traded in spot markets at each state of the world in the second period. Spot commodity prices at state s are $p_s = (\cdots, p_{(s,\ell)}, \cdots)$, $s \in \mathbf{S}$, while, across states of the world, $p = (\cdots, p_s, \cdots)$ are spot commodity prices.

Assets are traded in the first period. Asset prices are $q = (\cdots, q_a, \cdots)$.

At each state of the world, s, an individual is informed of the associated element of his partition, $\mathbf{S}^h(s) \in \mathcal{S}^h$. He may, nevertheless, choose to declare a different element, $\mathbf{\Sigma}^h(s) \in \mathcal{S}^h$; the declaration function of the individual is $\mathbf{\Sigma}^h : \mathcal{S}^h \to \mathcal{S}^h$. The declared state of the world is then $\{\sigma(s)\} = \cap_{h \in \mathbf{H}} \mathbf{\Sigma}^h(s)$. Asset payoffs are determined by the declared state of the world and are thus $r_{\sigma(s)}$, while spot commodity prices are $p_{\sigma(s)}$.

At spot prices of commodities and prices of assets (p, q), if other individuals have chosen declaration functions $\mathbf{\Sigma}^{h'}, h' \in \mathbf{H} \setminus \{h\}$, individuals h chooses a consumption bundle and a portfolio, (x^h, y^h), and a declaration function, $\mathbf{\Sigma}^h$, so as to

$$\begin{aligned} &\max\ u^h(x) \\ &\text{s.t.}\ \ qy \leq 0 \\ &\qquad p_{\sigma(s)} x_s \leq p_{\sigma(s)} \omega_s^h + r_{\sigma(s)} y \\ &\qquad (x - \omega^h) \text{ is } \mathcal{S}^h - \text{measurable.} \end{aligned}$$

The measurability constraint, as formulated in the optimization problem, presumes that individuals limit their choices to those compatible with the information privately available to them, Radner (1968). Alternatively, as in the rational expectations paradigm, Radner (1979), individuals augment their information with the information revealed by prices, in conjunction with the declaration functions of other individuals. In this case, the measurability constraint in the optimization problem takes the form

$$(x - \omega^h) \text{ is } \mathcal{S}^h \wedge \mathcal{S}^{(p \circ \sigma)} - \text{measurable,}$$

where $S^h \wedge p^{(p\circ\sigma)}$ is the join of the private information of the individual, S^h, and the information revealed by the price function composed with the revelation functions, $S^{(p\circ\sigma)}$.

A competitive equilibrium consists of spot prices of commodities and prices of assets, (p^*, q^*), an allocation of commodities and assets, $(x^{\mathbf{H}*}, y^{\mathbf{H}*}) = (\cdots, x^{h*}, \cdots, y^{h*}, \cdots)$ and a profile of declaration functions, $\mathbf{\Sigma}^{\mathbf{H}} = (\cdots, \mathbf{\Sigma}^h, \cdots)$, such that, for all individuals, $h \in \mathrm{H}$, (x^{h*}, y^{h*}) and $\mathbf{\Sigma}^{h*}$ solve the optimization problem at (p^*, y^*) and $\mathbf{\Sigma}^{h'*}, h' \in \mathbf{H} \backslash \{h\}$, while $\sum_{h \in \mathbf{H}} x^h = \sum_{h \in \mathbf{H}} \omega^h$ and $\sum_{h \in \mathbf{H}} y^h = 0$.

It is an open question to characterize asset structures, R, for which competitive equilibria exist, which can be interpreted as endogenous determination of the asset structure.

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Strategic behavior

General equilibrium concepts under imperfect competition: a Cournotian approach*

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We introduce a pure exchange economy and restate for such an economy two basic concepts[1] of general equilibrium under imperfect competition, the Cournot-Walras Equilibrium and the Cournotian Monopolistic Competition Equilibrium. This will be done to express the main difficulties.

Let us consider a set of m *consumers* $I = \{1, 2, \cdots, i, \cdots, m\}$, exchanging a set of $\ell + 1$ *goods* $H = \{0, 1, \cdots, h, \cdots, \ell\}$, in which good 0 will be interpreted as money. Hence a *price system* is a vector in $I\!R^H_+$ of the form: $p = (1, p_1, \cdots, p_\ell)$. With each consumer $i \in I$ is associated a *consumption set* $X_i \subset I\!R^{\ell+1}$, a vector of *initial resources* $\omega_i \in X_i$ and a real-valued *utility function* $U_i(x_i)$ defined on X_i. Assumptions on these consumers' characteristics will be given later (often implicitly when these assumptions are standard). They will however always include nonsatiation.

To model imperfect competition we introduce two groups of agents, a set I^* of *strategic consumers* and a set $\hat{I} = I \setminus I^*$ of *competitive consumers*, and we consider two decision stages. At the first stage each strategic consumer $i \in I^*$ chooses a price or a quantity (or both) for each good h in some given set $H_i \subset H$ of markets in which he has some monopoly power. The set of strategic goods is $H^* = \bigcup_{i \in I^*} H_i$. Then, at the second stage, all consumers taking as given the strategic choices made at the first stage, behave competitively. We shall denote by I_h the set $\{i \in I : h \in H_i\}$ of consumers having monopoly power on good h, and by $\hat{H} = H \setminus H^*$ the set of competitive goods. We assume in the rest of the paper $0 \in \hat{H}$ and $I \setminus I_h \neq \emptyset$ for any $h \in H$.

1. The **Cournot-Walras Equilibrium** can now be defined. The original definition due to Gabszewicz and Vial (1972) was given in an oligopolistic framework with producers as strategic agents, choosing production levels. However it can be stated for a pure exchange

* Presentation based on a paper by C. d'Aspremont, R. Dos Santos Ferreira and L.-A. Gérard-Varet.

[1] We shall not consider here the concept of Monopolistic Competition Equilibrium. For this see Marschak and Selten (1974), Nikaido (1975), Benassy (1988) and d'Aspremont, Dos Santos Ferreira and Gérard-Varet (1990). For overviews see Hart (1985) and Gary-Bobo (1989).

context as shown in Codognato and Gabszewicz (1991) and Gabszewicz and Michel (1992). The two-stage procedure can be specified as follows. For every $i \in I^*$, and for all goods in H_i, i chooses the quantity q_i in some admissible subset Q_i of $\mathbb{R}^{H_i}$. Codognato and Gabszewicz suppose that $X_i = \mathbb{R}_+^{\ell+1}$ and $Q_i = \{q_i \in \mathbb{R}^{H_i} : \ 0 \leq q_{ih} \leq \omega_{ih}, \quad \forall\, h \in H_i\}$. However, as we will see, this is not indispensable. What is indispensable is to suppose that, for every $q \in Q = \underset{i \in I^*}{X} Q_i$ and every $i \in I$, there is a well-defined *net demand function* $\zeta_i(p, q_i)$, and that the following assumption holds:

Assumption 1: For all $q \in Q$, there is a unique price system $\bar{p}(q)$ such that

$$\sum_{i \in I} \zeta_i(\bar{p}(q), q_i) = 0 \tag{1}$$

Typically the functions ζ_i may be defined such that:

$$\zeta_i(p, q_i) + \omega_i = \arg\max_{x_i} U_i(x_i)$$

where $x_i \in X_i$ satisfies the constraints

$$p \cdot (x_i - \omega_i) \leq 0$$

and, for all $h \in H_i$,

$$x_{ih} = \omega_{ih} - q_{ih}.$$

This last set of constraints could be replaced (as in Gabszewicz and Michel (1992)) by inequalities if one wanted to give to consumer i the possibility of revising his consumption decision: for all $h \in H_i$,

$$x_{ih} \leq \omega_{ih} - q_{ih}.$$

This is a way to give more flexibility to a strategic consumer (his first stage choice is not binding) and to reinforce the signalling role of the quantity orders. However we shall use here only the definition of ζ_i based on equalities for all $h \in H_i$. Finally, also notice that, whenever $i \in \hat{I}$ (is a competitive agent), $\zeta_i(p, q_i)$ can be written more simply as $\zeta_i(p)$, since it is constant in q_i.

A *Cournot–Walras Equilibrium* is a pair of prices and quantities (p^{CW}, q^{CW}) in $\mathbb{R}_+^H \times Q$ such that:

$$p^{CW} = \tilde{p}(q^{CW}) \text{ and}, \quad \forall\, i \in I^*,$$
$$q_i^{CW} \in \arg\max_{q_i \in Q_i} U_i(\zeta_i(\tilde{p}(q_i, q_{-i}^{CW}), q_i) + \omega_i)$$
$$\text{with } q_{-i}^{CW} = (q_j^{CW})_{\substack{j \in I^* \\ j \neq i}}$$

In other terms, once all the functions ζ_i are well-defined and assumption 1 holds, the Cournot–Walras quantity q^{CW} is the Nash-Equilibrium of the game with players in I^*, strategies in Q and payoffs given by $\{U_i(\zeta_i(\tilde{p}(q), q_i) + \omega_i)\}$.

In the special case $H_i = \emptyset$ for all consumers i, then the first stage becomes trivial and each net demand function $\zeta_i(p, q)$ coincides with the competitive net demand. The Cournot–Walras Equilibrium reduces then to the *Walrasian Equilibrium*, charaterized by a price-system p^W such that:

$$\sum_{i \in I} \zeta_i(p^W) \;=\; 0.$$

2. To avoid some complexities and simplify the anticipations of agents, we want to define a second general equilibrium concept under imperfect competition, the **Cournotian Monopolistic Competition Equilibrium**. In d'Aspremont, Dos Santos Ferreira and Gérard–Varet (1991), this concept was defined for a partial equilibrium model allowing for several productive sectors. Here we shall keep the same pure exchange model as above and assume that, for every vector of prices $p \in I\!R_+^H$ and every vector of quantities $q \in Q$, the net demand functions $\{\zeta_i(p, q_i)\}$ as given previously are well-defined.

A *Cournotian Monopolistic Competition Equilibrium* is then a vector of prices and quantities (p^{CC}, q^{CC}), with $p^{CC} \in I\!R_+^H$ and $q^{CC} \in Q$, and such that for all $i \in I^*$:

$$(a.1) \qquad (p_i^{CC}, q_i^{CC}) \in \arg\max_{(p_i, q_i)} U_i\left(\zeta_i(p_i, p_{-i}^{CC}, \hat{p}^{CC}, q_i) + \omega_i\right)$$

with $p_i^{CC} = (p_h^{CC})_{h \in H_i}$, $p_{-i}^{CC} = (p_h^{CC})_{h \in H^* \setminus H_i}$ and $\hat{p}^{CC} = (p_h^{CC})_{h \in \hat{H}}$, and with $p_i \in I\!R_+^{H_i}$, $q_i \in Q_i$ satisfying, $\quad \forall\, h \in H_i$,

$$(a.2) \qquad \min\left\{0, -\sum_{j \neq i} \zeta_{jh}(p_i, p_{-i}^{CC}, \hat{p}^{CC}, q_j^{CC})\right\} \leq \zeta_{ih}(p_i, p_{-i}^{CC}, \hat{p}^{CC}, q_i)$$

$$\leq \max \left\{ 0, -\sum_{j \neq i} \zeta_{jh}(p_i, p_{-i}^{CC}, \hat{p}^{CC}, q_j^{CC}) \right\}$$

and

(b) $$\sum_{i \in I} \zeta_i(p^{CC}, q_i^{CC}) = 0.$$

Condition (a.1) requires that each strategic consumer maximizes his utility by choosing both the price p_i and the quantity orders q_i for the goods he controls (all h in H_i) taking as given the prices of other goods and the quantity orders of other consumers. This is a direct generalization of Cournot's concept: each agent behaves as a monopolist optimizing against a "residual" demand. But the market price for each good is "necessarily the same" (to use the words of Cournot) and so, at an equilibrium, any two consumers who are "controlling" the price of the same good, should (optimally) choose the same value (for all i and j in I^* and $h \in H_i \cap H_j$, the price p_h^{CC} should be an optimal price for both i and j). Here there is no explicit pricing coordination mechanism among strategic agents in the same market. This is somewhat analogous to the Lindahl solution for public goods, where each consumer has to choose independently the same quantity of public goods. In the next section, formal pricing coordination mechanisms will be introduced explicitly[2].

On the other hand, in the general equilibrium approach adopted here it is supposed that each strategic consumer solves an optimization program taking as fixed the prices of the goods he does not control, whether these prices are competitive prices (parametrically determined) or strategic prices (strategically determined by other consumers). This is an important simplification with respect to the Cournot–Walras or the Monopolistic Competition concepts where the vector of all prices was supposed to always clear all markets. This simplification is already used by Laffont and Laroque (1976, assumption 10, page 288) in their definition of a general equilibrium concept under imperfect competition.

Condition (a.2) restricts deviations by a strategic consumer to the ones that are on the nonrationed side of the market: the deviating consumer net demand should have an opposite sign to, and be bounded by, the resulting total net demand of the others. Hence deviation on a market should satisfy some market "feasibility" condition but not market-clearing. However market-clearing is imposed at equilibrium (condition (b)).

[2]See also d'Aspremont, Dos Santos Ferreira and Gérard–Varet (1991).

3. We shall now introduce the P-**equilibrium concept** a multi-purpose concept of general equilibrium under imperfect competition. This concept is a generalization of the Cournotian Monopolistic Competition Equilibrium just introduced. The main difference is that we shall define explicitly a formal mechanism of price coordination on oligopolistic markets: This is a rule associating to any vector of individual "price signals" a common "market price." This will lead to alternative interesting definitions.

The basic idea is to have a two-stage procedure as before, and to give each strategic consumer the possibility to send, at the first stage, both price signals and quantity orders. In some sense there is a "planning" stage and an "implementation" stage. We should now distinguish the set H_i^Q of markets in which consumer i sends *quantity orders* q_{ih}, with $q_i = (q_{ih})_{h\in H_i^Q}$ taken in some admissible set Q_i, and the set H_i^Ψ of markets in which i sends *price signals* ψ_{ih} in Ψ_{ih}, some admissible subset of $[0,\infty)$. Correspondingly we define:

$$I_h^Q = \{i \in I : h \in H_i^Q\},\ I_h^\psi = \{i \in I : h \in H_i^\Psi\},$$
$$H_i = H_i^Q \cup H_i^\Psi,\ H^Q = \bigcup_{i\in I^*} H_i^Q \text{ and } H^\Psi = \bigcup_{i\in I^*} H_i^\Psi.$$

A quantity order q_{ih} indicates the quantity of good h consumer i is ready to offer ($q_{ih} > 0$), or to bid for ($q_{ih} < 0$) in market h. In addition for the price signals, we suppose that there exists, for every $h \in H^\Psi$, some kind of mechanism or *pricing-scheme* P_h integrating the vector of signals $\psi_h \in \Psi_h = \mathop{X}_{i\in I_h^\Psi} \Psi_{ih}$ into a single *market price* $p_h = P_h(\psi_h) \in I\!R_+$. Considering our interpretation we will usually assume for any $h \in H^\Psi$ that $P_h(\overline{\psi},\overline{\psi},\cdots,\overline{\psi}) = \overline{\psi}$, for any $\overline{\psi} \in I\!R_+$, and that $P_h(\psi_h) \geq P_h(\psi'_h)$, whenever $\psi_{ih} \geq \psi'_{ih}$ for all $i \in I_h^\Psi$.

A pricing-scheme P_h being fixed for every market h in H^Ψ, we may again define, for every consumer i, a *net demand function* $\zeta_i(p,q_i)$ conditional on $q_i \in Q_i$, the vector of quantity orders, and conditional on the price vector $p = (1,p_1,\cdots,p_h,\cdots,p_\ell)$, where for every good $h \in H^\Psi$, $p_h = P_h(\psi_h)$ depends on all price-signals in market h. As before ζ_i may be taken to satisfy

$$\zeta_i(p,q_i) + \omega_i = \arg\max_{x_i} U_i(x_i)$$

$$\text{with } x_i \in \{x_i \in X_i | p\cdot(x_i-\omega_i) \leq 0 \text{ and, } \ \forall\, h \in H_i^Q,\ x_{ih} = \omega_{ih} - q_{ih}\}.$$

Again $\zeta_i(p,q_i)$ can be written more simply as $\zeta_i(p)$ wheneven $i \in \hat{I}$.

A *P-equilibrium* is a vector of prices and quantity orders (p^*, q^*), with $p^* \in \mathbb{R}_+^H, q^* \in Q$ and, for every $h \in H^\Psi$, $p_h^* = P_h(\psi_h^*)$ for some $\psi_h^* \in \Psi_h$, such that, for all $i \in I^*$:

$$(a.1) \qquad U_i(\zeta_i(p^*, q_i^*) + \omega_i) \geq U_i(\zeta_i(p_i, p_{-i}^*, \hat{p}^*, q_i) + \omega_i)$$

for all $q_i \in Q_i$ and for all $p_i \in \mathbb{R}_+^{H_i^\Psi}$ satisfying

$$p_{ih} = P_h(\psi_{ih}, \psi_{-ih}^*) \text{ for some } \psi_{ih} \in \Psi_{ih} \text{ and all } h \in H_i^\Psi,$$

with $p_i^* = (p_h)_{h \in H^\Psi}, p_{-i}^* = (p_h)_{h \in H^\Psi \setminus H_i^\Psi}$ and $\hat{p}^* = (p_h^*)_{H \setminus H^\Psi}$
and also satisfying, for all $h \in H_i$

$$(a.2) \qquad \min\{0, -\sum_{j \neq i} \zeta_{jh}(p_i, p_{-i}^*, \hat{p}^*, q_j^*)\} \leq \zeta_{ih}(p_i, p_{-i}^*, \hat{p}^*, q_i)$$

$$\leq \max\{0, -\sum_{j \neq i} \zeta_{jh}(p_i, p_{-i}^*, \hat{p}^*, q_j^*)\}$$

and

$$(b) \qquad \sum_{i \in I} \zeta_i(p^*, q_i^*) = 0.$$

Expressions (a.1), (a.2) and (b) are very close to the corresponding expressions for the definition of a Cournotian Monopolistic Competition Equilibrium and can be interpreted similarly. The difference is that in defining this concept, we supposed $H_i^Q = H_i^\Psi = H_i$ for all $i \in I^*$ and we did restrict implicitly to a specific kind of pricing-schemes. This is the class of *fully individually manipulable pricing-schemes* in the sense that, for any $i \in I_h^\Psi$ and any given $\psi_{-ih} \in \mathbb{R}_+^{I_h^\Psi \setminus \{i\}}$, the function $P_h(\cdot, \psi_{-ih})$ has full range. Indeed we get,

Proposition 1: Let, for any $i \in I^*$, $H_i^Q = H_i^\Psi = H_i$. If the pricing-scheme is fully individually manipulable, then P-equilibrium and Cournotian Monopolistic Competition Equilibrium coincide.

Proof: Since $P_h(\cdot, \psi_{-ih})$ has full range, it is equivalent for a strategic consumer i to choose a signal ψ_{ih} leading to some value $p_h = P_h(\psi_{ih}, \psi_{-ih})$ or to choose directly the price p_h itself. The result follows. ∎

The main benefit of considering the class of fully individually manipulable pricing-schemes is that the equilibrium concept can be defined without explicit reference to a particular pricing-scheme. The agents succeed in coordinating their pricing strategies through some process which has not to be completely specified.

