This volume reviews the principal studies which have been made in recent years but adds no important insights to their interpretation. The reader is left with the usual sense of frustration which arises whenever he is furnished with carefully tabulated averages which perform their usual function of obscuring differences. Similarly, this type of analysis ignores the potentially rewarding exploration of the reasons for persistently inferior adjusted rates of return. Surely an efficient market must require some special effort on the part of the manager to produce below average returns in all kinds of market environments! Would not an examination of this systematic underperformance shed light on the investment decision-making process which could give new definition to the returns, either positive or negative, to security selection?

A striking observation from this kind of a review is the notable instability of performance measures in different capital market environments. The contrast is between the 1958–1968 years and subsequent periods. One is tempted to believe that the updating of many empirical tests of concepts of efficient portfolios will be highly productive when the years 1969–1975 can be observed in perspective. Equally productive may well be efforts to achieve the more effective use of market measures for homogeneous groups which relate to the composition of individual portfolios. Measuring the differential behavior of the various sectors of the market is a valuable exercise which should not be submerged in the analysis of efficiency of individual portfolios.

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Interest Rates on Savings Deposits. By MRYON B. SLOVIN and MARIE E. SUSHKA. Toronto and London: Lexington Books. 1975. Pp. XIII + 168. $16.00.

This book examines four topics having to do with the interest rate paid on savings deposits, either at commercial banks or non-bank savings institutions. These topics are: (1) the rate setting behavior of savings institutions, with a focus on distinguishing the objective function that these institutions maximize (Chapters 1–6); (2) the market for negotiable certificates of deposit (Chapter 7); the stability of the demand for money during the period 1955 to 1973 (Chapter 8); and (4) the macroeconomic impact of changes in Regulation Q ceilings (Chapters 9 and 10).

The only innovative section of the book is the authors' treatment of the first topic—the rate setting behavior of savings institutions. Their analysis of the other three topics is quite standard amd does not contribute significantly to existing knowledge. Indeed, the book would have been a far more coherent and tightly integrated treatise if Chapters 7–10 had been omitted entirely and the analysis contained in Chapters 1–6 expanded. Their analysis of the rate setting behavior of savings institutions, while at times imaginative, is also flawed by a number of expositional and theoretical deficiencies. In sum, this book is of interest mainly to those interested in modeling the microeconomic behavior of financial institutions.

In the remainder of this review I shall describe in greater detail the topics addressed by the book and attempt to point out some of its strengths and weaknesses.

Chapters 1 through 6 examine the rate setting behavior of savings institutions with a view towards discovering their objective function. Do they maximize profits, deposits, or utility? The authors first develop alternative models of rate setting behavior (assuming alternative objective functions) and show the comparative static properties of these models. In general, the properties of the profit-maximizing and utility-maximizing (with both profits and deposits entering the utility function) models are identical while those for the deposit-maximizing model differ in several ways. Most importantly, both profit- and utility-maximizing models imply that savings yields will be: (1) positively related to yields on instruments that are close substitutes for savings deposits; (2) negatively related to shifts in the demand for savings deposits; and, (3) independent of past portfolio investment decisions (i.e., yields on the intermediary's inherited assets). The deposit-maximizing model, on the other hand, implies that savings yields will be: (1) negatively related to yields on instruments that are close substitutes for savings deposits; (2) positively related to demand shifts; and, (3) will not be independent of past portfolio decisions (but will be positively related to yields on the institutions's inherited portfolio). These distinctions form the basis for an extensive cross-section and time-series empirical analysis, performed in Chapters 5 and 6. The authors conclude that the empirical results are inconsistent with the deposit-maximizing model but consistent with (either or both) the profit-maximizing and utility-maximizing models.

In arriving at this conclusion the authors employ a number of procedures which are of questionable validity. In particular, their model of firm behavior implies (or requires) that savings institutions be monopsonists in the savings market, or that they be price-setters rather than price-takers. This assumption is difficult to accept. Indeed, competition for savings deposits would seem to be quite keen among banks, savings and loan associations, and mutual savings banks. Alternatively, their model might be viewed as implying that all savings institutions in a particular (geographical) market collude on savings rates, jointly setting the market rate. In this case, however, it would not be possible to identify the objective functions of the colluding firms with the "single firm" approach utilized by the authors. Second, the authors' critical proof (on p. 8) that the optimal deposit rate of a profit maximizing intermediary is independent of the rate of return earned on the institution's inherited portfolio employs confused notation (between i_{-1} and i_{-0}) and cannot possibly be correct in its present form. (Specifically, equation 2.11 cannot be shown to equal $i_0kD_0-r_0D_0$). Lastly, their empirical work contains a number of annoying features: the measure of k (p. 41) is inconsistent with the analogous theoretical construct; dubious measures of the degree of competition are used, such as offices per capita (p. 41); changes in the degree of competition are seen as shifting the demand schedule, similarly to changes in income (p. 41); there are no statistical tests of the structural stability of the estimated equations from year-to-year; etc.

The second topic of the book—contained in Chapter 7—is an examination of the market for negotiable certificates of deposit (CDs). Equations explaining both the demand for CDs and the yield on CDs are estimated using quarterly data for the period 1962 to 1972, where the estimating equations explicitly incorporate the fact that at times Regulation Q constrains CD rates.

The third topic deals with the stability of the demand for money during the period 1955 through 1973 (Chapter 8). More specifically, has the administration of Regulation Q ceilings induced shifts in the structure of the demand for money function? Standard demand for money functions are estimated for three distinct subperiods during the 1955–1973 period and evidence of structural instability is found. A problem with their analysis, however, is that the theoretical (or behavioral) rationale for distinguishing among the three subperiods selected by the authors for study is not clear. Their explanations are mechanical—"a span during which yields on non-bank liabilities were sharply higher than time deposits at commercial banks," etc. Also, although the authors frequently employ quite sophisticated econometric techniques, they inexplicably fail to apply the standard statistical tests of structural stability, preferring to "eyeball" the estimated parameters for evidence of instability.

The final two chapters of the book (Chapters 9 and 10) are a macroeconomic analysis of the effect of Regulation Q ceilings on the effectiveness of monetary policy. In Chapter 9 these questions are examined theoretically within the context of a standard Keynesian model of income determination and the comparative static properties specified for alternative monetary policy variable specifications—money supply, open market interest rates, or high powered money (unborrowed reserves plus currency). In Chapter 10 various simulation experiments are conducted with the Federal Reserve—MIT-PENN econometric model to assess the validity of their comparative static predictions. In general, the results of these experiments—too numerous to summarize—are consistent with the authors' expectations.

A particularly disturbing aspect of this analysis is the absence of a discussion of what is meant by "effectiveness of monetary policy." The authors appear to argue that "effectiveness" is to be judged by the size of the respective money multipliers (e.g., the magnitude of the impact of a change in money supply on income and open market rates). They say: "...the size of each of the money multipliers in case B is likely to be smaller than in Case A. This suggests that the impact of monetary policy on economic activity is less effective if..." (p. 133). This is not a meaningful criterion of effectiveness. As long as the money multipliers are known to the Federal Reserve, whatever their respective magnitudes might be, monetary policy can be made equally effective by adjustments in the policy target (e.g., increasing the money supply somewhat more). Surely monetary policy effectiveness is a concept having to do with the predictability (or variability) of the money multipliers, rather than with their magnitudes. About this the authors have nothing to say.

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