2 Recent Developments in the Theory of Incomplete Markets

ANDREU MAS-COLELL
Harvard University

Introduction

The purpose of the paper is to review some of the recent accomplishments of the general equilibrium theory of incomplete markets. The theory was initiated in the 1960s by R. Radner, pursued by, among others, Hart, Jordan, Grossman and Wilson, and codified by Kreps. In recent years, the interest in the theory has been renewed. A forthcoming special issue of the *Journal of Mathematical Economics*, edited by J. Geanakoplos, will provide a convenient status report. I refer to this issue for precise references. Here I will only mention authors.

The general equilibrium theory of incomplete markets aims at constructing a coherent model which the possibilities of intertemporal and, mainly, inter-state of nature wealth transfer possibilities are restricted in ways which are *a priori* given. The theory is thus meant to be a short cut and it is based on a big leap of faith. The hope is that one will succeed in enriching the foundations of applied fields such as finance (based in excess on the complete, perfectly competitive market mechanism) without at the same time having to deal with the thorny problem of why some contingent commodities exist and others not; that is to say, without having to explain why markets are incomplete.

The missing hypothesis of complete markets is not minor. It implies, for example, that the theory becomes inherently dynamic.

I will review the recent developments under three headings: (a) existence of equilibrium; (b) determinateness of equilibrium; and (c) restricted Pareto optimality.

Existence of Equilibrium

Relative to where it was left by Radner, progress has been made on two fronts:

(a) Existence of equilibrium has been established in great generality for models with a finite number of final states. In particular, the restrictions on short sales imposed by Radner have been eliminated. For the case where assets pay in a single commodity in each state, the proof (due to independent work by Cass, Duffie, Wener, and Geanakoplos-Polemarchakis) has followed more or less traditional lines. For the general case, the proof is only generic (i.e. equilibrium exists in most cases, but there are exceptions, the celebrated example of Hart is the most well known) and of a surprising mathematical depth (Duffie-Shafer, Husseini-Lasry-Magill, Geanakoplos-Shafer, Hirsch-Magill-Mas-Colell). A fascinating aspect, at least for a mathematical economist, of this latter result is that it provides the first instance in economics where an existence problem cannot be solved by means of Brouwer's or Kakutani's fixed point theorem. More advanced topological tools are essentially required.

(b) Even with restrictions on short sales and a single good per state, we are much farther from a complete solution in the case of an infinite horizon and/or infinitely many states of nature. There are results for the countable case (by, among others, Green-Spear, A. Hernandez and Zame) and also for some situations where the economic environment is suitably recursive (there is relevant research by Hellwig and, also, by Duffie-Geanakoplos-McLennon-Mas-Colell). But we are still very far from theorems of a generality similar to what is available for the complete market model.

So far the existence research has been highly abstract and non-constructive. A serious research effort on computational issues is much needed.

Determinateness of Equilibria

It is well known from the work of Debreu and

many others that the classical complete markets model with a finite number of commodities is determinate, in the sense that generically (that is, in all but exceptional cases) every equilibrium is locally isolated.

Except for one proviso, the same is true, in principal, for the incomplete market model with a finite number of states (as it has been shown by Geanakoplos-Polemarchakis). The proviso is that the assets are even partially financial (the return in some states is denominated in a commodity which is available in zero supply and does not enter any agent utility function, call it money) then in the incomplete market model (but not in the complete one) the presence of a high-dimensional continuum of equilibrium becomes typical (Cass, Balasko-Cass, Geanakoplos-Mas-Colell).

As a matter of interpretation, I believe, however, that the above conclusion, namely that the incomplete market model is as determinate as the complete, is misleading. The reason can be illustrated by a trivial example. Let S be the set of states. If markets are complete there is no particular reason to think that the number of equilibria depends on the cardinality of S (this vague statement can be made precise). This is not so if markets are incomplete. Take the extreme case where no transfers of wealth across states are possible at all. Then equilibrium is simply a list of S price systems $(p_1, \ldots, p_S)$ with the property that each p_i clears the spot markets at state i. If at each state there is a unique clearing price then the overall equilibrium is unique. But suppose that at the spot markets there can be multiplicity of equilibria (there is nothing pathological in this; to simplify, suppose there is multiplicity at each state). Then the number of overall equilibria is at least $2^{\#S}$. *If* $\#S$ is large then $2^{\#S}$ is very large indeed (although still finite). In the limit if S is a continuum, then there is a continuum of equilibria. Thus, the qualitative conclusion one gets from the case where S is a continuum ('local uniqueness is not generic in the incomplete market model') seems to me a better description of the reality of the situation than the contrary conclusion derived from the case with a finite number of states.

Note that the indeterminacy considered in the last paragraph does not depend on infinite horizons. In fact, the possibility of robust indeterminacy of equilibria, which emerges naturally in so many models departing from the classical hypothesis of the Walrasian model, seems to be due to one of the two, logically distinct, following possibilities: either incomplete markets (sunspots fall into this category) or a double infinity of commodities and agents (the deterministic overlapping generation models being the most well-known example).

Optimality Properties

Clearly an effective incomplete market equilibrium cannot be expected to be a full Pareto optimum (the term effective is used as a reminder of the fact that sometimes with few or derived assets it is possible to span all possible wealth transfer). The real question is if it is a constrained Pareto optimum.

But what is the relevant constraint set? There is no easy general answer because, on the one hand, the set must respect the given structure of assets but, on the other, the theory does not tell us why some assets exist and others do not. With so much missing, it is difficult to be sure of what are feasible policy measures.

At any rate, defining the feasible set is easy in the case (studied by Diamond) where there is one good per state and where production uncertainty is multiplicative. Then the incomplete market model can be viewed as a standard Walrasian model with redefined consumption set and the equilibria and therefore constrained optima.

A simple example due to Hart shows that with more than one good per state (and no production) one can have Pareto ordered equilibria. Hence by any definition of constraint, constrained optimality is not guaranteed. Unfortunately, this is not the end of the story. As if a robust example was not bad enough, a number of recent results (by Geanakoplos-Polemarchakis and Geanakoplos-Magill-Quinzii-Dreze) show that for a sensible notion of constraint, the incomplete market equilibrium is generically not a constrained optimum.

This is where we are, but I doubt that the end of the story is going to be so negative. I still would hope (although I have little to substantiate it) that price taking will be proved to have some optimality property even in the incomplete market context. More importantly, for these negative results to carry complete conviction, they will have to survive the necessary refinements of the concepts of policy feasibility.

Conclusion

My conclusion is that there is much exciting work going on in incomplete market theory but that there are still some hurdles to be overcome. Partly repeating myself, I will mention two:

(a) The spur of interest in incomplete market theory that followed Radner's innovation died out partly because of the inability to solve the problem of the appropriate objectives of the firm. The current second wave has obviously also forced this key issue and there is important work by Duffie-Shafer, Geanakoplos-Magill-Quinzii-Dreze and DeMarzo. But, at least for me, it is too early to say if the highest peak has already been conquered or still lies ahead.

(b) Some theorizing on why markets are missing is necessary. Belief cannot be suspended indefinitely. The same observation applied, incidentally, to the, in many respects parallel, theory of incomplete contracts.