

Book Review

Financial Markets and Incomplete Information: Frontiers of Modern Financial Theory, Volume 2. Sudipto Bhattacharya and George M. Constantinides, eds. Totowa, N.J.: Rowman and Littlefield, 1989. 361 pp., \$37.50 (cloth), \$19.95 (paper). ISBN 0-8476-7597-1 (cloth), 0-8476-7598-X (paper).

Most books of readings contain an introduction by the editor(s) that attempts to synthesize the papers that have been chosen for inclusion. This book and its companion volume [reviewed previously in Duffie (1989)] depart from this pattern by, in addition, having either one or two discussions of each paper. This makes them considerably more useful than a standard book of readings since it allows a much broader perspective to be obtained.

Rather than review each paper and discussion in turn, I will summarize my views on some of the strengths and weaknesses of the volume. The editors outline a number of their goals in the preface. The first is to collect some of the important papers published between 1973 and 1986 and combine these with discussions in an attempt to provide a summary of recent progress in theoretical financial economics for both specialists and nonspecialists. The second is to forecast the trends in theoretical financial economics. Finally, the volumes are meant to provide material for PhD-level courses and possibly advanced MBA and undergraduate courses.

How successful is the second volume in achieving these aims? One of the things that a discerning reader of the book may quickly notice is the fact that although the book was published in 1989 the most recent references in many of the discussions are dated 1986; there are some references to 1987 papers but these are usually "forthcoming." As the discussants know only too well, the obvious deduction that the discussions were mostly written in 1986 is, in fact, correct! The publishers have taken a long period of time to get the book into print. Despite this handicap, I think the editors have to a large extent achieved their aims. The book does represent a good statement of the current state and likely future development of theory in the area, for both specialists and nonspecialists. It will also be useful for PhD-level courses, although I would hesitate to use it for MBA or undergraduate courses.

Inevitably, the choice of papers will not meet with unanimous approval. My first observation in this regard is the fact that the first two papers in the volume covering incomplete information, namely, Miller (1977) and Hart (1977), use frameworks with complete infor-

mation. The paper by Miller has clearly been one of the most influential in recent years and needed to be included somewhere. However, given the choice to split the volumes into the areas of valuation and incomplete information I think this paper more naturally belongs in the former than in the latter. The decision to include Hart (1977) is more surprising. This is not because it is not an interesting contribution. The paper is a general equilibrium analysis of the proposition that allowing takeover bids leads to a Pareto-efficient allocation of resources, in the context of a model with complete information but incomplete markets. Hart shows that takeover bids will lead to an efficient allocation of resources in a fairly restrictive set of circumstances, namely, when firms are "small" relative to the aggregate economy. The decision to include it is surprising because the theoretical paper that has possibly had the most impact in mergers and acquisitions between 1973 and 1986 is Grossman and Hart (1980) and I would have thought this would have been the obvious one to include. In fact, in his excellent discussion of the area, Spatt takes this paper as his starting point and does not even reference Hart (1977).

Apart from these, two other papers that some people may be surprised to see included are Bhattacharya and Pfleiderer (1985) and Salant and Henderson (1978). Their inclusion reflects the editors' laudable attempt to forecast the future direction of development of the subject. I would agree with their judgment that the application of agency theory to portfolio management and the theory of speculation are two areas that will prove to be important in the coming years. [This is not an unbiased opinion as I am the discussant for Bhattacharya and Pfleiderer (1985)!]

The paper that was not included that I would have expected to be is Jensen and Meckling (1976). Whenever I read this paper, I am always amazed anew at how many interesting ideas it contains. The introduction of agency theory to the area has led to many contributions in the theory of corporate finance and still continues to lead to interesting contributions. For example, Jensen's controversial "free cash flow theory" [see, e.g., Jensen (1986)], which is essentially an application of agency theory in a mergers and acquisitions context, is stimulating the production of a large literature on corporate takeovers. My own belief is that the application of agency theory to asset pricing will also be important for a full understanding of departures of stock prices from fundamentals [see, e.g., Allen and Gorton (1989)].

Another surprising omission is the lack of any papers by Myers. A strong argument could be made for including Myers (1977). The idea that when a firm has debt outstanding it may underinvest because equity holders have to share returns with bondholders has been influ-

ential. The notion that the type of security a firm chooses to issue is a signal of its future prospects, set forth in Myers and Majluf (1984), has also proved to be an important one.

With the benefit of hindsight that the lag in publication allows, it can be argued that another omission is the lack of a paper on market microstructure and asymmetric information because this is an area that has received a good deal of attention since 1986. Two obvious possibilities are Kyle (1985) or Glosten and Milgrom (1985). However, the lack of a paper in this area is made up for to a large extent by Kyle's thought-provoking discussion of Grossman and Stiglitz (1980).

In fact, one of the strengths of the discussion format used is that this argument holds in a number of other areas where a particular paper might have been included. Thus, Stiglitz and Weiss's (1981) paper on credit rationing and asymmetric information has been very influential but its omission is largely mitigated by Weiss's discussion of Diamond and Dybvig (1983). Similarly, Diamond's (1984) paper on financial intermediation and Tirole's (1982, 1985) papers on speculative bubbles are covered in Diamond's discussion of Leland and Pyle (1977) and Tirole's discussion of Salant and Henderson (1978). The discussion format also allows coverage of subsequent contributions when the paper chosen for inclusion is an early classic. Thus Williams' discussion of Bhattacharya (1979) contains coverage of John and Williams (1985), Miller and Rock (1985), and other important papers that followed.

In addition to allowing a much wider range of the literature to be covered, the discussions also contain many important insights. For example, in his discussion of Rock's (1986) theory of underpricing of initial public offerings, Raviv makes the important point that the issuing mechanism in Rock's model does not maximize the capital raised by the issuing firms; a higher amount could be raised with an auction mechanism similar to that used to sell Treasury bills. The comprehensiveness of the survey aspect of many of the discussions is also good. Before reading Jacklin's discussion of Diamond and Dybvig (1983), I had not realized the nature of Bryant's (1980) contribution to the literature on bank runs.

Criticizing the failure to include certain papers is, of course, an easy task when there have been so many good ones in the area in recent years. Overall, the editors have done an excellent job in their structuring of the book and their selection of papers. The discussions are also very good. It is certainly a book I recommend buying.

When a suitable time has elapsed, I hope that the editors repeat the exercise. One change that might be worth considering at that time is to have more papers discussed by two people since I partic-

ularly liked the range of perspectives that this allowed in the cases where it was done. For example, in discussing the paper by Miller (1977), Kim surveys other papers in the area while Scholes and Wolfson are able to give some insight into the implications of tax code complexities for capital structure. In discussing the paper by Grossman and Stiglitz (1980), Admati provides a critical coverage of the rational expectations literature while Kyle focuses on some of the market microstructure issues raised by the paper. The other change that I would suggest is that the publishers eliminate the lag in publication!

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