4. Proposition 1 relies on fully individually manipulable pricing schemes. However it is not clear that the kind of coordination mechanisms that is used in practice has such degree a manipulability by each individual agent. Empirical as well as theoretical studies of pricing strategies in some industries have concentrated on a number of "facilitating practices", or conventional norms of conduct among competitors leading to a market price well above its pure competitive level[3]. For example the practice of advance public notification of list-price increases, with the possibility of cancelling the increase if not followed, has been under attack in the Ethyl and DuPont de Nemours case. Moreover in many selling contracts there are particular clauses allowing in fact competitors to coordinate their pricing strategies more efficiently than by tacit collusion: for instance the "meet-or-release" clause, whereby a seller should meet a lower offer to a customer or release him from the contract, or the "most-favoured-customer" clause, whereby a seller engage not to sell to customer at a lower price. As argued in the literature the introduction of such practices amounts to use a pricing-scheme which consists in having the market price as the minimum of all announced prices. Indeed the best-price provisions imply that any seller should be informed (directly or through some trade association) of any price reduction by a competitor and follow it, and the advance notification of a price increase (publicly or via a trade association) allows the sellers to coordinate this increase. This could result in a pure collusive price except that, as remarked by Holt and Scheffman (1987), the combination of the possibility of discounting with the use of the meet-or-release clause, ensures that any discount made by a seller can be matched by the other sellers, thereby maintaining their sales quantities. This argument tends to show that the highest attainable price in a market should be the Cournot price. Of course since the result of using the min-pricing-scheme on a market is to create a kinked demand curve many other prices are also attainable at a (partial) equilibrium[4]. In general, one should expect that the set of $P^{\min}$-equilibria be larger than the set of Cournotian Monopolistic Competition Equilibria.

[3]See Salop (1985), Kalai and Satterthwaite (1986), Cooper (1986), Holt and Scheffman (1987) and Logan and Lutter (1989).
[4]See d'Aspremont, Dos Santos Ferreira and Gérard–Varet (1991).

Formally, we define another kind of pricing-schemes, namely the min-pricing schemes and, its dual, the max-pricing schemes:

$$P_h^{\min}(\psi_h) = \min_{i \in I_h^{\Psi}} \{\psi_{ih}\}$$
$$P_h^{\max}(\psi_h) = \max_{i \in I_h^{\Psi}} \{\psi_{ih}\}.$$

We shall apply each of these pricing-schemes to two different sets of agents, "natural sellers" and "natural buyers". An agent $i \in I$ is called a *natural seller* (resp. a *natural buyer*) with respect to good $h \in H \setminus \{0\}$ if, for every $p \in \mathbb{R}_+^H$, $q_a \in Q_a$,

$$\zeta_{ih}(p, q_i) \leq 0 \quad (\text{resp. } \zeta_{ih}(p, q_i) \geq 0).$$

We denote by S_h (resp. B_h) the set of natural sellers (resp. natural buyers) with respect to good h. A natural seller (or buyer) for good h is an agent who is selling (buying) good h whatever the values of the prices and other variables in its net demand function. It implies restrictions on tastes and endowments for the consumers.

We shall limit the set of price-makers to be either natural sellers or buyers, as a plausible necessary condition for monopoly power, excluding however bilateral monopolies. This allows us to apply the min-pricing scheme to a market with natural sellers and, symmetrically, to apply the max-pricing scheme to a market with natural buyers. The interesting fact is that, for an economy where pricing-schemes are limited in this way, all Walrasian Equilibria are included in the set of P-equilibria, under the condition that there are at least two strategic agents on each market where there is some monopoly control. Formally we have

Assumption 2: For all $h \in H^{\Psi}$, either $I_h^{\Psi} \subset B_h$ or $I_h^{\Psi} \subset S_h$ (but not both) and $|I_h^{\Psi}| \geq 2$.

Proposition 2: Under assumption 2, any Walrasian Equilibrium with price system p^W is obtained as a P-equilibrium, for $P_h = P_h^{\min}$ when $I_h^{\Psi} \subset S_h$ and $P_h = P_h^{\max}$ when $I_h^{\Psi} \subset B_h$, for any $h \in H^{\Psi}$.

To conclude it should be emphasized that in an economy as described by assumption 2, one could expect, in general, multiple P-equilibria with the $P^{\min}$-$P^{\max}$-pricing-schemes. However by proposition 2, whenever there exists a Walrasian Equilibrium, it is one of them. In that sense, proposition 2 gives an existence result.

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Oligopoly and profit maximization in general equilibrium

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I would like to raise a question about which I have no answer. This question arises in the area of imperfect competition and General Equilibrium. A natural way of modeling imperfect competition in a general equilibrium framework consists in assuming that firms maximize profits, taking into account the impact of their own individual supply of goods on the prices which clear the markets where they operate. Then the resulting profits are distributed to the owners of the firms. This is what I have done with J.-P. Vial (1972). It turns out however that this approach has revealed two major difficulties. First, there may be situations in which the maximization of profits cannot be regarded as a rational decision criterion for the firms which are able to manipulate equilibrium prices. By choosing an output vector so as to maximize the wealth of the firms owners, the firm may neglect alternative strategies which would yield higher utility levels for its shareholders, a point confirmed in a recent paper I wrote with P. Michel (1992). Second, the noncooperative equilibrium is not invariant with respect to the normalization rule used for defining the absolute prices. An example is provided in my paper with Vial, in which the equilibrium is different when two different normalization rules are used. How to circumvent these difficulties ? The first one can be logically circumvented if the firms are owned by single individuals, simply because one can replace the profit criterion by the owner's utility criterion. Furthermore, this assumption of firms owned by single individuals, also solves the second difficulty, because the resulting definition of noncooperative equilibrium, using utilities as payoffs, no longer depends on the normalization rule used to define absolute prices. But this does not *really* solve the problem, not only because firms are generally owned by several individuals, but mainly because of the reason for which this difficulty arises. The reason is the following: the goods produced by an oligopolistic firm enter as arguments in the utility functions of its owners. Since the choice of the level of production serves simultaneously as an instrument in view of influencing prices via quantities *and* in view of meeting the desired level of consumption of these goods by the owners, it is impossible to separate, as under perfect competition, consumption and production decisions via the profit criterion. However, firms do not generally take into account, in their production decisions, the level of consumption of the goods they produce desired by their owners ! A firm producing toothpaste, even if it is owned by thousands of individuals who brush their teeth every morning and evening, does not choose the quantity of toothpaste to produce by taking into account the desired consumption of toothpaste by its owners. The natural model of a firm could however require this, to the extent that private consumption of toothpaste by the owners of the firms, influence the relative price of toothpaste ! This is clearly absurd and some device should be found to circumvent this difficulty. But this is not easy, because it opens the door to arbitrariness. This problem of course disappears for a profit maximizing firm when it takes the price system as given: then profit maximization leads to owners' wealth maximization, which in turn implies utility maximization.

Now let me state precisely my question: What should be the criterion used by firms in an oligopolistic context in General Equilibrium ? Profits ? But then the normalization problem arises. The utilities of the owners ? Then an aggregation problem arises when the owners are not identical in terms of their preferences.

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Imperfect competition and general equilibrium

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In the Arrow-Debreu-McKenzie model we have a beautiful and consistent model, which allows us to analyze many different economic questions, when there is pure competition in the economy. However, our present understanding of how strategic behaviour can be modeled using the same basic methodology is rather poor. Of course, we have during the last years obtained a lot of insight on strategical behaviour in partial equilibrium models by using the methodology of the Arrow, Debreu and McKenzie model and game theory. The problem arises, when we want to analyze the entire economy.

The main assumption when defining a Walras equilibrium is that the agents take the strategic choice and the price system as given. This implies that the Walras equilibrium can be formulated as a Nash equilibrium of a properly defined generalized game. The same method has been useful when studying other institutional restrictions e.g. quantity restrictions in the fix price literature.

When one wants to model situations where agents behave strategically, then some major difficulties arise. E.g. one wants to allow that the agents take into account that their choice of strategy influences the institutional constraints, e.g. the price system, or the acts of other participants in the economy. Often these difficulties come since the set of outcomes corresponding to the strategy profiles of the agents who have the strategic power does not vary continuously. Indeed, they are often determined as fixed points of derived functions or correspondences and are therefore only upper hemi-continuous. Since upper hemi-continuous correspondences do not allow a continuous selection, it is of little help to try to solve the problem by using selections from the outcome correspondence, - at least if one does not allow random selections.

Cournot-Walras equilibria

Let me now turn to a concrete model, where firms behave strategically à la Cournot, namely the model introduced by Gabszewicz and Vial (1972). The reason is that it highlights some of the problems we have, when we allow the firms to behave strategically.

In a Walras equilibrium it is assumed that firms take the price system as given and maximize profit with respect to the price system. Since the shareholders of the firm take the same price system as given, when they choose consumption plan, no shareholder will object to this criteria function of the firm. Also in the Walras equilibrium only the relative price is determined, but the normalization of the price system does not have any real effect.

A Cournot-Walras equilibrium is defined by the following thought process: For each choice of production plans of the firm a pure exchange is determined. By looking at the corresponding set of Walras equilibrium price systems, each producer can calculate the profit corresponding to the choice of production plans. However, as there are typically several relative equilibrium price systems, one has to consider a price selection. And since only relative price systems are determined, one has to consider a normalization rule in order to obtain that the profit functions are well defined. An equilibrium is, after these choices, a Nash equilibrium of the game where the strategy sets of producers are the production sets, and the pay off functions are the profit functions. The three problems, which arise when firms behave à la Cournot, and which are highlighted by this model, are that

1) the normalization rule has real effects,
2) there does not have to exist an equilibrium, even in mixed strategies, and
3) the choice of objective functions for the firms.

The normalization rule has real effects

Is was already noticed by Gabszewicz and Vial that the choice of normalization rule has real effects. Indeed, it can be shown that almost all production plans can be obtained as equilibrium production plans by changing the normalization of the price system (see Grodal (1984), H. Dierker and Grodal (1986), Böhm (1990), and Grodal (1992)). It can also be shown that this dependence of the normalization rule even has an impact on the limit (see Grodal (1992)).

There does not have to exist an equilibrium, even in mixed strategies

In Roberts and Sonnenschein (1977) is given an example where there does not exist an equilibrium in pure strategies. The reason is that the profit function for a given price selection and normalization does not have to be quasi concave. In H. Dierker and Grodal (1986) is given an example where there even does not exist an equilibrium in mixed strategies. The reason is that there does not exist a continuous selection from the Walras price correspondence, and that one is free to choose the normalization rule.

The choice of objective functions for the firms

The shareholders, of course, unanimously want to have maximal income as long as this does not influence the price system which they are facing. However, in the Cournot-Walras model the fact that firms maximize profit also has impact on the price system which the shareholders face as consumers. Therefore in a fixed finite economy, it does not have to be in the interests of the shareholders that firms maximize profit (with respect to some normalization rule). The shareholders, of course, want to have the maximal utility from the trade on the market - assuming that the shareholders cannot get any commodities directly from the firm. If the shareholders have the same utility function one might let the firm maximize the indirect utility function of the owners instead of maximizing profit. This overcomes the dependence of the normalization rule but not the existence problem (see H. Dierker and Grodal (1986)).

The constraints of the shareholders on the Walrasian market

The Gabszewicz and Vial model assumes that shareholders cannot get the excess commodities directly from the firm, i.e. the firm cannot choose a production plan, give some of the commodities directly to the shareholder and deliver the rest to the market. If this possibility is allowed, one has to be very careful if one has chosen the restrictions for the shareholders on the market. This is shown by the following example, where we consider a pure exchange economy i.e. one shareholder for each firm and "production sets" of the form $Y_i = \{q_i | 0 \leq q_i \leq w_i\}$.

Example

Let $\varepsilon = (\mathbb{R}^\ell_+, \succ_i, w_i)_{i=1}^m$ be a pure exchange economy. In this economy we can as usual define the set of Walras allocations, $W(\varepsilon)$.

We can also consider the following game, where we intend to give the consumers some strategic power:

The consumers have strategy sets $\Omega_i = \{q \in \mathbb{R}^\ell | 0 \leq q \leq w_i\}, i = 1, \cdots, m$. The possible outcomes corresponding to the strategy profile $(q_1, \cdots, q_m)$ are determined by the Walrasian mechanism. The consumer i chooses a vector $u_i \in \mathbb{R}^\ell$, constrained by his consumption set and a Walrasian price system. I.e $(u_i)_{i=1}^m$ is an outcome corresponding to $(q_1, \cdots, q_m)$ if there exists a price system $p \in \mathbb{R}^\ell \setminus \{0\}$ such that

(1) for all $i = 1, \cdots, m : pu_i \leq pq_i, u_i + (w_i - q_i) \in \mathbb{R}^\ell_+$ and there does not exist $u_i' \in \mathbb{R}^\ell$

such that $pu' \leq pq_i, u_i' + (w_i - q_i) \in I\!R^\ell_+$ and $u_i' + (w_i - q_i) \succ_i u_i + (w_i + q_i)$

(2) $\sum_{i=1}^m u_i = \sum_{i=1}^m q_i$

Let $A(q_1, \cdots, q_m)$ be the outcomes corresponding to the strategy profile $(q_1, \cdots, q_m)$.

Now we define $(x_i^*)_{i=1}^m$ to be an equilibrium outcome for ε if $x_i^* = u_i^* + (w_i - q_i^*)$ where $(u_i^*)_{i=1}^m \in A(q_1^*, \cdots, q_n^*)$, for all $i = 1, \cdots, m$ there does not exist $q_i' \in \Omega_i$ and $(u_i', \cdots, u_m') \in A(q_i, \cdots, q_i', \cdots, q_m)$ such that $u_i' + (w_i - q_i') \succ_i x_i^*$.

It is easily seen that $(u_1, \cdots, u_m) \in A(q_1, \cdots, q_m)$ if and only if $(u_i + (w_i - q_i))_{i=1}^m \in W(\varepsilon)$. I.e for all $(q_1, \cdots, q_m)$ the final consumption plans corresponding to the outcomes $A(q_1, \cdots, q_m)$ are exactly the Walras allocations.

(Consequently $(x_i^*)_{i=1}^m$ is a Walras equilibrium and for all $i \in \{1, \cdots, m\}$ there does not exist another Walras equilibrium where i gets a preferred consumption plan. Therefore, when there exists an unique Walras equilibrium for ε, this is an equilibrium. However, when the preferences of the consumers are complete, monotone and continuous there will only exist an equilibrium if $W(\varepsilon) \neq \emptyset$ and all Walras allocations are equivalent for all the consumers.)

The example clearly shows that it is very important how one restricts the actions of agents in "the second stage of the game." In Gabszewicz and Michel (1992) they use a game similar to the one described in the example, however, they restrict the trades in the second stage to $u_i \in I\!R^\ell_+$. This raises the question how the consumers can commit themselves to this restriction. If this is done by burning the surplus initial resources $(w_i - q_i)$, then we are back to a situation where the consumers (the firms) just choose the strategy to maximize the indirect utility function.

Conclusion

In my opinion the present understanding of how to integrate imperfect competition in a general equilibrium approach is not satisfactory. It is a hope that the present research on aggregation allows one to put more structure on the demand side of the economy and thereby avoid some of the problems coming from multiple equilibria in "the second stage game."

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Notes on equilibrium existence in economies with oligopolistic firms

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Dierker and Grodal (1984) showed that equilibrium - even in mixed strategies - may fail to exist in economies with oligopolistic firms when firms set quantities à la Cournot. The failure they identify arises because the Walrasian price correspondence - thanks to a multiplicity of market-clearing prices - may behave badly even in regular economies.

This suggests the possibility that if firms set prices as well as quantities,[5] equilibrium existence might be restored. The idea here is that, although Walrasian prices as functions of quantities may be badly behaved, quantities demanded as functions of prices are continuous (as long as the underlying preferences are continuous and convex).

Once firms choose both prices and quantities, however, timing becomes an issue - we might model firms as choosing (1) quantities before prices; (2) quantities and prices simultaneously; or (3) quantities after prices. Each of the three makes sense in some circumstances. For example, 1 corresponds to the case where a firm must produce a good before selling it, whereas 3 can be thought of as "production to order."

Unfortunately, the three have radically different implications for the nature of equilibrium. For example, consider a market for a good in which there are two identical firms. Each firm incurs a fixed cost 3/4 if it produces a positive quantity of output, but its marginal cost of production zero. The inverse demand function is $p = 2 - q$. Notice that the fixed cost is so big that only one firm can operate profitably.

Given timing 1, there is an equilibrium in which only one firm operates and, in fact, behaves as a textbook monopolist. It first sets $q = 1$ (the monopoly quantity), and the other firm sets $q = 0$. Then, the monopoly firm sets $p = 1$ (the monopoly price); the other firm's price is irrelevant since it has nothing to selt. Under timing 3, there is again an equilibrium in which just one firm produces, only now the firm earns zero rather than monopoly profit. First both firms set $p = p^* < 1$ such that $(2 - p^*)p^* = 3/4$. Then, in the next period, one firm sets $q = q^*(= 2 - p^*)$, whereas the other takes $q = 0$. Finally,

[5] A pure Bertrand framework in which firms simply set prices may not make sense unless a firm is willing to supply all that is demanded at a given price, which would not be the case under strictly decreasing returns to scale.

it is not hard to see that timing 2 has no equilibrium at all (even in mixed strategies).

The sensitivity of results to the timing in this example illustrate why oligopoly has proved to be such a difficult and messy subject. When market conditions sufficient for perfect competition are met, by contrast, the timing is not likely to make a big difference to the outcome.

The general existence of equilibrium under timing 1 is not a matter that, to my knowledge, has been intensely studied (there have, however, been a few isolated examples such as Kreps-Scheinkman (1983)). As for timings 2 and 3, if technologies are convex (ruling out the above example) and the demand facing *individual* firms is a continuous function of prices[6] (ruling out perfect substitutability between different firms' outputs), then Glicksberg's (1952) theorem ought to ensure the existence of an equilibrium in mixed strategies. If mixing actually occurs, however, then equilibrium will typically entail rationing or over-supply, since not all realizations of prices will be equilibrating. if we allow for perfect substitutability (as in the above example and the original Bertrand and Cournot models), firms no longer faces continuous demand. But application of the Simon-Zame or Dasgupta-Maskin (1986) discontinuous games apparatus ought, once again, to assure existence of mixed-strategy equilibrium.

[6] I have already assumed that aggregate demand is continuous.

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General equilibrium with imperfect competition: the Cournot case

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Starting with a standard model of general equilibrium, it is not difficult to introduce agents who are not price-takers. However, non-price-takers do not sit easily with the idea of decentralization which is at the heart of general equilibrium. One model which is fully "decentralized" is a special case of the Negishi (1961) subjective demand model-firm j believes that the price vector will be $P_j(p^*, y_j, y_j^*)$ if it chooses the net output vector y_j, given that its prevailing net output is y_j^* and the prevailing price vector is p^*. If the firm maximises perceived profits then its supply function will be $y_j(p) \in Y_j$ (its production set) where

$$P_j(p, y_j(p), y_j(p)) \cdot y_j(p) \;\geq\; P_j(p, y_j, y_j(p)) \cdot y_j \text{ for all } y_j \in Y_j.$$

Under standard assumptions and a quasi-concavity restriction on the perceived revenue function, the supply function will be continuous and existence of general equilibrium follows along standard lines. In this model, all that has changed is the firm's objective function: each firm conditions behaviour on price and behaviour is decentralized (in the original Negishi model, P_j can depend upon demands and supplies elsewhere in the economy).

The problem with the above story is that the perceived price functions are not determined within the model. Almost any outcome can be made an equilibrium for some set of beliefs. To be able to say more about the nature of equilibrium one would like to determine the price function within the model - to base behaviour on "objective" demand. The most well-known example of this derives the price function from Cournot behaviour: A firm has perfect information about the underlying structure of the economy and maximises profits under the assumption that other firms do not change their production plans in response to a change in production by the firm in question.

To see that the Cournot story may not fit well with decentralization, consider an exchange economy where net supplies are net trades and all agents act in a Cournot manner. At a feasible allocation (excess demand equal to zero) no agent can change his net trade and maintain market clearing: if each agent accepts market clearing as a restriction then any feasible allocation will be an equilibrium. The conventional way of circumventing this problem is to assume that some agents - the consumers, say - do not act in a Cournot manner and are, instead, price-takers. If a firm contemplates a net supply change then consumers will adapt to this change if there is an appropriate price change. Thus each firm can determine the equilibrium price vector that results if firm k chooses net supply $y_k - P(y_1, \cdots, y_K)$ - and firm j, acting in a Cournot manner will choose y_j to maximise $P(y_1, \cdots, y_j, \cdots, y_K) \cdot y_j$ subject to $y_j \in Y_j$. This approach to the problem is the one taken by Gabszewicz and Vial (1972).

The assumption of a price-taking group of agents does not overcome all of the difficulties that occur with the model where all agents act in a Cournot manner. If there are intermediate goods produced by 'large' firms then it may be inappropriate to assume that these goods are either demanded or supplied by price-taking agents. In this case there may be a multiplicity of equilibrium of the sort that occurs when there are no price-takers: if a firm produces a good that is demanded only by other non-price-taking firms then with a Cournot assumption, the firm cannot contemplate a change in supply. Another way of viewing this is that without a competitive market to 'determine' prices for such goods, there is nothing in the set-up of the problem which allows prices to be determined - the model does not provide a theory of price-making.

What if all goods are demanded or supplied by price-taking agents ? Given the net supplies of all Cournot agents, prices can be determined which clear all markets (given appropriate assumptions) but similar problems remain (Hart (1980)). To take an example, if nuts and bolts are supplied by two separate Cournot firms then with a production of nuts equal to zero, the demand price of bolts may be too low to encourage the bolt supplier to produce. If it is the same the other way round then there is one equilibrium where no nuts and bolts are produced and another (superior) equilibrium where positive production occurs. This problem is particularly acute when one considers intermediate goods and the Cournot model does not deal in a satisfactory way with such goods.

The difficulties that have been alluded to above point to the difficulties of using the Cournot assumption for the equilibrium concept rather than saying much about 'real-world' problems. Many (intermediate) goods are produced by just one firm and then sold to a small number of other firms. In this situation, the Cournot assumption does not look attractive and one would expect direct negotiation to occur. In the nuts and bolts example, one could except either direct coordination between the two producers or a merging of the two firms. But the situation where complementary goods of this sort exist are likely to lead to real-world difficulties is when there is imperfect information -

firms fail to produce goods because there is no market for them and they are unaware of the fact that there are other potential goods which, if they were produced, would ensure a market.

Turning away from these issues, there is the question of whether an equilibrium exists with some firms acting in a Cournot manner. Firm j wishes to maximize $P(y_1, \cdots, y_j, \cdots, y_K) \cdot y_j$ where $P(\cdot)$ is the price that clears markets with fixed supply by the Cournot firms. In general, very few restrictions can be imposed on P and, in consequence there is no reason to expect the optimal behaviour of firm j to be continuous in the net supplies of the other firms - existence will be ensured only under very strong assumptions concerning preferences, etc.

From what has been said so far, general equilibrium with Cournot agents does not look appealing (and there are other difficulties, e.g. non-invariance with respect to choice of numeraire, that have not been discussed). But there are positive results when individual firms become small. For instance, when the economy is replicated r times then the same price function $P(\cdot)$ is applicable if the r firms replicated from firm j each produce y_j. But if $P(\cdot)$ is differentiable then the responsiveness of $P(\cdot)$ to one firm's net supply diminishes and, in the limit, the Cournot firms act like price-takers - the equilibrium is perfectly competitive (the problem concerning intermediate goods remains). Although this is of some importance, there is no guarantee that the price selection $P(\cdot)$ can ever be chosen to be well-behaved and, for instance, it may have to be discontinuous. When this is the case, replication can involve an equilibrium which converges to a point of discontinuity (Roberts (1980)) - close to a critical point where a small perturbation destroys an equilibrium, price is sensitive to a perturbation and, despite replication, each firm can produce a relatively large price change from a change inproduction plan. This 'strange' equilibrium is robust to perturbations in the economy. One can also turn the argument around - at almost all perfectly competitive equilibria the selection $P(\cdot)$ will be well-behaved and one can find a Cournot equilibrium close to the perfectly competitive equilibrium after a suitable number of replications. Whilst it is reassuring that perfect competition may be viewed as a limit when monopolistic power diminishes, it is worth noting that the limit argument does not permit one to discriminate between perfectly competitive equilibria. For instance, perfectly competitive equilibria that are unstable under all reasonable adjustment processes are limit points of monopolistically competitive equilibria.

As we have seen, it is not sensible to assume that all agents to act in a Cournot manner and we have considered the case where there are some price-takers. But Cournot bahaviour does not seem appropriate when one considers goods traded between large firms. A more general solution concept, e.g. an equilibrium based upon conjectures (rational or otherwise) about how other agents will respond to a change in production plan, may be applicable to more circumstances but is likely to introduce considerable indeterminacy into the model.

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Asymmetric information

Perfect competition and incentive compatibility in differential information economies

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As a game theorist, I am specially interested in the problem of how to *implement* competitive solutions. By *implementation,* I mean the construction of (strategic, i.e. "noncooperative") games the solutions (Nash equilibria) of which coincide with (or at least contain) the competitive ones. (I thus have a fairly general definition in mind; I think of the general implementation of social welfare correspondences as well as of particular mechanisms like market games *à la* Shapley and Shubik). The previous question can also be formulated in "cooperative" terms (the core has been extensively studied in the complete information context; active research has recently been started in models of asymmetric information – e.g. Allen (1992)). The advantage of the noncooperative approach is that it provides explicit scenarios which may help us to identify market mechanisms. Strategic outcome functions, strategic market games, etc. are already useful tools to understand complete information economies. When agents do not share the same information, the market process becomes complex; the precise analysis of the agents' behavior is crucial to explain how the information scattered in the economy influences such fundamental parameters as prices.

The present paper is essentially a *collage,* made of selected paragraphs from recent articles dealing with the implementation problem. I focused on this theme and did not attempt to illuminate the relationship between this literature and other work on economies with asymmetric information. I hope that this will be part of our discussion.

1. The messages from information economics and the defects of the rational expectations equilibrium

Postlewaite and Schmeidler (1987) propose a general equilibrium model in which different agents have different information about the economic environment. They begin their article with the following summary (which seems particularly relevant to our session): "The last three decades have seen the development of what is probably the most general model in economic theory - the Arrow, Debreu and McKenzie model of general equilibrium theory. Within this model it has been possible to prove the existence of equilibrium under very general conditions and to prove that the equilibrium allocations are Pareto efficient. Over the last decade, however, there have been a plethora of papers dealing with (sometimes) less general models in which either the existence or the efficiency of equilibria is called into question. Akerlof, in his famous "Lemons" paper (1970), showed that in a model in which buyers and sellers have asymmetric information about the quality of a product, it is possible that the equilibrium outcome is inefficient. [...] Kreps (1977) made a single change in the Arrow-Debreu-McKenzie model, namely, that one agent would know the state of nature and a second would not. He showed by example that there might not be an equilibrium when the uninformed agent used the information that might be contained in the price."

Milgrom (1981) already notes: "Since the introduction in the early 1950's of the Arrow-Debreu-McKenzie theory of general equilibrium under conditions of certainty, a large literature has evolved seeking to extend that theory to accomodate production and trade under uncertainty. [...] When each trader is endowed with his own private source of information, or when traders can acquire information at a cost, the traders' strategic options may be drastically different than in the case where all information is public. It may be possible, for example, for a trader to infer information from the terms of trade he is offered or, more generally, from any observations he makes concerning the behavior of other traders. Some rational expectations equilibrium (REE) models attempt to capture this process of inference. I argue below that the existing REE models are defective in important ways, and I offer a partial alternative which escapes these defects. [...] The problem here arises from the expanded definition of price taking behavior. Traders ignore their information because they see it reflected in the prices. But how does private information come to be reflected in prices if no trader uses his information? [...] To address seriously such questions as: (i) "How do prices come to reflect information?", (ii) "Are there incentives to deviate from price-taking behavior?", and (iii) "How do information gathering decisions affect prices?", one needs a theory describing how prices are formed."

In the presentation to the 1986 Journal of Economic Theory symposium "Strategic Behavior and Competition", E. Kalai writes: "Six years after the 1980 symposium "Non-cooperative Approaches to the Theory of Perfect Competition", we find ourselves in the middle of an explosion of research activity on the role of strategic behavior in economics. [...] The extent of the strategic behavior revolution is substantial. It has led to some fundamental methodological changes in major areas of economic theory including general equilibrium, decentralization, industrial organization, and social choice. [...] We can use the 1980 and this 1986 symposium issues as snapshots in order to see the development of the theory of strategic behavior and competition. One observes the following trends.

First, there is a growing tendency to use the Harsanyi-Bayesian-Nash equilibrium concept as opposed to the simpler Nash equilibrium concept.[...] This allows researchers to model informational issues in an analytical way."

Blume and Easley (1990) make the following comments: "In modeling economies with differentially informed traders it is crucial to recognize that each trader will attempt to infer the private information of other traders from observable market statistics. This idea appears in the concept of rational expectations equilibrium. This equilibrium concept couples the usual market clearing condition with the requirement that each trader's inferences from market prices be correct. Unfortunately it is not obvious that the information transmission in the market required by the REE equilibrium concept is feasible given the market institutions. As Radner (1979) points out: "*A thorough theoretical analysis of this situation probably requires a more detailed specification of the trading mechanism than is usual in general equilibrium analysis.*" [One approach is] to ask when is there a game whose Bayes-Nash equilibria coincide with (or at least contain) the REE allocations."

Dubey, Geanakoplos and Shubik (1987) also "criticize the REE approach to asymmetric information general equilibrium because it does not explain how information gets into the prices. The REE equilibrium literature makes precise a way in which the market might communicate information through the price system, and it has provided an important explanation of certain macroeconomic and financial phenomena. But the difficulty with the REE model is that it does not raise the concomitant, and critical question: how is information put into prices in the first place? This step in the market process is the central problem that we must attempt to solve."

One cannot consider the implementation of competitive allocations in differential information economies without raising two questions. First, as suggested in Postlewaite and Schmeidler (1987) and Kalai (1986), the effects of incomplete information have been extensively studied in various specific economic models (signalling, auctions, ...). Game theory has often been applied succesfully in this context. What is the relevance of these recent achievements to competitive analysis? The second question is related to the first one and concerns the competitive solution concept itself. Should we stick to the famous *rational expectations equilibrium* (or at least to some expectations equilibrium)? Or should we move to other solution concepts? (see e.g. Prescott and Townsend (1984a,b) and the recent work by Younès). Note that this problem is usually overlooked in the implementation literature (where implementation is understood in a technical sense). This literature models abstract social welfare correspondences (not necessarily Walrasian ones). To sum up: should we try to implement classical solutions as rational expectations equilibria or should we derive new solution concepts for competitive economies with differential information in the light of the game-theoretic approach?

2. The negative results of the classical implementation approach

Laffont (1985, 1989) claims that the "Green-Lucas" equilibrium (see Green (1973), Lucas (1972) but also Radner (1967, 1968)), if it exists, is implementable as a "dominant Nash" equilibrium. However, Laffont considers a specific economic setup (in particular, a continuum of agents) and constructs a rather artificial mechanism. Each agent observes an unbiased estimator of the state of nature; the law of large numbers guarantees that the aggregated signal of all agents coincides with the true state of nature. The incentives problem completely disappears. But this phenomenon is not typical of economies with a continuum of agents (see below); it crucially depends on the underlying informational structure.

Postlewaite and Schmeidler (1986) – henceforth PS (86) – provide necessary conditions for implementation of social choice correspondences in differential information economies and slightly stronger conditions which are sufficient to guarantee implementation. "Our goal in this paper is to merge several central ideas in economic theory: strategic behavior (incentive compatibility), differential (or incomplete) information, and the Arrow-Debreu-McKenzie model of general equilibrium. By strategic behavior we refer to the literature which models economic institutions as games in strategic form and uses Nash equilibrium as the solution concept. This literature, motivated by informational decentralization questions, deals not with a single economic environment and a single game, but rather considers *a class of environments* and a *strategic outcome function* (game form) which is applied *uniformly* to this class. [...] In the complete information framework the problem of implementation is to *design a strategic outcome function* whose Nash equilibria for *any* economy coincide with the allocations prescribed by the social welfare correspondence. Here we want to design a strategic outcome function for a differential information economy such that the set of the Bayes equilibrium allocations coincide with the social welfare correspondence. [...]"

PS (86) define the following properties: monotonicity (M) (extension to incomplete information of the property introduced by Maskin), strong monotonicity (SM) and nonexclusivity in information (NEI). This latter condition requires that each trader's private information be perfectly predictable by an outside observer who could observe the private information of all the other traders. They first prove that a social welfare correspondence (SWC) which is implemented on an economy satisfying suitable assumptions is monotonic. They also show that given a differential information economy satisfying suitable assumptions and NEI, a SWC satisfying SM is implementable.

Observe that M and SM are properties of the SWC while NEI is a property of the information structure. Note also that Walrasian SWC are not investigated.

PS (86) go on as follows: "Next we wish to discuss the relationship between our implementation results and the literature on partial equilibrium models using Bayes equilibrium as the solution concept, e.g. the literature on implicit contracts, the principal-agent problem, auctions, optimal taxation, etc. In these latter models some agents typically have private information; at least one agent can distinguish two states which cannot be distinguished by any other agents. Thus these informational structures do not satisfy our NEI assumption. Hence our theorem giving sufficient conditions for implementation does not apply. These models rely on self selection for implementation. If the strategic outcome function gives an agent the proper incentive to "reveal" his information, it is not necessary that any other agent possess this information. Our sufficiency results focus on the informational structure. Note, however, that monotonicity remains a necessary condition independent of the informational structure and the number of agents."

PS (86) then introduce a weak form of incentive compatibility (self-selection), which takes account of the possibility that deviation from truth be detected. They finally prove that *monotonicity and self-selection are necessary conditions for implementation; strong monotonicity and self-selection are sufficient conditions.*

Postlewaite and Schmeidler (1986) consider general social welfare correspondences. Their work has been extended by Jackson (1991) and Palfrey and Srivastava (1987, 1989). Blume and Easley (1990) – BE (90) – fix a social welfare correspondence, the Walrasian correspondence, and ask what conditions on the information structure are necessary for this social welfare correspondence to be implementable for all well-behaved preferences and endowments. They show that NEI (non-exclusivity in information, see above) is a necessary condition for the weak implementation of a large class of Walrasian equilibria, called expectations equilibria.

BE (90) note that the proof of this result "depends upon two ideas. First, there are some economies for which any statistic must induce full information in a Walrasian equilibrium. Second, this set of economies is sufficiently rich that it includes economies for which truth-telling does not pay. For these economies a counterexample to implementability is constructed. Palfrey and Srivastava (1986) use similar Cobb-Douglas examples to show that a revealing REE need not be incentive compatible. [...] In contrast to the Palfrey and Srivastava (1986) example ours has a unique REE, which is revealing."

BE (90) then propose an asymptotic analysis for sequences of economies with ever larger numbers of traders. "The necessity of a condition like NEI in the finite trader case is not surprising, because traders may have residual market power. We demonstrate the necessity of the straightforward asymptotic version of public predictability in the following sense: If the condition fails, then the gains to exploiting private information do not disappear as the number of traders grows. [In other words] an asymptotic version of NEI is necessary if the gains from misrepresenting information are to shrink to 0 as the number of traders grows. Our asymptotic version of NEI is a necessary condition for any version of approximate implementation in large economies."

NEI is a very strong condition; if we believe in the implementation approach, the result in BE (90) leads us to question the relevance of (rational) expectations equilibria. On the other hand, implementation is typically applied to classes of economies; it is thus not surprising that strong restrictions must be imposed on the informational structure in order to guarantee the implementation of a given social welfare correspondence. The problems apparently disappear if one considers abstract social welfare correspondences as Postlewaite and Schmeidler (1986) do; incentive compatibility (self-selection) becomes then an admissible necessary condition for implementation. Still the search for sufficient conditions remains problematic; this often leads to the construction of artificial mechanisms. As Blume and Easley (1990) point out: "The concept of (even strong) implementability is insufficient to address the possibility of some market institution realizing equilibrium outcomes because it imposes no requirements on the "reasonability" of the implementing mechanism. [The standard] research constructs direct revelation mechanisms which bear no resemblance to any market process."

3. Strategic games

Wilson (1977) and Milgrom (1979) have studied models of a sealed-bid tender auction for a single indivisible object, with each bidder having only sample information about the object's value. As the number of bidders goes large, the winning bid may converge to the true value of the object.

Milgrom (1981) analyzes a more general model. He "develops a bidding model (the Vickrey auction model) with explicit rational expectations features. The model has several desirable properties which are missing in many REE models. These are: (i) The bidders act as price takers because they cannot, in fact influence their prices. [...] (ii) The prices vary directly with underlying qualities. Higher prices indicate better quality. (iii) There is no tension between the informational efficiency of prices, the incentive to gather information, and the possibility of reaching an equilibrium. (iv) The process by which prices are formed is made explicit. (v) For most model specifications, the price at which trading takes place is not fully revealing. (vi) All profits earned by the bidders arise either as gains from trade or as a result of speculation based on private information. Poorly informed speculators can only lose. The bidding model also has two principal shortcomings relative to REE models. First, it is a one-sided market model; the sellers play a purely passive role. Second, the buyers have limited options: each can acquire only one of the objects being sold. Each of these weaknesses needs to be addressed by further research.

As pointed out by Milgrom himself, the weakness of his analysis lies in the fact that the economic model is very restrictive. Dubey, Geanakoplos and Shubik (1987) – DGS (87) – consider strategic games which are sufficiently general to provide insights for the study of exchange economies with differential information. Their model is an incomplete information, two stage variant of the market game introduced by Shapley and Shubik (1977) (see also Dubey and Shubik (1978)). "We consider a model with an explicit process for the flow of information via prices. [...] The Nash equilibrium approach we take in the specific continuous game we first analyze retains many of the desirable features of REE. First, it reduces to Walrasian analysis when there is no asymmetric information between agents. When information is disparate, a Nash equilibrium generically exists. Second, there can be no gains from pure speculation in a Nash equilibrium, a fact that also holds for REE. Third, prices may reveal information in a Nash equilibrium to all agents and thus tend to bring about efficient markets. But since the transmission of information (both into and out of prices) takes time in the game, agents who have superior information can earn initial profit from it. [...] We mostly work with a specific game mechanism due to Shapley and Shubik. [...] With separable utilities the second period Nash equilibrium prices and allocations will (generically) be exactly the same as the REE prices and allocations, not because agents learn from second period prices, but rather because they already learned from first period prices. [...] Since there is a continuum of traders of each type and the utilities are separable between time periods, a player will make his first period move simply to maximize his first period payoff, assuming correctly that he can have no effect on his second period payoff, or on the first period price."

In DGS (87)'s model, Nash equilibria possess many of the properties of REE; in particular they are consistent with the weak efficient market hypothesis that tracking past prices is not profitable. However, in Nash equilibria (unlike in REE) individual expert knowledge can be exploited.

DGS (87) make precise the distinction between Nash equilibria and REE. "No one mechanism can possibly describe the variety of market processes that occur in real life. It might well be asked to what extent our results hinge on any particular choice of the mechanism. We show that in fact our analysis remains intact across a wide group of smooth mechanisms. Even replacing smoothness with a weak requirement on the mechanism, the Nash equilibrium approach retains its distinction from the REE. Precisely: given any continuous mechanism, there is an open set of economies, each of which has a REE that cannot be achieved via the mechanism in question.[...] no continuous mechanism can implement the REE correspondence."

DGS (87) is an interesting paper because it is the first one to deal with a multistage market game with incomplete information. However, this analysis does not provide the REE approach with very convincing foundations. The no implementation result seems more robust than the generic equivalence between REE and Nash equilibria. First, the underlying multistage game does not make it possible for the players to get new signals during the course of the game; the players observe past prices and update their information. This is a simplistic representation of an economic situation evolving over time. More importantly, the equivalence between Nash and competitive solutions is guaranteed by a continuum of agents. This trick is common in complete information economies but may take different forms once the agents do not share the same information. Here, every agent is replicated *ex post*, so that there is a continuum of agents with exactly the same information (not just with the same distribution of information). This is a way to circumvent the incentives issues (NEI is trivially imposed).

4. Informational smallness in large competitive economies

Gul and Postlewaite (1992) – GP (92) – write: "Despite the seeming ubiquity of this asymmetric information induced inefficiency, many economists believe that in many circumstances competitive markets generate efficient, or nearly efficient, outcomes. This belief is based on a notion that while there may be asymmetry in agents' information, it is relatively unimportant for the problem at hand because any single agent has only a small amount of information not known by the other agents. It is an attractive idea that there should be a concept of an agent's being informationally small, and when agents are informationally small, the inefficiency due to asymmetric information is small."

We have seen above the role of a property like NEI. We also presented some naive models of informational smallness (Laffont (1985), DGS (87)). Palfrey and Srivastava (1986) choose a more sophisticated, stochastic model which allows them to replicate the economy so that replicated agents do not necessarily share the same information. Palfrey and Srivastava show that any allocation that is achievable with complete information can asymptotically be achieved when the number of replications goes to infinity. The replication process considered in GP (92) (see below) captures informational smallness in a simpler way than the one of Palfrey and Srivastava. GP (92)'s replication process eliminates the conflict between efficiency and incentive compatibility.

5. Axiomatic mechanism approach

This approach consists of identifying properties that should be satisfied by the competitive allocations of a differential information economy. For instance, GP (92) select the following properties: feasibility, incentive compatibility, (ex post) individual rationality and (approximate, ex post) efficiency. They propose conditions on a (replicated) economy with asymmetric information that guarantee the existence of allocations satisfying the previous requirements. As one may expect, this result does not apply to Akerlof's lemons problem. This axiomatic approach is in the tradition of general equilibrium analysis while making the incentives constraints explicit.

GP (92) illustrate their result on a transparent example (pp 1275-1276):

States of nature	**Value of a smoke alarm**	
	to B	to A
1/2: B smokes	8	10
1/2: B does not smoke	1	3

The only IC, IR (deterministic) solution is that B sells the smoke alarm only if he does not smoke, at a price between 1 and 3. This solution is not ex post efficient (not efficient if B smokes). With 2 agents, this example has essentially the same features as Akerlof's lemons market. However, here, the quality of the object for sale is identical in both states of nature. This has consequences when the economy is replicated. GP (92) consider i.i.d. replications of the "cohorts" (A,B); in each cohort, B smokes with probability 1/2. Suppose that sale can only be realized at the price 5. Then truthful revelation is incentive compatible: non-smokers want to sell and smokers do not. The trick is that A-agents coupled with a smoker (who value the smoke alarm at 10) buy it from B-agents who do not smoke and value it at 1. If the number of smokers is equal to the number of non-smokers, this solution is ex post efficient: the agents who do not trade are sellers with value 8 and buyers with value 3. The law of large numbers guarantees asymptotic efficiency.

Obviously, the replication process proposed by GP (92) does not capture the characteristics of Akerlof's market. Nevertheless, this shows that some forms of informational smallness are not completely artificial and may solve the incentives issues.

6. Summary

1) The best known solution concept for competitive economies with differential information is the rational expectations equilibrium (REE). The REE approach is elegant and synthetic; it applies to general, abstract economies. However, the REE framework does not provide any description of market mechanisms, which would seem to be crucial to understand basic features in economies with asymmetric information (for instance the process of aggregation of disparate signals into prices). In particular, REE do not involve any incentives constraint.

2) Recent research has been devoted to the implementation of REE. Different models have been considered, from classical implementation of Walrasian correspondences to strategic market games. The results are rather negative: the implementability of REE imposes strong restrictions on the underlying information structure.

3) A requirement like *nonexclusivity in information* seems to go beyond the effects of perfect competition on the information pattern. Nevertheless, *informational smallness* of isolated agents may be a reasonable assumption to make in competitive economies. The agents' incentives to misrepresent their information may disappear under suitable forms of informational smallness.

4) Some authors (e.g. Radner (1979), Milgrom (1981)) argue that general equilibrium analysis is no longer appropriate when the information structure becomes complex. They claim that a detailed specification of the trading mechanism is necessary. Such a

view leads to the use of game-theoretic tools. The obvious drawback of this approach is that the outcomes of a particular trading mechanism depend critically on the rules of the underlying game. Nevertheless, the study of specific market scenarios may resolve paradoxes (e.g. of information transmission) and has proved successful in Information Economics and Industrial Organization. The solution concept used in this context is the Nash equilibrium, which implicitly incorporates the incentives constraints.

5) Other authors propose abstract models which seem in the spirit of general equilibrium analysis. Postlewaite and Schmeidler (1986) characterize implementable social welfare correspondence (not necessarily Walrasian ones). Incentive compatibility obviously appears to be a necessary condition. Gul and Postlewaite (1992) adopt a more axiomatic approach and identify properties that should be satisfied by the competitive allocations of a differential information economy. Under suitable assumptions, they prove an existence theorem.

6) Sophisticated models as those surveyed above do not eliminate the difficulty pointed out by Akerlof (1970); in a very simple and natural economic environment, there might be a conflict between incentive compatibility and (ex post) efficiency.

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General equilibrium: asymmetric information*

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I would like to assess how the phenomena that have been associated with *asymmetric information* – moral hazard, adverse selection, incomplete contracts – have affected the *conceptions* that are associated with the Walras, Arrow, Debreu and McKenzie general equilibrium construct. I will not limit myself to the positive aspects of the construct but also to its normative or welfare aspects so that I could refer to the Walras, Pareto, Arrow, Debreu and McKenzie "culture."

Let me distinguish the impact of asymmetric information ideas on this so-called culture by order of increasing ambiguity: I will first stress the domains where the message is the most clear-cut and then go to domains where it is more ambiguous.

To me, the *most clear-cut message* of the asymmetric information literature concerns the limits of "first best" welfare prescriptions. The most *prominent reference* of first best welfare analysis is the *second theorem of welfare economics*. The theorem suggests that redistribution and financing problems can be solved through the implementation of appropriate lump sum income transfers. However, *actual tools* used for redistributing income or financing public goods – commodity taxes, income taxes – are distortionary. Asymmetric information is clearly an important *clue* for trying to explain the *discrepancy* between ideal lump sum transfers – whose implementation requires the knowledge of all characteristics of the agents that are relevant – and *"rough" taxation systems* – that use less information or extract it in an incentive compatible way. Indeed, under some extreme circumstances, the best a *central Planner* who wants to redistribute income and finance projects can do is to use standard tax systems. This is true in some sense, in a large economy in which *agents' characteristics would be drawn at random from* a known distribution in a way that would make them *uncorrelated*. Naturally under other circumstances *more "fancy" implementation* mechanisms that exploit *symmetric information* between agents and more generally the *correlation* of individuals' information

* This text is a transcript of my oral intervention in the session on *Asymmetric Information* during the General Equilibrium 40th Anniversary Conference. A more detailed investigation of some of the ideas presented here is contained in "The Arrow-Debreu paradigm faced with modern theories of contracting: a discussion of selected issues involving information and time." Proceedings of the Nobel Symposium on contracts, Basil Blackwell, 1992.

would allow to *do better*. However, asymmetric information places *limits to redistribution* or more generally *limits on central intervention and limits on the gains of exchange* that are much stronger than what the first best analysis of the second welfare theorems would suggest. *This is a very important message for our discipline.*

The second lessons of the asymmetric information literature concern the nature of *competition and the generality of modelling options à la Walras.*

R. Radner had stressed in the sixties the problems of trading between agents when they have *different structures of information.* However the solution he suggested clearly differs from the one that follows from the modern theory of contracts. To which extent is the present solution compatible with the conception of competition that is implicit to the Walras model ?

One could argue for compatibility on the following grounds. In order to extend the validity of the Arrow, Debreu and McKenzie reduced form, we should consider contract as the basic object of trade. Take for example a *moral hazard context* in which agents *undertake risky activities.* A contract specifies some algebraic payment to the agent in the good state and some payment in the bad state. Such a contract can be given a *price*, and given the *price* a firm can *compute the expected profit* that it will give – since it can compute the effort level that it will induce. In fact, the production of such contracts, in a world in which the number of agents is large enough to make the law of large numbers approximately true as soon as a small proportion of the population takes the contract, is almost *constant returns to scale.* A general equilibrium in contracts, where each possible contract has a price and in which firms have a profit maximising supply of contracts and individuals a utility maximising demand of contracts, is analogous when contracts are subject to an *exclusivity clause* – a crucial restriction – to a standard Walras equilibrium and may lead to (second best) efficiency. Indeed the analogy is even closer when – as shown by Prescott-Townsend (1984) – contracts are stochastic i.e. they are lotteries, a fact that even restore convexity.

In fact, this is a too extreme story in which the increasing returns to scale of insurance activity are too much discarded - a large firm can take more advantage of the law of large numbers than a small firm – and reintegrating these neglected features leads to interesting stories. But the point I want to make is something else. The above story that extends the Arrow, Debreu and McKenzie concept of commodity to more complex objects called contracts necessarily breaks down when adverse selection rather than moral hazard is taken into account.

Consider the same insurance problems when the "risk" of the agent is not known from the insurance company (low risk, high risk). We can still define a contract by a contingent payment and still price it. However now a central difficulty appears that can be briefly ascertained in as follows. The new object has a profitability that does not only depend upon its characteristics but *upon who* "consumes" it (it is more profitable if "consumed" by low risk people). Hence it is very difficult to extend the standard walrasian story: certainly, in equilibrium the proportion of people who "consume" the contract can be assumed to be the actual proportion. But what about contracts that are not offered in equilibrium but that, in the Walras spirit, have to be priced ? Their profitability – and hence the equilibrium – depends upon *conjectures* on who they would attract. These difficulties are behind the Rothschild and Stiglitz (1976) model in particular and are at the heart of the existence or multiplicity problems. They have been analysed in particular by Gale (1987), have not, to the best of my understanding, a "Walrasian" solution. In other words *they raise problems on the nature of competition that were overlooked by standard competitive models.*

The third message I want to emphasize is one that *I find ambiguous.* In the standard abstract model – say chapter 7 of Debreu – agents have subjective probabilities on states of nature that are fixed once for all (and not revisable). In the modern theory that introduces Aumann's common knowledge and imports Harsanyi's views, agents are willing to change their own probabilistic assesments as soon as they learn about the others' assesments.

At one end of the *spectrum* – risk neutrality, bilateral relationship – an agent will refuse to trade with another agent that would accept to trade with him. As well, one of the Marx's Brothers did not want to join a club that would be so undistinguished so as to accept him! At the *other end of the spectrum*, prices will *convey information* in equilibrium or even will *reveal it completely* – a source of a well known *paradox*: if prices transmit information freely why should agents try to acquire it in a costly way ? In fact, for reasons that would be a little long to explain – but that mainly refer to the *process of getting such "revealing" equilibria*, – I tend to believe that in its present state, the theory *does not provide satisfactory answers concerning the amount of information* that is actually conveyed through prices by modern market economics.

After this expression of skepticism, my fourth and last point concerns a *message that is* negative rather than ambiguous.

The literature on asymmetric information has produced many alternative variants of the functioning of the credit or financial markets. Let me take two examples that are extreme in terms of dates and content. Stiglitz and Weiss (1981) have argued that adverse selection leads *risky firms to demand funds with high interest* so that the optimal contract *rations* less risky firms for meeting self selection requirements. Recently Hart and Moore (1991) have argued that the main features of the credit contract is its incompleteness so that the contractual influence of the debtor lies essentially in his power to seize fixed assets. Others put at the forefront the interconnections of the debt-equity issue – etc –. From this literature, what we should do seems ambiguous; what we should not do seems clear: we should stop – in partial as well as in general equilibrium settings – to model financial markets as pure Walrasian markets in which the interest rate is the adjustment variable.

A brief conclusion attempts to open doors to reflection.

The asymmetric information literature is populated of a large number of animals. This is a kind of rich zoo with many species. Clearly some of these animals have relevance for understanding issues that are at the periphery (and maybe at the heart) of general equilibrium conceptions - normative global policies, modelling of competition, credit market. Does it make sense however to bringing this zoo in the elegant (and relevant) territory of general equilibrium ? More generally, up to which point does it make sense to increase the number and complexity of phenomena that we want to apprehend in a single general equilibrium setting ?

The question raised yesterday comes back into this debate: deciding what should be modelled in a general equilibrium framework and what is better investigated in a partial equilibrium context is an important and sometimes non-obvious research strategy decision.

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Asymmetric information in general equilibrium models

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Prior questions:

1. Do we need to put asymmetric information into general equilibrium models ? A first response in the form of a second question is

2. How well do general equilibrium models predict markets, institutions, and outcomes without asymmetric information ? – assuming full commitment, few other impediments to trade, and the usual "regularity" conditions.

A possible response - go take a look

My own initial efforts took me to the northeast part of Thailand, looking for an Arrow (1964), Debreu (1959), McKenzie (1954) economy! In particular, my focus was on risk, that is, allocations where the commodity point is expanded to include all possible "states of the world." Naturally, one goes to village economies which suffer from high risk and are poor, so that low realizations matter.

A clarification – Here an Arrow, Debreu and McKenzie economy is an economy viewed as a general equilibrium model in which preferences, technology, and endowments are all specified. One can then either solve for Pareto optimal allocations, or one can attempt to decentralize an optimum with a complete set of securities, or subsets of securities traded intertemporally if these span the space of all possible returns. By a search for an Arrow, Debreu and McKenzie economy I mean only that allocations be Pareto optimal, not that a complete set of markets necessarily exists. Indigenous institutions such as gift giving networks may suffice to achieve an optimum. I shall return to this distinction later and focus first on the optimum problem.

Empirical implications of full risk sharing - or perfect markets and institutions

The work of Robert Wilson (1968) and Peter Diamond (1967) as elaborated on by my colleague at Chicago, Jose Scheinkman (1984), has taught us that under some "regularity" conditions – all households are risk averse, all discount the future utility at the same rate, all have common expectations – various strong propositions apply:

1. Household consumption should comove with aggregate consumption – possibly also with aggregate leisure if preferences are nonseparable – assuming no binding corner solutions. Controlling for aggregate risk in this way, consumption should not move with idiosyncratic shocks – sickness, unemployment, household specific income shocks, etc. We also know from the neoclassical separation theorem that

2. There should be efficiency in production independent of a household's wealth, status, owned resources, and apparent access to risk reduction mechanisms.

The point is that these implications can be tested given sufficient data. My own effort to do this was not in villages in northeastern Thailand, but rather from three villages in southern India, as these were sampled for ten years by the Institute for Crops Research of the Semi-Arid Tropics, see Townsend (1994). Though statistically one rejects full risk sharing, the data are striking; idiosyncratic shocks matter little in the determination of household consumption, despite the poverty and the enormous fluctuations in these incomes. I suppose an implication of this work is that general equilibrium models without asymmetric information provide a surprising good approximation in some instances. Perhaps we are on the right track in working with general equilibrium models and the state-contingent commodity space.

Actually, there are observations in the ICRISAT villages which are anomalous to full information models, e.g., sharecropping. More on this momentarily. And a series of research efforts with large scale data sets has generally rejected full risk sharing, providing some guidance about where and for whom markets or institutions are incomplete: Cochrane (1991), Mace (1991), Altonji-Hayashi-Kotlikoff (1989), Altug-Miller (1990), Rashid (1990), and Deaton (1990a). Related, field research and village level studies have provided mixed conclusions. Udry (1990), in Nigeria, finds all sorts of contingencies in borrowing-lending contracts, while Townsend (1992) finds some anomalies in villages in northern Thailand, where I have now conducted extensive field research and done a preliminary survey. I shall return to this momentarily. Finally, an evaluation of some well-known programs targeting credit to the poor, to women and to those in remote areas – e.g. my work with Rashid (1993) at the World Bank on the Grameen Bank in Bangladesh, SEWA in Ahmnabad in India, and FINCA in Latin America – turns up striking contingencies in loan contracts but also turns up procedures such as monitoring and group formation that suggest that private information is an important impediment to trade.

An affirmative answer to the prior question

In summary, then, we need to put private information into general equilibrium models for several reasons. (1) Sharecropping and other contracts seem inconsistent a priori with perfect risk sharing. (2) Large scale data sets generally reject perfect markets and institutions, suggesting moral hazard and adverse selection problems may be prevalent, and (3) Ongoing credit programs and policy remedies need to deal with incentives, that is, to try to mimic, if possible, what would be an optimal mechanism in the context of a reasonably well articulated environment with private information problems.

Technical and analytic problems with a report on progress to date

The literature on contract theory and optimal mechanism design which flourished from about 1975-1985 suffered from various technical difficulties which seemed to limit applicability: (1) One might make some progress on two-person contracts, e.g. firm-worker, or principal-agent models, but embedding these into a larger general equilibrium context seemed arbitrary; more on this momentarily. (2) There were difficulties in proving theorems characterizating information-constrained outcomes – e.g., see the literature on sharecropping. To get the sharing rule to be monotone increasing with output one needed separability of the utility function of the agent in consumption and leisure, a monotone likelihood ratio condition, a convexity of the distribution function condition, and so on. Despite pleas to the contrary, the applicability of these models was brought into question. (3) The models seemed severely limited in dynamic aspects. In particular, the message of Rogerson (1985), or Townsend (1982), was that optimal multiperiod contracts should take into account entire histories of past realization of shocks, not an easy space in which to prove theorems, other than the necessity of such tie-ins.

These problems have been overcome to some degree, though much work remains. On problem (1), Prescott and Townsend (1984) and others have shown that most contracting, mechanism design problems can be converted without loss of generality to problems in the space of lotteries or random allocations. Despite incentive constraints, these spaces are convex and allow beneficial gains to trade. Occasionally, one can characterize solutions analytically, and, if not, one can happily resort to computing solutions numerically via linear programs. Sometimes numerical results give conjectures about theorems which can be proved.

On problem (3), dynamics, progress has also been made. In particular Abreu, Pearce, and Stachetti (1986), followed by Spear and Srivastava (1987), Green (1987), Phelan and Townsend (1991), and Atkeson and Lucas (1991) have shown in one context or another that current promised utilities are natural state variables capturing the past, or equivalently, that future promised utilities are natural variables to be assigned today. A related analytic literature of Fudenberg, Holmstrom and Milgrom (1990), and also Allen (1985), has shown that optimal information-constrained multiperiod contracts reduce in a sense to a series of static agreements if there is unlimited access to outside borrowing and lending. Still, some are left wondering if this is a reasonable information specification, suggesting that one go take a look, either directly or indirectly.

Tests of asymmetric information general equilibrium models

The numerical methods described above have greatly expanded our ability to take seriously general equilibrium models with private information in an evaluation of what we see. Specifically, working with large scale data sets, Christopher Phelan (1990) in his dissertation at Chicago has asked whether the observed increase in the cross sectional variance of consumption by age is consistent with principal-agent type models with intertemporal tie-ins. He argues that private information models do fit the Consumer Expenditure Survey data better than would a full information model, but there are remaining anomalies. Ethan Ligon (1993), currently working on his dissertation at Chicago, has shown that the ICRISAT data from villages in southern India are more consistent with a long-term incentive contract model then they are with either a full information model or a pure borrowing-lending model, that is, with a sequence of static agreements. The inference is that asset transactions of potential borrowers can be observed and restricted by primary lenders. (See also the dissertation of Youngjae Lim (1992).)

Village level studies also take asymmetric information, general equilibrium models seriously and on occasion encounter anomalies. Specifically, Townsend has asked in his village level survey in northern Thailand whether the anomalies of full information models can be reconciled with the addition of private information. If one views village markets, institutions, and outcomes as solutions to an optimal mechanism design problem, then limited risk sharing, the importance of idiosyncratic shocks, and land fragmentation can all be explained, though one should emphasize quantitative, not just qualitative outcomes, as in my work on land fragmentation in the Medieval Village Economy (1993). Still, private information models cannot explain why the relatively well-off in one village only partially smooth consumption with rice storage and livestock transactions, remaining disconnected from credit markets or other village institutions. Similarly, one cannot explain why the poor in another village suffer fluctuations by working harder, without resort to credit markets or storage. Finally, attempts to measure the degree of shared information turn up a correlation between the degree of activity in financial markets in villages and the size of common ex post information sets. Apparently, information is not perfect even at the level of the villages. It is an important factor. But there may be other factors determining both activity in credit and insurance markets and the degree of shared information.

Issues in decentralization

Thus far I have avoided the issue of decentralization in general equilibrium models with asymmetric information, that is, the distinction between mechanism design problems, on the one hand, and the existence and optimality of competitive equilibria, on the other. In fact, this issue emerges clearly in the context of an applied general equilibrium literature on credit and insurance. Specifically, the work of Stiglitz and Weiss (1981) and of Rothschild and Stiglitz (1976) has been taken as symptomatic of the difficulties of the operation of competitive financial markets with private information, justifying government intervention. It should be stressed here, however, that these results do not flow naturally from either the optimal contract nor the decentralization literature. In particular, Stiglitz and Weiss impose a limited financial contract without risk contingencies, allowing only bankruptcy, and Rothschild and Stiglitz impose a particular form of interaction among financial competitors, as in a Nash game. In contrast, Prescott and Townsend (1984) have argued that many environments with private information can be decentralized with ex ante competition, drawing heavily on the general equilibrium literature of Arrow, Debreu and McKenzie. In effect firms bid for the right to provide financial services. Boyd, Prescott and Smith (1988) have invented the notion of a private information core; this embodies strategic aspects but yields nevertheless one of the information-constrained Pareto optima. The literature on bank runs has also argued that appropriate deposit insurance is merely the solution to a mechanism design problem.

More work is needed in this area, to decide what model of competition is reasonable a priori and what models predict well in practice. There is certainly ample evidence from the field research of Aleem (1990), Siamwalla and others (1990), that credit markets do suffer from information problems. But in practice these are partially alleviated by long term contracts and sorting among relatively few potential lenders. There are important policy conclusions which stem from this work. Are lenders necessarily usurious, to be replaced with target credit ? Should we liberalize financial markets, free all interest rates, and promote competition in all instances ?

Future work - missing impediments to trade

I conclude with two areas where theoretical results and empirical work are sparse. One is whether communication of information and shocks is more difficult than general equilibrium models with asymmetric information allow. A second is the issue of limited contract enforcement. More generally, one faces the question of why are there firms, that is, firms embedded in markets, or in the applied credit context, what is so special about the small joint liability groups which are featured in Grameen Bank schemes. As in the work of Varian (1990), Holmstrom and Milgrom (1990), Tirole (1985), and Besley and Coale (1991) one hopes in the end to address these questions not by abandoning the general framework but rather by building upon it.

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Some remarks on information

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What is fundamentaly interesting in the General Equilibrium approach to economics as a social science is general interdependance. The mere fact that many markets are taken into account – and for instance that labor markets and consumption good markets are simultaneously pictured – forces us to analyze feedback effects – and for instance the fact that production entails distribution of incomes which leads to demand and thus to production. It is possible to go further and to say that many partial equilibrium problems can be analyzed only by embedding them in a larger framework which can include features which are not only the usual ones.

That does not mean that other approaches (Game Theory, for instance) must be discarded. On the contrary, one may think that economists who were working inter alia in General Equilibrium Theory were very tolerant about the tools which are used: the emphasis was on rigor (maybe too much) and on the substance or the interest of the problem but certainly not on the approach or the specific tools. We can refer here to the founding fathers of General Equilibrium with Uncertainty like K. Arrow, G. Debreu, L. McKenzie and R. Radner (it is possible to disagree with the joke of a great French writer who said more or less, "la tolérance, c'est bon pour les maisons qui portent ce nom"). I think that they were basically right. One can complain that it is less the case, maybe that it was less the case a few years ago. Too many game theorists were saying – particularly about information – that only well defined games must be analyzed, and as much as possible with their preferred concepts of equilibrium. They were taking an even stronger point of view than the one which is contained in the so called "Nash Program." Thus I shall take advantage of this tradition to refer also to game theory.

It is possible to begin with asymmetric information about data and actions (not strategies). After I shall say some words about asymmtric information on economic agents' behaviour. The last part is devoted to the big questions of the connections between asymmetric information and notions like power or authority and legitimacy. As this note is not a summary of a survey, I shall feel free to pick disconnected questions, in a very unsystematic way, referring as much as possible to open problems. However, one of the main defects of this note is that as it tries only to locate problems, it does not truly analyze contributions.

I - Asymmetric information about data and actions: Markets, contracts and organizations.

I would like to propose to your attention three subjects:

(i) The question of the relationships between some types of frictions: incomplete markets, incomplete contracts and asymmetric information.

(ii) The problem of rent-seeking due to asymmetric information.

(iii) Some research in the theory of investment financing.

1. Some economists are perhaps emphasizing too much on the distinction between contracts and markets. After all, the notion of contract is a legal one. According to French law, buying a pencil at some specified store at a posted price is entering in a contract and a bunch of laws are then applicable by courts. Thus in the market for pencils, we have contracts. No necessary anonymity and reputation effects can arise. We can make a stronger argument for the labour market. Obviously we have different types of markets (or of contracts).

At the very beginning of the eighties, some scholars were saying that when some markets are missing, we must explain their absence in terms of self selection or moral hazard. Some people are still thinking that one must explain the role of money only in terms of incomplete information about data. But a recent literature is explaining that in terms of other frictions, like difficulties of meeting, separated market places, incomplete markets, etc ... After all, we are not gods only because we do not know everything (what does it mean, to know everything ?). We cannot be in many places at the same time, we cannot compute at an infinite speed, it is costly to focus intellectually or to call some knowledge to memory. Moreover, we have different individual, social and national histories.

More recently, some authors used in an interesting way the notion of incomplete contracts, incompleteness due to the difficulty of signing very complicated contracts. Sometimes, the idea is advanced that it is because of courts' information structure but often the justification stops there. It is true that the absence of cross – fertilization between the incomplete market literature (remember that in the sixties, R. Radner was emphasizing – as the founding father of French Planning did and as was perhaps assumed by Keynes – that the investment decision is usually made without the possibility of signing contracts for selling the future proceeds of this investment) and the literature about incomplete contracts is due to the fact that the settings and the problems and the tools used are different. But I cannot help to think that there are also a-priorisms.

In a nutshell, a market can be considered as a set of contracts defined a priori or which arises endogeneously. The terms of a contract include also the number of cocontractors (and sometimes their names). Thus if we assume that multilateral contracts are difficult to sign or to be implemented, we get either the set-up of pairwise meetings or the firm as a nexus of contract and not as a grand contract.

It is not necessarily silly to consider a model where two markets are taken into account: a consumption good market and a labour market. Money enters as a conterpart in each trade and the market structure is thus incomplete, a priori. On the contrary, on each market, the fact that buyers cannot recognize the quality of goods constrains transactions in an endogeneous way because the "qualities" that buyers expect to get are determined at equilibrium. In turn, the incompleteness of the market structure as it is defined a priori constrains the ways possible signals can be used by buyers: one keeps the Keynesian assumption that workers do not buy consumption goods at the firms in which they work.

Thus it is maybe legitimate to marry asymmetric information with other "frictions," instead of trying to explain all "frictions" in terms of asymmetric information.

2. Now, let us consider the relationships between rent seeking and asymmetric information. To introduce the subject, one can refer to the simplest model with one uninformed principal who proposes a mechanism on a take or leave basis to one agent who can be of many types. We know that, usually, rents for good types of the agent arise in such a model and they are due to asymmetric information. Generally speaking, rents due to asymmetric information can arise in many models. But, then the process of rent seeking can be interesting when we ask the question (Williamson) of the choice between organisations (authority and hierarchy) and markets. The analysis is no more in terms of economy of transactions costs, which is said to be socially good. (Recall that some sociologists found that managers of plants of a big firm often prefer, and implement when they can, extrafirm trades to intrafirm trades).

It is nonetheless hard to think that rent seeking in the private sector is only motivated by asymmetric information on the data on moral hazard. Recently, it was pointed out that a large private firm is, from some points of view, like a public bureaucracy and the term "influence costs" was coined for describing rent seeking (Milgrom and Roberts).

More generally, the good places (of authority also) in a hierarchy, but also in the society at large, are in limited supply. This is the general problem of social groups, elites and power.

3. We know that investment financing involves moral hazard and adverse selection. But we know also that even in the group of democratic capitalist economies, there are some differences in the institutional settings of the financing sector (J. Zysman). Until the eighties, it was possible to discriminate among at least four groups:

(i) The "American system" with the important role attributed to the stock exchange.

(ii) The "German structure" where autonomous banks play a large coordinating role because of their close links with industrial firms.

(iii) The "French system" characterized by the leadership of the State and where banks have no strong ties with industrial firms.

(iv) The "Japanese setting" where the financial sector has strong links not only with the economic public bureaucracy but also with the industrial sector.

There are some partial equilibrium analysis, using simple models, in which some aspects of the American setting are emphasized (short termism) and some attempts at formal comparisons. However, I am not aware of any general equilibrium analysis in this setting. That raises an important issue. Many economists think that the state of the art does not allow to draw conclusions in an equilibrium general setting. It is this feeling which explains partly why people working in industrial organization are so suspicious about general equilibrium analysis. It is surely an issue which deserves some discussion.

II - Asymmetric information about agents' behaviours.

The problem is different from the one of moral hazard. It is not the other agents' actions which are not known, but the strategies themselves, or the "equilibrium" strategies. This raises the very problem of the concept of equilibrium itself as it is usually handled. One can define an equilibrium as a state of affairs which is stable with respect to some perturbations. (Even if we agree with this approach, the problem is to define these perturbations in an interesting way, taking into account, e.g., bounded rationality which is surely a difficult thing to do).

In this connection, I would like to refer to two problems.

1. The first one bears directly on the question of multiplicity of equilibria. If there are many equilibria, it is said that economic agents do not know how to act.

My ideas about the so called eductive approach (Binmore's terminology) are not clear. From the point of view of a social science, it is perhaps only an extension to a world with two persons of the stories about Robinson Crusoe that the Austrian Marginalist School used to like so much, one century ago. But exercices in logic are not always useless for a social science (see the developments of the epistemic logic).

I would like to refer here to two questions. The first one bears on the relationships between the incompleteness of the market structure and the number of equilibria. This question is interesting per se and it can be dealt with in terms of real or of nominal securities. But I do not think that giving some answer to this question is automatically providing an answer to the Keynesian question of the volatility of the economy. Two steps must be made for going from the first one to the second one. The multiplicity of equilibria which is at the roots of the problem raises obviously the question of coordination (at some equilibrium) but also one may feel that the notion of equilibrium – as it is conceived presently – is not a good concept for answering such a question.

The second one is intellectually very exciting but very difficult to attack. The idea is that "social norms" must be an ingredient of a model in order to reduce the number of solutions, but not only for that reason. One must be clear that it would be tautological to relabel the equilibria in terms of social norms or conventions (Lewis). Also, I do not think that it would be interesting to refer in this connection to the "evolutive" approach to equilibrium. What is at stake is that the economic agent in General Equilibrium Theory and Game Theory is neatly undersocialized (I shall come back to this problem in the last part).

The association of equilibria with social norms have been analyzed at the level of the relationships among workers in the Japanese firm (Okuno et al.) or in the terms of status (Postlewaite et alii) – In order to be more clear, I shall describe a contribution by A. Greif. He analyzes the difference between the solutions – an individualistic one and a collectivist one – in a repeated game with many players. The stage game concerns the relationship between a merchant and his overseas agent. An agent has to be honest in order to acquire some reputation. A merchant's individualist (anonymous) strategy consists in rehiring an agent who has not cheated him and not rehiring him if the agent has cheated while in a "collectivist" strategy, an agent who has cheated a merchant is not rehired even by another merchant. The author interprets the Latin type of relationships in terms of individualist strategies and the North African type of relationships in terms of "collectivist" strategies. He explains this difference by the strong ties among merchants of the Muslim world. I do not know if the author is an economic historian but he discusses very seriously the historical evidence.

The criticism that it is possible to make is rather obvious. We understand that the cultural ties among the actors of the non anonymous equilibrium are forged somewhere. But these places are not formulated and the analysis rests on one foot: There is no theoretical integration with tools of comparable precision. This is not very good for a possible cumulative progress of the analysis which, in general is much helped by integrated formulations (in some sense, it is a partial equilibrium analysis).

2. It is possible to come now to the second question, the explicitation of the "evolutive" approach. We have models with rational expectations (General Equilibrium let us say) and game theoretic models with or without asymmetric information on the data. As the general ideas are almost the same in the two fields, even if the techniques are not necessary alike, I shall focus on so called game theoretic models. Loosely speaking, they are of two types: learning type and evolution type. In the first class (Jordan, Kalaï and Lehrer, Nyarko and others) repeated games are, in general, analyzed. Convergence to, let us say, a Nash Equilibrium is gotten, through a Bayesian updating process and starting from given priors. The present state of the art requires that the support of each prior contains the equilibrium beliefs.

The main objective of models of explicit evolution is to choose among equilibria some which are particularly appealing (Mailath, Rob, Canning and others). The evolution process is based on some "boundedly rational behaviour" plus some random perturbations called "mutations." Most of the papers in this field are outstanding ones but it is often difficult to link them to some real situation that a social science wants to picture.

Consider a common interest game with signalling. The problem is to select an equilibrium for which the two agents can link in the same way messages and actions. When we use such an evolutionary process, do we mean that we want to analyze how a child learns a language first with his parents and then at school, how she learns in what situations she can expect jokes, double meaning, play of words, pervese use of languages, deceptions, lies ? In that case, it is better to picture truly the social environment in which this learning process appears. Given that I think that such a process cannot be used for describing the emergence of a general language (like English or French) among some group of persons, we are left with the case where two specialists tries to overcome the ambiguities of a general language in order to be able to communicate on some precise matters. But do they learn a common "meaning," a bijection between some subset of messages and some subset of actions in this way ?

In some sense, this literature faces a dilemma.

- Either, the basic conditions change "In the long run, we are all dead."

- Or they are stable and it seems that sometimes the "choice" of a trajectory is not based on such processes of tâtonnement or non-tâtonnement.

III - Power, elites and legitimacy.

It is possible to argue that if the Homo-Economics is undersocialized, the Homo-Sociologicus is oversocialized in the sense that the primary socialization (school, family, social group of origin) determines completely his individual trajectory. A sociologist, Granovetter, has proposed a research agenda in terms of "Embedding" for getting a reasonable average between these two extrems.

However, it strikes me that he did not allude to the analysis of the division of society in large scale groups, elites etc ... It seems to me that these links among economic agents are very important for understanding not only markets and organizations like firms but also macroeconomic policy. Why ? Because these links are transverse to markets, organizations like firms and industries. Some coordination not only about actions but maybe mainly about expectations is gotten in this way. Consider the French elite. Some political scientists, sociologists and economists are telling us that it is very much unitary. This has advantages from the point of view of communication among its members but this may be dangerous if it is going too much out of reality (self perpetuating mistakes which are not challenged in a constructive way).

1. Power and asymmetric information.

In many contributions, it is assumed that the power of the principal is only limited by her imperfect or incomplete information. This is the traditional point of view of optimal taxation (J. Mirlees) where the only limit to income redistribution is often asymmetric information. By a curious change of perspective, in models of similar structure, some economist working in industrial organization (J. Tirole) are borrowing from a well known French sociologist (M. Crozier) the idea that the (only ?) component of power is the possibility of creating uncertainty (because of asymmetric information ?). At the end of the sixties, some philosophers were saying that "Power is information." I do not know exactly how to interpret the above but it is sure that the relationship between power and information is two-sided. Think how powerful groups try to set up communication channels, try to define when a question becomes or does not become a public issue, and in what terms it must be defined. There are many articles in political science about how and when a question is put on the Government agenda. The process must be similar for private bureaucracies.

There is a literature in economics which is connected with these problems. In extensive form games with asymmetric information, we have contributions which connects power with, for instance, impatience. There were attempts to extend the Nash-Bargaining solution to asymmetric information (R. Myerson e.g.). More recently, there are contributions where asymmetric information is taken into account in market games defined in a "cooperative" way (B. Allen e.g.).

2. Elites and asymmetric information.

Obviously, there are many ways of getting information. Through prices, through markets for information (B. Allen) etc ... I would like to emphasize another way, in order to discuss briefly the problem of elites: it is learning by doing and/or by participating. Traditionally, the economic elite contains many circles: from the high people who are supposed to take formal decisions (Heads of State, Prime Ministers) to the agents who are in charge of the diffusion and legitimation of these decisions, through directors of banks, owners or heads of large firms, high civil servants of the economic bureaucracy and syndicates leaders. The main distinctions are made along two axis: pluralist versus unitary elites and pervious versus impervious elites. From the point of view of our subject, a pluralist and pervious elite is a good feature because its fractions have better chances to be sensitive to different social realities while having a common language to be able to communicate needed changes of policy or of expectations.

Remarks: I shall not enter here in the growing literature about political economy, which incorporates asymmetric information.

3. Market, legitimary and struggle for recognition.

When one says that authority (legitimate power) is a substitute for market (contract), it is possible to mean something a little different from the opposition between market and organization. Many social scientists were worried about the growing ungovernability of western societies, at the beginning of the seventies. For instance, one can refer to the intelligent neo-conservative D. Bell. Among them, some economists were worried about the wage explosion of the end of the sixties and/or the beginning of the seventies, the rate of absenteism, the attitude towards work, the will to profit of a free health system. The problem is that the remedy – the market and/or the monetary discipline – is probably as bad, in some circumstances, as the sickness itself. Let us say that, by itself, it cannot implement all objectives. In a capitalist society, no government can accept that potential investors are in a too weak position and it is not a difficult task in our present world to reduce the status and the real wage of workers. Obviously a vicious circle of segregation is then at work. I do not think that I am outside my subject. I say only that side by side with incentive schedules (as they are conceived by theorists for organizations) and high powered incentives of the market, there is the cultural attitude towards work which is inseparable of the social recognition that good work must entail. Every specialist of the labour market knows that an incentive schedule can be diverted and can have perverse effects. It is very easy to conceive situations in which the high powered incentives of the market do not lead to production and/or innovation.

Aggregation and the structure of equilibria

Competition among price-setting firms: aggregate demand and the distribution of consumers' tastes

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Fundamental problem: Oligopolistic equilibria with price setting firms may fail to exist, since the aggregate demand function faced by a given firm need not give rise to a quasiconcave profit function, J. Roberts and H. Sonnenschein (1977).[1]

Consequence: We have many individual cases or examples of oligopolistic markets (such as the Hotelling model with quadratic transportation costs) in which equilibria happen to exist due to particular assumptions on functional forms. However, the Arrow, Debreu and McKenzie theory of equilibrium existence has no analogue at all in the framework of oligopolistic competition. Therefore, the implications of the Sonnenschein-Debreu-Mantel result for the theory of oligopolistic competition are far more severe than for general equilibrium theory.

Two major differences between the analyses of perfectly competitive and oligopolistic markets are:

i) - The Law of Demand (that overcomes the Sonnenschein, Debreu, Mantel problem) involves *first order* changes of aggregate demand induced by a price change (monotonicity);
   - The quasiconcavity of the profit function is more subtle, since it relies on *second order* derivatives of aggregate demand.

ii) - It is possible (although not fully satisfactory) to derive the Law of Demand from an assumption on the distribution of *income* or expenditures, W. Hildenbrand (1983).
   - On the other hand, assumptions on the distribution of *consumers' tastes* themselves are crucial for the quasiconcavity of the profit function of a firm and hence for the existence of an equilibrium. To formulate such assumptions an algebraic structure on the space of consumers' tastes is needed. In particular, in order to formulate

[1] This problem arises independently of whether quantities or prices are the strategic variables. However, if we would consider Cournot- rather than Bertrand-competition, we would also have to deal with the difficulty of taking a selection out of the Walras-correspondence of the associated exchange economy in order to describe the aggregate demand function with respect to which firms maximize profits.

concavity assumptions, one is led to ask the question: *What is the mean of two tastes ?*

The existing literature does not provide a fully satisfactory answer to this question. For the sake of simplicity the following remarks refer to the particular framework of an indivisible differentiated product of which a consumer buys at most one unit.

In an important article A. Caplin and B. Nalebuff (1991) assume:
Preferences have a utility representation that is linear in the parameters $\alpha = (\alpha_1, \cdots, \alpha_n)$:

$$U(\alpha, \chi, z) = \sum_{k=1}^{n} a_k t_k(\chi) + g(z)t_{n+1} + t_{n+2}(\chi),$$

where $\chi = (\chi_1 \cdots \chi_n)$ describes the characteristics of a brand, the t_k's are real-valued functions of these characteristics, and where z denotes the amount of the numéraire good.

Furthermore it is required that the joint density of the coefficients α_k in the utility function satisfies an appropriate concavity assumption.

Advantages:

- presents a valuable tool to prove equilibrium existence in various individual examples
- provides a unified view on a large part of the existing literature

Disadvantage:

- Making assumptions on the distribution of the parameters in some special utility representation sidesteps an important conceptual issue: What is the algebraic structure of the space of consumers' tastes ?

Other example: Linear Random Utility Model of discrete choice, S. Anderson, A. de Palma, and J. Thisse (1992). The virtue of the model is that it works quite smoothly. However, the linear structure in the context of this stochastic model appears to be essentially motivated by its convenience. In addition, a clear conceptual foundation is most desirable.

Similar remarks apply to other popular models of product differentiation based on the representative consumer approach or the address approach.

Personal view: The theory of oligopolistic competition needs more than just to encompass as many existing cases as possible within one flexible functional form. It is worthwhile to become specific in order to investigate the algebraic structure underlying the demand aggregation process in detail. Simple polar cases need to be studied! The generality of the Arrow, Debreu and McKenzie theory has been obtained at the end of a long historical development. As long as the structure of the aggregation problem is not better understood it is premature to aim at a general theorem for oligopolistic equilibria.

Example for the specification of an algebraic structure: We consider Bertrand competition for an indivisible good, of which n firms offer one brand each. An individual's willingness to pay for brand j is denoted by $w_j \in \mathbb{R}_{++}$. It is thought of as arising in the following way: First we consider a hypothetical willingness to pay $b \in \mathbb{R}_{++}$ for the 'pure' good independent of all brand specific aspects. Horizontal product differentiation is then introduced by brand specific random perturbations d_j summarizing all these aspects. The perturbations raise or lower any hypothetical willingness to pay b. Then we can write an actual willingness to pay as $w_j = b \diamond d_j$, where the symbol $\diamond$ stands for a not yet specified algebraic operation. The joint distribution of $(b, d_1, \cdots, d_n)$ and the mapping $(b, d_j) \longmapsto w_j = b \diamond d_j$ give rise to a joint distribution of the variables $(w_1, \cdots, w_n)$.

As pointed out above, we need an algebraic structure on the space of willingnesses to pay $(w_1, \cdots, w_n) \in \mathbb{R}^n_{++}$ in order to express the appropriate concavity assumption (logconcavity) of their density. Since concavity of a function relates the value of the mean of two points to the mean of their values, the algebraic structure to be considered is that of a mean. Moreover, such a mean, in the following denoted by $\circ$, has to fit to the operation $\diamond$. More specifically, we assume for all b and d_j:

$$b \circ (b \diamond^2 d_j) \;=\; b \diamond d_j.$$

That is to say, if, starting from some willingness to pay b one reaches another willingness to pay by applying a certain perturbation d_j twice, the mean of the first and the final willingness to pay is obtained by applying the perturbation d_j once.

Under some additional, more or less technical conditions one can show, E. Dierker and K. Podczeck (1993), that this implies:

- The operation $\diamond$ is uniquely determined and satisfies $\log(b \diamond d_j) = \log b + \log d_j$;
- The mean $\circ$ is the *geometric mean.*

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Some comments on the use of general equilibrium theory in aggregate analysis

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When I teach the second course in University of Minnesota's first year aggregate theory sequence, I use the abstract language in the Debreu (1954) "Valuation Equilibrium and Pareto Optimum" paper. Rules in this course are that

- First, the commodity space S and set of people *types* I are specified. The commodity space must be a linear space, and in the course, a normed linear space. I deal only with a finite number of types, with $\lambda_i > 0$ denoting the measure of each type $i \in I$.
- Second, for each person type the consumption possibility set $X_i \subset S$ and the utility function $U_i : X_i \rightarrow \mathbb{R}$ defining that type's preference ordering are specified. The consumption possibility sets are convex and the utility functions are both continuous and concave.
- Third, the aggregate technology set $Y \subset S$ is specified. This set must be a convex cone.

In this framework, an allocation $\{\{x_i\}, y\}$ is feasible if $x_i \in X_i$ for all $i \in I, y \in Y$ and

$$\sum_i \lambda_i x_i = y.$$

With these assumptions, the first and second welfare theorems in Debreu (1954) apply.

I do not apologize for making these restricted assumptions, since they are satisfied in nearly all applications. Given the existence of a randomizing mechanism and expected utility maximization, the assumptions on preferences are necessarily satisfied. Indeed, prohibiting randomization is an arbitrary restriction on trades. In requiring the aggregate production possibility set to be a convex cone, I am following McKenzie. I deal only with economies in which production units are small relative to the size of the economy. When these units are not small relative to the size of the economy, the no market power assumption is not even approximately satisfied.

With this class of economies there are no endowments. Sometimes I use an alternative representation which has endowments. In static situations, the endowments are the factors of production, and payments for their use are income. The connection with national income and product accounting is then immediate. In dynamic situations with capital accumulation possibilities this connection is lost, however. The capital stocks at future dates are not endowments. Here I use the representation without endowments.

One general equilibrium convention that I do not follow is that the outputs of the household sector and inputs to the business sector are negative, a convention which results in positive prices. In practice I have found that this is not a good convention. When following, or trying to follow, this convention, both students and instructor make too many errors. What works in practice is to keep the quantities positive except, of course, for the intermediate goods.

A desirable feature of this framework with people and production units being small relative to the economy is that, with some restrictions on the commodity space S, there is no market power.[1] A more important reason for having measures of people types rather than one person of each type, as in Debreu (1954), is that the competitive equilibria for replicas of economies with a finite number of people and with the aggregate production possibility set not convex are not the same. I will show this via an economic example with only one type. The example is not contrived. It arose in an applied analysis. The key feature of the example is an indivisibility in the consumption possibility set.

I permit lotteries to be used to allocate commodities to households.[2] Given the technology to randomize, the utility functions must be defined over random allocations. In my first year aggregate theory course, and in these notes, I restrict attention to expected

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[1] One case for which the no market power assumption is not reasonable is for the Spence, Dixit and Stiglitz environment that has an infinity of goods with no arbitrary close substitute. In the Mas-Colell monopolistic competition world, the set of commodities is infinite but compact. As he has shown, the competitive equilibrium with its no market power assumption is reasonable in such environments.

[2] Prescott and Townsend (1984a, 1984b) have lotteries in competitive analyses with private information. In such worlds, lotteries do more than convexify preferences. Cole (1989) has pointed out that even for a private information economy with convex preferences and technologies absent lotteries, the introduction of lotteries may result in Pareto superior equilibrium outcomes. In the worlds considered in these notes, however, the role played by lotteries is to convexify the economies.

utility maximization.[3]

A restriction on allocations that I impose is that people of the same type consume the same commodity vector. This is not a real restriction, given that there are lotteries. If there is an equilibrium with fractions of some type choosing different commodity points, there is an observationally equivalent equilibrium, with all people of that type choosing the same comodity point. This observational equivalence is for statistics that are anonymous, which almost always is a property of the statistics of interest.

Arrow, Debreu and McKenzie equilibria and lottery equilibria are equivalent.[4] I use the lottery equilibrium language rather than the Arrow, Debreu and McKenzie language because using the lottery language is easier. There is no need to keep track of names or to index allocations by some event that doesn't alter utility functions, consumption possibility sets, technology sets, or policy constraints.

Dealing with a finite number of types is not a significant restriction. When it comes to determining what will happen in the model economy given the policy arrangement, computational considerations typically lead us to use economies with a finite number of types. Going to the limit, that is going either to a continuum of people types or to a continuum of commodities is not hard, but it cannot be covered in the first year of the University of Minnesota graduate program. Competitive analyses of economies with both a continuum of types and commodities, I suspect, raise some interesting mathematical problems. In these comments I permit the commodity space to have infinite dimension.

Dealing with a finite number of types with all those of the same type making the same choice permits us to use all the theory laid out so well by Debreu in *The Theory of Value*. The existence arguments developed 40 years ago can be used. The two welfare theorems apply and are often useful in developing an algorithm to compute an equilibrium and sometimes in establishing the uniqueness of the equilibrium.

I turn now to some examples of competitive analyses that have used lotteries. Throughout these examples B (A) denotes the Borel sigma algebra of compact metric space A.

[3] The Kreps and Portes type preferences have been used in applied analyses, e.g. by Epstein and Zin and by Polemarchakis and Selden. These are the only applied general equilibrium analyses with uncertainty that I know of with people who were not expected utility maximizers.

[4] Ken Arrow pointed this out to me in 1979.

A labor indivisibility economy

Economic environment

There is one person type and the measure of this type is one. These people have a von Neuman-Morgenstern utility function

$$u(c) - v(h)$$

where consumption $c \geq 0$ and hours of market labor $h \in \{0, 1\}$. Function u is continuous, increasing, and concave. Function v is increasing with $v(0) = 0$.

Each person has $k_0 > 0$ units of capital. The aggregate production function $f(k, n)$ is neoclassical with constant returns-to-scale. Here n is the measure of people who work h hours. There is a randomization device.

Economy representation

- The commodity space is $\mathbb{R}^3$, where x_1 is consumption, x_2 is capital services, and from the prospective of a household x_3 is the probability $h = 1$, and from the prospective of the firm x_3 is the measure of people working h hours.
- The consumption possibility set is

$$X = \{x \in S : x \geq 0, x_2 \leq k, 0 \leq x_3 \leq 1\}$$

and the utility function is

$$U(x) = u(x_1) - x_3 v(1).$$

- The aggregate production possibility set is

$$Y = \{y \in S : y_1 \leq f(y_2, y_3), y_2, y_3 \geq 0\}.$$

The important result is that the utility function is linear in the probability of working.

Labor indivisibility and business cycles

Hansen (1985) introduced Rogerson's labor indivisibility into the growth model and found that the difference between one type and many of the same type is dramatic with this labor indivisibility. In Hansen's world the utility function is

$$U(x) = E\{\sum_t \beta^t [u(x_{1t}) - x_{3t} v(1)]\}.$$

If the probabilities of working x_{3t} and x_{3t+1} are not 0 or 1, locally these two commodities are perfect substitutes. In real business cycle theory, intertemporal substitutability of leisure is key. Only if people's willingness to intertemporally substitute leisure is high do reasonably sized public finance, technology and terms-of-trade shocks induce business cycle fluctuations of the magnitude and nature observed. In public finance applications this willingness to substitute is important as well. Estimates of welfare consequences are sensitive to this parameter.

This indivisibility assumption matches with the observation that most of the variation in total market hours are the result of the number working in a given week and not in the length of the average workweek of those who do work. An obvious question to ask is why restrict h to be either 0 or 1 ? Kydland and Prescott (1991) and Hornstein and Prescott (0000) permit h to vary continously and find that in equilibrium h does not vary.

An economy with an endogenous workweek

Economic environment

The two principal differences between this environment and the labor indivisibility A environment are (i) h, the fraction of the week that an individual works, is constrained to the interval $[0,1]$ rather than to the set $\{0,1\}$ and (ii) the output of an individual using k units of capital and working h hours is

$$hk^{\theta} \text{ where } 0 < \theta < 1.$$

The utility function is $u(c, 1-h)$, where c is consumption and $1-h$ is the fraction of productive time allocated to nonmarket activities.

Economy representation

- The commodity space is $S = M(C \times H \times K)$ where $C = [0, c_{\max}]$, $H = [0,1]$, and $K = [0, k_{\max}]$.
- The consumption possibility set is

$$X = \{x \in S_+ : \int dx = 1,\ x(C \times H \times (k_0, k_{\max}]) = 0\}$$

  and the utility function is

$$U(x) = \int u dx.$$

- The aggregate production possibility set is

$Y = \{y \in S : \exists$ measure a on $\mathcal{B}(H \times K)$ for which

(i) $\int k da \leq \int k dy$;

(ii) $\int c dy \leq \int h k^\theta da$;

(iii) $a(B \times K) \leq y(C \times B \times K)$ for all $B \in \mathcal{B}(H)\}$.

Constraint (i) in the definition of Y is that capital assigned to plants be less than or equal to the total capital input. Constraint (ii) is that the consumption good output be less than or equal to the sum of the outputs of the plants. Constraints (iii) are that the measure of people who work $h \in B$ hours be equal to the number of units operated $h \in B$ hours.

A finding is that the equilibrium of this economy behaves as if there were an institutionally determined workweek for standard utility and production functions. If there is an aggregate technology or public finance shock, what changes is the fraction of people working, not the length of the workweek, except in the case in which the entire population is working.

This abstraction does endogenize the workweek, but one feature of reality with which it is at variance is the observation that within a given week not all people and machines work the same number of hours: Some capital, e.g. some offices in office buildings, some machines in factories and even some factories, are not used at all in period. Some capital is operated a regular workweek, and some capital is operated longer than a regular workweek. These observations suggest constructing a model in which not all capital is operating the same number of hours. This has recently been done in Cooley, Hansen and Prescott (1993).

A economy with idle capital

Economic environment

There is heterogeneity at the plant or production unit level. Capital must be allocated to plants to observing a plant specific idiosyncratic shock z. These shocks are identically distributed across plants and their joint distribution is such that the fraction of plants with shocks in set $A \in \mathcal{B}(Z)$ is $\pi(A)$, where $Z = [z_{\min}, z_{\max}]$. More specifically, the plant technology is

$$zhk^\theta g(n).$$

Here $h \in H = \{0, h_{\text{reg}}, h_{\text{over}}\}$ is the number of hours a plant is operated in a given week. A plant type is indexed by $(h,k,n,z) \in H \times K \times N \times Z$ where $K = [0, k_{\max}]$ and $N = [0, n_{\max}]$. We assume that $g'(\infty) = 0$ and that $0 < \theta < 1$. This insures a finite optimal plant size. Further, we assume that there is a range of increasing returns so that the optimal plant size will exist. Each person is endowed with $k_0 > 0$ units of capital. Preferences are the same as in the previous example.

Economy representation

- The commodity space is $S = M(C \times H \times K)$, where $C = [0, c_{\max}]$.
- The consumption possibility set is

$$X = \{x \in S_+ : \int dx = 1, x(C \times H \times (k_0, k_{\max}]) = 0\}.$$

The utility function is

$$U(x) = \int u(c, 1-h) dx.$$

- The aggregate production possibility set is
  $Y = \{y \in S : \exists$ measure on a $\mathcal{B}(H \times K \times N \times Z)$ for which
  (i) $\int c dy - \int zhk^\theta g(n) da \leq 0$;

  (ii) $\int k da - \int k dy \leq 0$;

  (iii) $a(B \times K \times N \times Z) - y(C \times B \times K) \leq 0$ for all $B \in \mathcal{B}(H)$;

  (iv) $a(H \times B_k \times N \times B_z) = \pi(B_z) a(H \times B_k \times N \times Z)$ for all $B_K \in \mathcal{B}(K)$ for all $B_z \in \mathcal{B}(Z)\}$.

Condition (i) in the definition of Y is the restriction that production of the output good be at least as large as consumption of the output good. Condition (ii) is that the allocation of capital to plants cannot be larger than the quantity available for allocation. Condition (iii) is that measure of people assigned to plants with workweek h not larger than the measure of people working h hour workweeks. Condition (iv) is the requirement that the allocation of capital to a plant must be made prior to observing that plant's shock z.

Equilibrium is characterizd by there being some measure of plants all with the same capital. Those with a z realization below some number are idle, that is, $h = 0$; those with a z above some other number are operated h_{over} hours, and those with z in between these values are operated h_{reg} hours. When this structure is introduced into a dynamic model with capital accumulation, with persistent Markov aggregate technology shocks that alter the measures π_t and with people who maximize discounted expected utility, the nature of the equilibrium process is that the fractions of plants operated $h \in H$ hours varies.

Indivisibility in the underlying consumption possibility set is a key feature of all these examples. Another important example of indivisibilities in the consumption possibility is the fact that people cannot be in two places at once. Indeed, this was the indivisibility that first concerned Rogerson (1984). Prescott and Rios-Rull (1992) use the tools reviewed in these notes to model the movement of people between locations, which they call *islands*. Their environment has islands with information sets differing across islands and with people who can and in equilibrium do move between islands. Their model can be viewed as a competitive equilibrium with search.

Indivisibilities are key in other situations too. Recently Harold Cole and I realized that this tools can be applied to the study of clubs and to matching through the use of lotteries. I turn now to the basic club environment.

A competitive analysis with clubs

Economic environment

People of type i have utility function $u_i(c, g)$, where c is a vector of individual consumption goods and g is a vector of club or local public goods. We restrict c and g to belong to closed and bounded subsets of a finite dimensional space. Further (c, g) must belong to B_i, a subset of $C \times G$. Vector g are club goods in the sense that all members of a club must consume the same g. The input vector c required to produce club good vector g is

$$c \geq f(g, n)$$

where n is the number of club members belong to finite set N.

Economy representation

- The commodity space is the space of signed measures on $\mathcal{B}(C \times G)$. That is, $S = M(C \times G)$. The set of agents is the finite set I with $\lambda_i > 0$ being the measure of type i.
- The consumption possibility sets are

$$X_i = \{x \in S_+ : \int dx = 1, x(B_i) = 1\}.$$

The utility function or a type i is

$$U_i(x) = \int u_i(c,g)dx$$

for $x \in X_i$.

- The aggregate production possibility set is
  $Y = \{y \in S_+ : \exists$ measures $a \in M(G \times N)$ such that
  (i) $\int cdy \leq \int f(g,n)da$;
  (ii) $y(C \times B) = \int_{B \times N} nda$ for all $B \in \mathcal{B}(G)\}$.

In the definition of Y, a is the measure of clubs as indexed by the club's level of g and by the number of club members n. Constraint (ii) is that the number of people assigned to clubs with club good value g be equal to the number of people whose club good consumption level is g.

An obvious extension of this abstraction is for n to be a vector denoting the number of each type in the club. The following matching problem, drawn from Cole, Mailath and Postlewaite (1992), is an example of this extension.

A competitive analysis with matching

Economic environment

People are indexed by $S \in \{f, m\}$ and by endowments $e \in E$, a finite subset of $\mathbb{R}_{++}$. The measure of types are λ_{es}. People have preferences over the private good $c \in [-e, c_{\max}] = C$ and over a club good $g \in (0, g_{\max}]$. Their utility function is

$$u(e+c) + v(g)$$

where u is strictly increasing, strictly concave, and bounded and where v is strictly increasing and strictly concave. Further, $v'(0)$ is finite and $v(0) = 0$. Each unit of the club good produced requires two units of the endowment, while each unit of the private consumption good produced requires one unit of the endowment good.

Economy representation

- The commodity space is $S = \mathbb{R} \times M(G) \times M(G)$ with element $x = (x_0, x_f, x_m)$.
- The consumption possibility set of a type (e,s) is $X_{es} = \{x \in S_+ : x_0 + e \geq 0, x_t(G) = 0$ for $t \neq s, x_s(G) \leq 1\}$. The utility function is

$$\int u(e+c,g)dx.$$

- The aggregate production possibility set is $Y = \{y \in S_+ : \exists$ measure a on $\mathcal{B}(G)$ for which

$$\text{(i) } 2\int g da + y_0 \leq 0 \text{ and (ii) } y_f = y_m\}.$$

The competitive equilibrium exists and is generically unique. Only when

$$\sum_{e>e^*} \lambda_{ef} = \sum_{e>e^*} \lambda_{em}$$

for some e^* is there a non uniqueness. Cole and Prescott (1993) have developed an algorithm for computing the equilibrium.

Two final comments

The equivalence of lottery and gambling equilibria

An Arrow-Debreu-McKenzie competitive equilibrium can be supported as a sequence of market equilibrium with date t commodities and Arrow securities only. Similarly, there is an alternative way to support lottery equilibrium allocations without lotteries on the commodities. The alternative is to have gambles on units of value prior to operation of a no lottery market system. In the class of private information environments considered by Prescott and Townsend (1984a, 1984b), however, this equivalence does not hold.

Contracting technology and the commodity vector

The commodity vector is in a sense part of the specification of the technology. If some future events become perfectly predictable and people are expected utility maximizers, the commodity space and the economy have changed. For the labor indivisibility example, absent a randomizing mechanism, the no lottery, competitive equilibrium is a Pareto optimum. If $u(0) = -\infty$, in equilibrium all work. If a randomizing device is introduced, this allocation may not be Pareto optimal. It will not be Pareto optimal if there exists an $n < 1$ for which $u'(f(k,n)) = v(1)$. In this case the allocation with people working with probability n and consuming $f(k,n)$ with certainty is the anonymous Pareto optimum.

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Money and macroeconomics

General equilibrium

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My talk today wil be on the type of microeconomic foundations we need for macroeconomics, and I shall strongly argue in favor of an approach which unfortunately seems to have fallen into disrepute in the U.S., that of General Equilibrium models with non-clearing markets. Because this field has been widely misrepresented, rather than making a precise description, which the interested reader can find for example in Benassy (1990, 1993), Drèze (1991), I will try to make an attempt at perspective and convey briefly the spirit and scope of this line of research.

Actually in order to understand why this approach and other competing ones emerged, one should go back to the situation of about twenty years ago when these schools of thought made their beginnings. At the time anybody interested in both microeconomics and macroeconomics was bound to suffer from acute schizophrenia.

Indeed microeconomics was split between, on the one hand, the highly rigorous Walrasian General Equilibrium models as formalized notably by Kenneth Arrow, Gérard Debreu, Lionel McKenzie, and, on the other hand, imperfect competition models in a partial equilibrium framework (Though a beginning of a synthesis had emerged with Takashi Negishi's 1961 seminal article).

As for macroeconomics it was based on neither Walrasian nor imperfect competition models but, following the IS-LM tradition laid down more than forty years before by John Hicks (1937), emphasized non clearing markets and price rigidities. Although this model, unlike microeconomic ones, allowed to discuss such a major problem as unemployment, many macroeconomists were conscious (and unhappy) of the fact that the models they used had no serious foundations. This is when the split occurred.

One group of people decided that, since the one theory that had clear foundations then was the Walrasian model, they would throw away anything related to non-clearing markets and imperfect competition, and reconstruct Walrasian macroeconomics, leading to what is now known as the "new-classical" school.

Another group of economists thought in the contrary that, because the Walrasian model made very extreme (and unrealistic) assumptions, limiting oneself to general market clearing situations would be somehow throwing the baby with the bathwater. They therefore engaged in the task of generalizing General Equilibrium "Arrow, Debreu and McKenzie" theory to encompass non-clearing markets and imperfect competition, so that the resulting theory would be a synthesis of the Walrasian, Keynesian and imperfect competition models.

Because this group was made of serious people, they started with the "Arrow, Debreu and McKenzie" model and extended it principally in two directions.

- First by allowing markets not to clear by prices, showing how quantity signals would form in addition to price signals, and letting individual agents react optimally to these quantity signals as well as to price signals.
- Secondly by endogenizing prices through explicit maximization by private agents, thus solving Arrow's (1959) famous problem of how to make explicit the workings of the "invisible hand."

As a result, one obtains generalizations of the Walrasian General Equilibrium concept which can be, somewhat arbitrarily, classified into two main categories:

- General Equilibrium models with a priori given price rigidities. This includes of course fixprice equilibria, but is actually quite more general. For example in his famous and seminal 1975 article Jacques Drèze studied an equilibrium concept with prices flexible between bounds or indexed on each other, which thus leaves large scope for price flexibility.
- General Equilibrium models with fully explicit price makers in a General Equilibrium framework. It turns out that these models allow to solve a problem which had been left in the dark before, i.e. how to define rigorously an objective demand curve and an equilibrium concept in a model where agents explicitly use prices as strategies.

In view of the above considerations we should note in passing that the (unfortunately common) denomination of this field as "fixprice economics" is particularly misleading, and certainly contributed to develop the "communication gap" alluded to above.

Now these microfoundations were not developed for the pure sake of doing abstract theory, but have been quickly applied to a number of domains in macroeconomics, including unemployment, inflation, foreign trade, growth, business cycles, and of course the problems of planned economies. This approach has been particularly in the forefront in renewing the debate on unemployment (starting with Robert Barro and Herschel Grossman's 1971 article) and in introducing imperfect competition into macroeconomics; this last development has been largely followed since.

Progress has occured on the empirical side as well, and a number of applied macroeconometric models have been developed. I would like notably to draw attention on the European unemployment program which found its roots here in Louvain-La-Neuve, and successfully mixed non-clearing markets and imperfect competition in studying empirically the nature of unemployment in various european countries.

Now what kind of messages can we draw from these theories for a General Equilibrium audience such as today's ?

- The first message which emerges from the non-clearing markets literature is that it is actually fundamental to have a General Equilibrium approach to macroeconomics. Too many people nowadays study the problem of unemployment from a purely partial equilibrium point of view, concentrating only on the functioning of the labor markets. This may give a completely misleading view about the causes of unemployment and appropriate government policies. Indeed one of the important insights of the theory is that disequilibrium on one market can be entirely caused by the malfunctioning of another market. For example one can build a very simple model where unemployment is caused by the rigidity of goods prices inspite of fully flexible wages. In such a case a full General Equilibrium analysis is evidently called for.
- A second message, which concerns both microeconomists and macroeconomists, is that if you want to do rigorous "flexprice" analysis without an auctioneer, you should use the theories just described. Why this apparent paradox ? Because if an agent sets some prices, or bargains about them, he wants to be able to compute the actual quantity consequences of *any* price choice, and this theory precisely allows to derive such consequences, whereas traditional theories allow such a derivation for Walrasian prices only.

Now that I have talked about achievements of the field, I must talk also a bit about directions of research and open problems. A first natural direction of research would be to devote more time studying the dynamic properties of these models, notably in stochastic settings. "Real business cycle" theorists and others have submitted Walrasian models to such a scrutiny, and it is likely that the models we described could generate richer and especially more realistic dynamics.

An open problem of this line of research is also that of generating nominal rigidities in a satisfactory manner. Indeed I indicated above that there were both models with a priori given rigidities and models with fully endogenized prices. It turns out that these last models can reproduce so far what is called "real" rigidities (for example, rigidity of the real wage), but not "nominal" ones (for example nominal price or wage sluggishness). Though the currently most fashionable way to model these nominal rigidities in the U.S. seems to be the so called "menu costs," I believe a deeper and more fruitful way to approach the problem would be to integrate the non-Walrasian approaches developed in the works I outlined with some imperfect information insights developed by some prominent contributors to the "new-classical" school. This would lead to the much needed synthesis between the two lines of thought which parted twenty years ago.

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Notes on the optimum quantity of money

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I here try to explain in simple terms a fundamental difficulty with Friedman's theory of the optimal quantity of money, the difficulty being that perfect self-insurance is not achievable. Friedman proposed that money should earn interest at a real rate equal to individuals' pure rate of time preference. He asserted that the resulting equilibrium would be optimal. Perhaps exaggerating Friedman's meaning, I interpret optimal as meaning that the equilibrium would be Pareto optimal in the same sense that an Arrow, Debreu and McKenzie equilibrium with complete markets would be Pareto optimal. Pareto optimality implies full insurance, and in Friedman's model, full insurance would be provided by self-insurance through the holding of money or other assets. The payment of adequate interest on money would encourage people to hold high levels of assets for self-insurance. However, mortal people would not use assets so as to insure themselves perfectly. To have Pareto optimality, we are driven to think in terms of a model with immortal consumers, as Friedman proposed. Even immortal consumers would have to accumulate assets in the early part of their lives, and so could not be perfectly insuring during that period. We therefore consider stationary equilibria, for in such equilibria adequate assets could already have been accumulated.

A trick that makes it easy to analyze stationary equilibria is to assume that there are a continuum of individuals, each faced with independent random variation. So consider a continuum of individuals, indexed by $i \in [0,1]$. Let there be only one commodity and let time be discrete. Each consumer has a utility function for a random consumption stream $(c_0, c_1, c_2, \cdots)$, defined by

$$E \sum_{t=-T}^{\infty} (1+\rho)^{-t} u(c_t),$$

where $0 < \rho < 1$ and u is increasing and strictly concave. The pure rate of time preference is ρ. The endowment of consumer i in period t is a random variable $\omega_{it} \in [\underline{\omega}, \overline{\omega}]$, where $\underline{\omega} > 0$ and the ω_{it} are independently and identically distributed, for all i and t. The money holdings of consumer i at the end of period t are M_{it}. Money earns interest at rate $r \geq 0$, and the interest is paid by a lump sum tax, τ, paid by every consumer in every period. The total money supply is $M > 0$, so that $\tau = rM$. The evolution of M_{it} obeys the equation

$$M_{i,t+1} = (1+r)M_{it} + \omega_{i,t+1} - c_{i,t+1} - \tau. \tag{1}$$

Each consumer i solves the problem

$$\begin{aligned} &\max E \sum_{t=-T}^{\infty} (1+\rho)^{-t} u(c_t) \\ &\text{s.t. } M_{it} \geq 0, \text{ all } t, \\ &M_{i,-T-1} \geq 0 \text{ being given.} \end{aligned}$$

Since $\tau = rM$, equation (1) may be rewritten as

$$(M_{i,t+1} - M) = (1+r)(M_{it} - M) + \omega_{i,t+1} - c_{i,t+1}$$

or, letting $B_{it} = M_{it} - M$, as

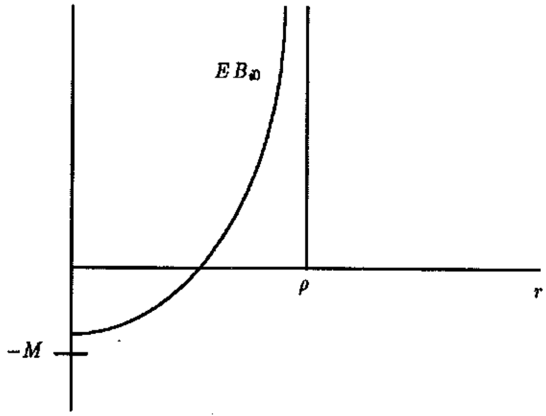
$$B_{i,t+1} = (1+r)B_{it} + \omega_{i,t+1} - c_{i,t+1}.$$

The consumer's maximization problem may be rewritten as

$$\begin{aligned} &\max \sum_{t=-T}^{\infty} (1+\rho)^{-t} u(c_{it}) \\ &\text{s.t. } B_{it} \geq -M, \text{ all } t, \\ &B_{i,-T-1} \geq -M \text{ being given.} \end{aligned} \tag{2}$$

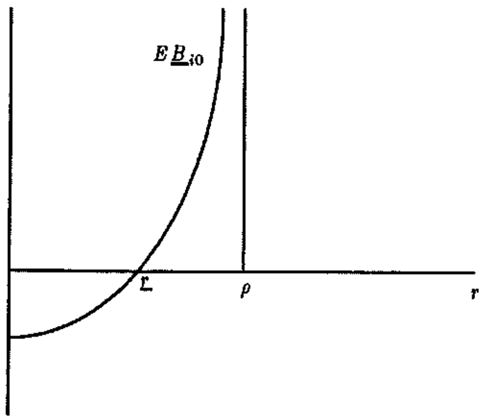
For each $r \in [0, \rho)$, problem (23) has a solution, call it $(c_{it}(M,r,T), B_{it}(M,r,T))$. As T goes to infinity, $(c_{it}(M,r,T), B_{it}(M,r,T))$ converges to a limit $(c_{it}(M,r), B_{it}(M,r))$, where each $c_{it}(M,r)$ and $B_{it}(M,r)$ depend only on current and past realizations of the endowments, ω_{is}. A stationary equilibrium is defined by an M and r such that $EB_{i0}(M,r) = 0$. It then follows that $Ec_{i0}(M,r) = E\omega_{i,0}$. ($EB_{i0}$ and Ec_{i0} may be thought of as equivalent to $\int_0^1 B_{i0}di$ and $\int_0^1 c_{i0}di$.)

The next figure shows $EB_{i0}(M,r)$ as a function of r.



The function $EB_{i0}(M, r)$ is well-defined and continuous, for $0 \leq r < \rho$.

As M increases, $B_{i0}(M, r)$ decreases. But for $M > \overline{\omega}/r, B_{i0}(M, r) = B_{i0}(\overline{\omega}/r, r)$, since consumers must be able to pay the interest on the debt they incur. Let $\underline{B}_{i0}(r) = B_{i0}(\overline{\omega}/r, r)$, so that $\underline{B}_{i0}(r) \leq B_{i0}(M, r)$, for all M, and let $\overline{r}$ be defined by $E\underline{B}_{i0}(\overline{r}) = 0$. Then since $\lim_{r \to \rho} E\underline{B}_{i0}(r) = \infty, \overline{r} < \rho$ and no equilibrium exists with $\overline{r} < r < \rho$, as illustrated in the next figure.



Thus, an optimal equilibrium with r equal to ρ is impossible.

In fact, Perry Mehrling (1993) has recently shown that it may not be optimal to bring r near to $\bar{r}$ by increasing the borrowing limit (or money supply) M, for doing so may increase the probability that a consumer would get stuck deeply in dept and hence suffer a long period of low consumption.

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Some reflections on general equilibrium theory, macroeconomics and the coordination problem

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The years of high theory

When we recall the triumphs of General Equilibrium Theory (GET), we tend to think of the classic papers of the nineteen-fifties and 'sixties, papers that made fundamental contributions to mainstream economics as well as setting a new standard of achievement in terms of rigor and elegance. The proofs of the existence of equilibrium by Arrow and Debreu (1954) and McKenzie (1954), provided a general model of the price system and described the assumptions that are needed in order for the price system to "work." The proofs of the fundamental theorems of welfare economics by Arrow (1951) and Debreu (1951), characterized the efficiency properties of competitive equilibria in a way that makes clear the centrality of the perfect competition paradigm in economics. The extension of these theorems to economies with infinite horizons by Malinvaud (1952) and Cass (1972) made clear the limitations of the finite theory and warned of possible market failures. The ideas of complete markets and Arrow securities (Arrow (1953/1965)) extended the application of the theory to risk sharing and laid the groundwork for later applications in finance (by Black and Scholes (1973), Harrison and Kreps (1978), and others). The characterization of equilibrium in terms of game-theoretic solution concepts such as the core (Debreu and Scarf (1964), Aumann (1964)), the Shapley value (Aumann and Shapley (1974), and so forth, illuminated the institutional settings in which competitive behavior, as characterized by the Walrasian model, was appropriate. The analysis of the uniqueness and stability of equilibrium (Arrow and Hurwicz (1958), Hahn (1958) and Negishi (1958)) determined substantive restrictions on the excess demand functions that were sufficient for the equilibrium to be "well-behaved."

Looking back, it seems that the years from 1950 to 1970 were, to borrow G.L.S. Shackle's phrase, "years of High Theory" as far as GET was concerned. The contribution of GET during that period was fundamental to the progress of Economic Theory as a whole. Can we say that GET occupies such a central role in Economic Theory today ? My own casual observations suggest that it does not. Graduate students who flock to courses on Game Theory are often quite ignorant of GET. In some departments, GET is not even taught as a separate branch of Economic Theory: the most that students get is the brief survey found in Varian. There is even a tendency to regard GET as part of Mathematical Economics, containing the sort of arcane technical knowledge that may appeal to specialists but is not essential for the mainstream theorist.

This view is perhaps understandable, when one considers the sort of work that has captured the imagination of general-equilibrium theorists in the last ten years. Examples include the study of economies with incomplete markets, where the focus has been on the dimensionality of the equilibrium manifold, or the study of existence of equilibrium in economies with infinite-dimensional commodity spaces, or more recently the study of economies with incomplete information, where again the focus has been on the existence of various cooperative solutions. The technical accomplishments in this literature have been impressive, but the average macroeconomist could be forgiven for missing its relevance to his own work. In fact, there is a yawning gulf between the highly formal, abstract analyses of general equilibrium found in the *Journal of Mathematical Economics* and in *Economic Theory*, and the type of macroeconomic theory published in, say, the *Quarterly Journal of Economics*. This is a pity, because it seems to me that both sides lose when GET is reduced to a branch of Mathematical Economics and loses touch with the sort of issues that originally led to its creation. Macroeconomics would be richer if it had a well developed theory of general equilibrium with incomplete markets, incomplete information and imperfect competition to draw on and GET would benefit from the stimulus of real world questions and would once more be the focus of interest from the mainstream of the economics profession.

The coordination problem

General-equilibrium ideas still have a prominent place in the macroeconomic literature, of course. It could hardly be otherwise, since macroeconomics is about general equilibrium. The literature on Overlapping Generations (OLG) models of the kind popularized by Azariadis, Cass, Grandmont, Shell, Wallace, Woodford and others is part of the general equilibrium tradition, for example. General-equilibrium models are also used in the work of the self-styled New Keynesian Economists (Mankiw and Romer (1991)). But these are general-equilibrium models in a rather pale and attenuated sense. They tend to be simple, "aggregative" models with few consumers, few commodities, simple technologies and simple preferences. Somehow they fail to capture the complexity and heterogeneity of the economy that is one of the salient features of the Arrow, Debreu and

McKenzie model. It is not that this sort of research has become unacceptably "low brow" – some of the literature is technically quite sophisticated – the problem is rather that in the pursuit of rigor we have developed models that lack some of the flavor of the older macroeconomic concerns.

This is most clearly seen if we consider what is perhaps the most fruitful application of the ADM framework, namely, the Real Business Cycle (RBC) models developed by Lucas (1975) and Kydland and Prescott (1983) and their followers. The RBC research program treats macroeconomic time series "as if" they were generated by an individual solving an infinite-horizon maximization problem. The rationale for this approach comes from the duality between equilibrium allocations and the solutions of a planning problem, that is, the Fundamental Theorems of Welfare Economics, which is a powerful tool for analyzing the time-series properties of real economies. The theoretical and empirical results of this research program have been impressive.

This approach has limitations, however. Although there are notable exceptions in the more recent literature, the overall emphasis has been on models that exploit the efficiency properties of equilibrium. This makes it very difficult to address certain kinds of questions, concerning market failure, for instance, which were a crucial concern of general-equilibrium theorists like Hicks, Patinkin and Hahn. They lack any concern with what I will call, for want of a better phrase, the Coordination Problem. From the time of Adam Smith (1776) to the time of Keynes (1936) and Hicks (1946), economists were perplexed and preoccupied by the question of how it is that the uncoordinated decisions of many, non-cooperative agents produce an orderly allocation of resources rather than chaos ? The theory of Arrow, Debreu and McKenzie provides one answer, but it is obviously not the answer that Keynes, Hicks, Patinkin or Hahn were looking for. In the nineteen-seventies, the work on Fixed-Price Equilibria by Drèze, Malinvaud, Hahn, Grandmont, Laroque and others, was the most recent attempt to link the modern Walrasian tradition with macroeconomic issues. But that path seems to have been abandoned (*pace* Dr. Bénassy) and research on GET has now reverted to more classical concerns.

It is not at all clear to me that the Arrow-Debreu-McKenzie model is the last word on the Coordination Problem and it seems to me that recent work on information, strategic behavior and incentives has a lot to say about coordination and the possibility of (inefficient) fluctuations in the level of aggregate activity. Furthermore, I think that GET, with its emphasis on the interaction of large numbers of individuals in large numbers of markets is crucial to understanding some aspects of this problem. Partial equilibrium models and extensive-form games will not do the job. So why isn't GET producing useful models of the Coordination Problem ?

The problem, I think, is that we have fallen into the trap of treating Arrow and Debreu (1954) as if it *defined* the scope and method of GET, or at least defined what a paper on GET should look like, rather than letting the economic problem determine the appropriate tools. If GET is to be part of the mainstream again, the Walrasian tradition, which regards the economy as composed of a complex set of markets, interacting in highly dynamic and sometimes chaotic ways, needs to be reunited with modern theories of strategic behavior and information that provide the microfoundations for understanding the sources of market failure. If we genuinely want to solve economic problems and if we think macroeconomic modeling of the economy matters, then we should be open to new ways of modeling the economy. In particular, we should be willing to incorporate innovations suggested by developments in Game Theory, Mechanism Design and the Economics of Information.

Future research

If we want to bring the Coordination Problem back to center stage in macroeconomics, what are the problems that need to be looked at ? I will mention two that I find interesting.

The first is the work on coordination problems (in the narrow, modern sense of "coordination games"). Cooper and John (1988) have given a nice survey of the applications of these ideas to macroeconomics. The general flavor of the literature is captured by a game in which every agent chooses the intensity with which he pursues some activity and his payoff depends on his intensity and the average of the other players' intensity. If a player's best reponse is an increasing function of the average intensity of the other players, there will exist multiple equilibria which can be ranked in terms of the level of activity of all the players. Sometimes, equilibria can be Pareto-ranked as well.

As a metephor for market failure and the possibility of "low level" equilibria, this work is highly suggestive. But it obviously falls short of providing the sort of comprehensive framework for the analysis of the economy that GET has traditionally sought. The models are very simple and highly aggregative in character and rarely allow a convincing role for prices. The institutional structure is often very crude. But these models can be extended and interesting GE results can be produced if we are prepared to deviate a little from the Arrow, Debreu and McKenzie paradigm. Perhaps we should try.

The more important limitation of the literature on coordination failure is more fundamental and stems from its reliance on multiple equilibria. From a purely theoretical point of view, multiple equilibria seem to be a robust phenomenon, but what are we to make of it ? Obviously, a model characterized by multiple equilibria, whether locally unique or indeterminate, has weak predictive power. Moreover, the suggestion which is sometimes made that economic policy may take the form of moving the economy from one equilibrium to another is suggestive but, in the absence of a convincing dynamic account of the transition, it remains an article of faith. Most worrying, however, is the fact that there is no obvious empirical counterpart to the equilibrium switching found in theoretical models. By and large, it seems to be the case that "Natura non saltum fecit." It is true that multiple equilibria can be used to construct single equilibria in which the economy moves in ways that are not possible in Walrasian equilibria – sunspot equilibria are the obvious example – but here we have not so much a coordination failure as an excess of coordination, in the sense that precise coincidences of actions are required to support the self-fulfilling nature of beliefs. (This is characteristic of all equilibria, to some extent, of course).

Although the coordination failure literature undoubtedly opens the door to all sorts of interesting and suggestive possibilities, I am coming to the view it might be more realistic to focus on small movements of locally unique equilibria in response to exogenous shocks. Even if the economy moves continuously in response to shifts in underlying parameters, there is still an important coordination problem. Since economic agents do not know exactly what other agents are doing, and certainly do not know what the state of nature is, they must be uncertain about the optimality of their own actions. It is only over time that they may converge on a course of action which they are satisfied is in their long run interests.

This brings me to a second interesting line of research, which has focused on the role of informational externalities. Caplin and Leahy (1991, 1992) and Chamley and Gale (1992) have investigated these ideas in a specifically macroeconomic context, but they are related to a broader research program on herding and informational cascades, including the work by Banerjee (1992), Bikchandani, Hirshleifer and Welch (1992), Scharfstein and Stein (1990) and Gul and Lundholm (1992).

It is characteristic of these models that they are "small," highly stylized models of very simple economic settings that are scarcely recognizable as models of entire economies. Yet these models are attempting to focus on problems of coordination and information transmission that I think are central to macroeconomics. At some level, information must be crucial to understanding the problem of coordinating economic activity. What is needed is to extend these insights to models that have some claim to represent general equilibrium of the economy as a whole. Without having done the research, it is difficult to say what a good model "should" look like, but I think the following elements are important. First, there must be incomplete information. Second, there must be some kind of externality or strategic complementarity. Third, there must be a changing environment, perhaps most easily modeled by exogenous shocks that shift the optimal allocation. Finally, there must be a cost of acquiring information, either a direct cost of changing actions or an opportunity cost of experimentation (cf. Bolton and Harris (1993)).

Here is a matchbox size example. Assume that a large number of agents have to choose locations in some finite set. An agent's payoff depends both on his own choice, on an unknown parameter or state of nature, and on the number of agents who choose the same location (or, perhaps, a similar location). There is a fixed cost of changing location and agents are symmetric at least to the extend that they all have the same most preferred location. Agents try to maximize expected, discounted payoffs.

If there were complete information, there would be a unique location that maximizes everyone's payoff. Call this the "fully coordinated" equilibrium. The effect of the exogenous shocks is to shift the "fully coordinated" equilibrium, that is, the location x^* at which the payoffs of all agents are maximized when all agents choose the same location. Every agent's payoff is common knowledge, but there is noise, so it is impossible to tell exactly where the "fully coordinated" equilibrium is. Suppose that we start with all agents at the optimal location x^*. Then a shock hits the system, changing the optimum to x^{**}, say. Agents do not know where the new best location is. To find out, they need to experiment. There is a free rider problem, however, as most agents may prefer to stay where they are, rather than experiment and run the risk of making bad mistakes. Some agents may even choose to "sit out the game" as long as the uncertainty about the correct strategy is high. So there is good reason to believe that the level of activity may be low, during the transition to a new "fully coordinated" equilibrium and that the transition may be inefficiently slow.

There will also be a chance that the economy may get stuck in an inefficient equilibrium, as students of the multi-armed bandit literature will immediately recognize. The fact that there are strategic complementarities or externalities will, of course, exacerbate these problems. Note that even without complementarities modeled in the payoff functions, there is an informational externality that will have similar effects.

This thumbnail sketch is only intended to convey the flavor of the problem. Much of the potential lies in the imaginative elaboration of these ideas in a fully specified general-equilibrium framework. The idea is not to extend results to a more general setting simply for the sake of having a more general model. Rather, it is to construct a paradigm that might serve as a vehicle for the many kinds of analyses that macroeconomists perform all the time. On the one hand, we can enrich the Walrasian paradigm by incorporating new phenomena; on the other, we can enrich macroeconomics by building up a coherent general theory, rather than accumulating a series of examples. But note, the object is not simply to load the Arrow, Debreu and McKenzie model with a lot of bells and whistles: that has been tried and been found to be intractable. We need new models, that may look quite different from the older style of general-equilibrium model, and there may turn out to be no completely "general" theory that nests the others as a special case. But surely GET is capable of making a greater contribution to mainstream economics, if only we would loosen up and try something different.

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Remarks on monetary theory

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We are honouring the founders of modern General Equilibrium Theory and they deserve the honouring. I have been given ten minutes to discuss how monetary theory fits into what was revealed to us forty years ago and have, of course, to say that it barely fits at all. It is this "non-fitting" and the invitation which it presents to modify the founders' work which is the most interesting and challenging aspect of monetary theory. There has been some progress in this direction, but not very much. Indeed it is not clear whether it is the right move to "fit" monetary theory into or graft it onto general equilibrium theory. One reason for this is that it may be beyond our capacities. Another is that we may in the process do practical harm.

It is of course possible to treat money simply as an asset (but not in a finitely lived economy). This however neglects the medium of exchange aspect which does not seem to be a good idea. To study the latter we need not only to consider sequence economies and the attendant need to endow agents with price expectations but also transaction processes. This may involve a modelling of search, but it surely will involve an explicit modelling of the mechanisms which exist to reduce transaction and search costs. That is, we need to add "mediation activities" to production activities and we need to consider transaction uncertainties and, of course, the role of money in all of these. Some progress has been made in all of this but not much. For instance very little formal theory exists to describe the relation (first noted by Smith), between the ease of transactions and specialisation in production and there are all sorts of unresolved difficulties arising from non-convexity of the transaction technology. We also need to incorporate Diamond "search externalities" into a rather more complete theory of markets.

Since all of this is hard, formal monetary theory has not got very far on the way of the Arrow, Debreu and McKenzie perfection of modelling. But that has not stopped pronouncements in this field. A famous one concerns the "neutrality" of money. A strict statement of which is: comparing the set of real equilibria of two economies A and B which have the same preferences, real endowment and technological possibilities but where B has k times the money stock of A, we find that the equilibria are the same. A simple test of whether an economist has an understanding of economic theory is to ask him or her whether the proposition just quoted entails the impotence of monetary policy. The economist's understanding of actual economic life can be tested by asking the question while postulating that A and B have unique real equilibria.

Once we agree that we need to study sequence economies, we find a natural reason for distinguishing short run and long run equilibria. In the former, agents are still learning and know it, and in the latter they have learnt what there is to learn. Much of what passes for monetary theory is (with honourable exceptions) concerned with the latter which then, in the state of our present knowledge, makes it inappropriate for policy arguments. People living in an economy in long run equilibrium have, for instance, neither need nor desire for monetary policy. Of course there are further distinctions between types of equilibria - for instance that which turns on the assumption that prices are at all times market clearing. That too is an important distinction for monetary theory.

Once one thinks a little beyond the miniature General Equilibrium models of some macro-economists, it also becomes clear that the project of getting a decent and rigorous monetary theory leads one to question the Arrow, Debreu and McKenzie theory of uncertainty (which was quite proper for their construction). In a sequence economy one is bound to think of price (and transaction) uncertainty, for instance if there are multiple spot equilibria, which is not at all the uncertainty of Savage states. In other words one notices that much of uncertainty is endogenous to the economy and its functioning. This then, one suspects, will lead to pretty radical departure from the great opus, in particular in Welfare Economics. Again there are economists who have noticed all of this and have begun serious work, but as of now, no polished theory has emerged.

So far, in my view, the great achievement of those we are honouring today has had rather bad effects on the progress of monetary theory largely because very few have understood the nature of this achievement. Clearly it does not describe any economy we know. That economies behave "as if" they were in Arrow, Debreu and McKenzie equilibrium, is not a proposition which can readily be put to any test (although I am willing to bet quite a lot on the contrary proposition). Making the minimal changes to allow fiat money seem to have considerable formal consequences and in particular requires new elements in the description of economy, for instance, agents' price expectations and their method of learning. And so on. The achievement of the "founders" is that they have made it possible to ask all of these questions in a grammatical way. If they had not, for instance, taught us their very general definition of commodities, we might never have understood the importance of incomplete markets, if we did not know their existence proof we might not now understand why it is required and difficult for a monetary economy. Their work is by far and away the best place to start thinking, but it is not a good place to stop.

Keynes famously dismissed the long run - current theory (certainly in macro-economics) does not get beyond it. This is bad for monetary theory simply because in the long run many of the notorious and difficult problems of the relation of monetary and real phenomena cannot be properly studied. In a very fine paper Lucas did his best for those questions by proposing that agents were ignorant of the money supply. But money supply figures are continuously published. Lucas also rigged the model to yield a unique equilibrium. His paper was a start, but because of the long run equilibrium context, money could have only real effects by agents being "fooled". In a less restrictive context there are many other routes.

To argue this I conclude with the much despised IS-LM construction. I have time only for a staccato argument: (1) The underlying model is not one of perfect competition. That explains the appearance of income amongst the variables. (2) The context is the short period. (3) The diagram is a cross-section of a much larger space of prices, price expectations and labour supply. (4) Hence the intersection of the two curves does not represent a full equilibrium of the economy. What is displayed is a miniature general equilibrium given all the variables relatively to which it is a projection. (5) The argument that stocks and flows are confused is easily dealt with by an appropriate re-interpretation of LM. (6) The apparatus is to be used in process analysis. (7) The cross-section approach is justified by arguments similar to those employed by Hicks when he studied processes by means of the week.

What this apparatus allows us to recognise is that an economy can have many possible futures and that there may be many different consequences to economic policies, e.g. an increased money supply. Even in perfectly competitive, perfect foresight, infinitely lived economies not governed by a fictitious Ramsey maximiser, the equilibrium path of prices is indeterminate. Here it is perfectly possible, for instance, that a higher money stock at once raises actual and expected prices in the same proportion so that the two curves are not shifted at all (recall that it is the real money stock that matters). It was no part of Keynesian doctrine to deny homogeneity. On the other hand the curves may shift because expectations of prices do not change. What will happen in subsequent short periods can, with our present knowledge, only be guessed at, although one can make more or less plausible guesses. But there is nothing that prevents rigorous theorising for given assumptions.

What has happened to monetary theory and to a large extent to macro-economics is this: when we looked for micro-foundations we found Arrow, Debreu and McKenzie. We forgot that they have nothing to say on how equilibrium comes about and of course dealt only with a perfectly competitive economy. We forgot that their theory requires no price expectations and, if we recalled that, we simply put in perfect foresight. Then we claimed that we had a rigorous theory and incidentally that Keynes was dead. Even when one admits that economics is not a subject which often attracts the best minds, this is a sorry story. It has allowed politicians and bankers to call "science" in support of nonsensical policies !

General equilibrium conference: money and macroeconomics

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The most interesting development in macroeconomics in the last fifteen years has been Kydland and Prescott's 1982 real business cycle model, and the large volume of research that it stimulated. Since this paper was a direct outgrowth of the general equilibrium theory we are celebrating at this conference, I think its success is a useful illustration of one of the ways general equilibrium theory can contribute to applied economics.

For years, it has been a cliché in theoretical circles that general equilibrium theory needs to be generalized to incorporate money before one can think about using it in macroeconomic applications. Kydland and Prescott simply ignored this wisdom, took the theory off the shelf as it was (with Bewley's commodity space), specialized it drastically to the point where solutions could be calculated inexpensively, and compared its behavior to U.S. time series.

How well did it do ? People are still arguing about this, but what is interesting is not so much the outcome of this argument, which is certainly still in doubt, as the language in which it is being conducted. When people criticize real business cycle models they criticize its assumptions about demography, preferences, technology, and market structure, in terms that are recognizable to any economist. The private jargon that macroeconomics developed during the period when it was cut off from mainstream economic theory – multipliers and gaps and Phillips curves and so on – has largely passed out of use. Macroeconomist today are expected to be able to discuss their ideas in the language of Arrow, Debreu, and McKenzie. This is progress.

The Kydland and Prescott model is a complete markets set-up, in which equilibrium and optimal allocations are equivalent. When it was introduced, it seemed to many – myself included – to be much too narrow a framework to be useful in thinking about cyclical issues. There is a sense in which this initial reaction is right, I think, but there are two other senses in which it has proved to be wrong. In the first place, whether or not one believes productivity shocks account for *all* employment fluctuations, it is obvious that they account for *some*, so the objective of stabilization policy cannot be taken to be to produce a perfectly smooth time path of output and employment. Once this simple point is granted, one is led to ask the quantitative question: How much of observed variability in output is pathological, attributable to bad policy, and how much is just an efficient response to unpredictable real shocks ? This is one of those questions that becomes obviously pertinent once the right theoretical framework is adopted, but that can be hidden for years when the discussion is conducted within the wrong framework. If the answer to this question turns out to be somewhere in the middle, as I think it will – even for the postwar U.S. – it is still a great service to have it clearly posed.

The second sense in which early criticism of real business cycles has proved wrong is that the approach turns out to be much more adaptable than first appeared to be the case. I will discuss two specific examples. Cooley and Hansen (1989) introduced money into the model with a cash-in-advance constraint, and simulated the resulting model (in which optimal and equilibrium allocations do not, of course, coincide) to see how much realistic money supply variability affected the cyclical behavior of the system. The answer was, hardly at all. In this model, there are no goods or labor market rigidities in the system and money affects real variables only through inflation tax effects, so substantively, this conclusion has been a widely-held conjecture for years. But Cooley and Hansen quantified this fact for the first time, in a framework that permits replication and theoretical experimentation. Now, one even hears people referring to "monetary real business cycle models." The terminology is pretty awful, I know, but the point is well taken: Real general equilibrium theory is a very useful point of departure, even – or maybe especially – for thinking about issues it does not directly address.

As a second example, Danthine and Donaldson (1990) modifies Gary Hansen's real business cycle model to accomodate a form of efficiency wages. The equilibrium they calculate is, as one would expect, sub-optimal, and it exhibits periods of non-wage job rationing. The theoretical literature on efficiency wages had previously consisted only of illustrative, partial equilibrium examples, with verbal hints about possible macroeconomic implications. By recasting the discussion in a general equilibrium framework similar to Kydland and Prescott's, Danthine, and Donaldson make it possible to see what these implications are and to begin to assess their quantitative importance.

In short, a very wide variety of possible kinds of business cycle dynamics are now being explored within frameworks close to the one Kydland and Prescott introduced. All of these models involve individual agents solving dynamic decision problems that are consistent with the environment they are assumed to be living in. All of them are explicit about the assumed preferences, technology, and the market structure within which trading takes place. None of these features was even close to obtaining in the macroeconomic models that were in standard use in the 1960's, or in the IS/LM models that still dominate the textbooks. Yet these features are essential (though not sufficient) for any kind of practical usefulness. And all of them are immediately ensured once one decides to state the model in general equilibrium language.

Since I have been trying to articulate my enthusiasm for the quantitative, theoretical approach that Kydland and Prescott used, I suppose I should note at the same time my substantive dissatisfaction with the results so far achieved. I think it was monetary instability combined with some form of nominal price rigidity that was responsible for the Great Depression of the 1930's, in the U.S., so I see the postwar experience not as evidence that monetary instability is irrelevant (as real business cycle theorists see it) but as evidence that postwar monetary policy has been very good, relative to the past. When I referred to the "success" of the Kydland and Prescott paper at the beginning of these remarks, then, I did not mean that I think they or anyone using their methods has succeeded as yet in explaining real economic instability.

But substantive success is hard to come by, and when achieved it typically involves the joint efforts of many researchers. For such a process to get anywhere, those involved need a common language for expressing agreement and disagreement, and the better this language is, the more fruitful will their interactions be. Slutsky, Frisch, and Tinbergen introduced the language of stochastic difference equations into macroeconomics, and their innovation made possible a complete and very fruitful transformation of the field. When the limits of this largely statistical language had become apparent, Kydland and Prescott proposed restating the issues in the general equilibrium language of Arrow, Debreu, and McKenzie. When I call their work successful, I mean only that I think this important translation has succeeded, and I think we have only begun to see how far it can take us.

I should also add (since I know theorists are a sensitive lot) that the fact that macroeconomists are making good use of the general equilibrium theory of 20 or 30 years ago does not at all mean that we are indifferent to more recent or future developments. I agree entirely with Frank Hahn's famous remark that monetary theory still awaits its Debreu, and I hope I will be among the first to know when he shows up. I want to be one of his first customers. But I do not expect macroeconomists simply to admire this new monetary theory when it arrives. On the contrary, I expect we will specialize it, focus it, quantify it, and *use* it, to get better answers to the specific empirical and policy questions that we are curious about. That's what applied science is all about, and applied science is what macroeconomics is, or at least what we want to make it into.

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