Hyman Minsky and Charles P. Kindleberger, stressing overshooting of asset prices, leveraging of asset holdings, and the resulting financial fragility, is the direction preferred by many of the participants.

(2) There are a number of things that can be done to reduce the likelihood of a crisis. Sound monetary and fiscal policies, naturally, were mentioned by a number of participants. Deposit insurance was singled out several times as possibly needing reform. But concerns were voiced that scaling back deposit insurance would increase instability. And doubts were raised about whether the increase in discipline would be significant, and about whether reform was politically feasible. William Poole made some interesting suggestions for using tax incentives to discourage selloffs in jittery markets and to discourage the accumulation of debt that leads to financial fragility. And Rudiger Dornbusch warned that hasty deregulation of capital markets in developing countries is creating weak links in the world's financial system.

(3) Even though the economy has surmounted a number of financial problems in recent years, most of the experts are convinced that there is a nonnegligible risk of a destructive financial crisis. Opinion ranged from the dark and foreboding picture drawn by Benjamin Friedman to the more optimistic view of Paul Samuelson who was skeptical of the idea that what happens on Wall Street determines what happens on Main Street, but none of the participants was willing to say it just can't happen.

This book should be required reading for academics, opinion makers, and policy makers concerned about the prospect for a major financial crisis. I would also recommend it to graduate students in economics so they can see one way that distinguished academics use the apparently esoteric models presented in the classroom.

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Booms and recession in a noisy economy. By ROBERT E. HALL. Arthur M. Okun Memorial Lecture Series. New Haven and London: Yale University Press, 1991. Pp. xii, 71. $20.00. ISBN 0–300–04857–2. *JEL 91–0768*

Just as most macroeconomists' interests seem to be leaning toward long-run issues and away from the business cycle, this book—together with the current stubborn recession (or recovery?)—reminds us that we still have not settled the most basic issues about short run economic fluctuations: where do shocks come from, how large are these, and does the underlying microeconomic structure amplify or smooth the impact of these shocks.

This is a stimulating book that alternates swiftly between provocative propositions and new ways to measure and analyze cyclical fluctuations. The original style of the book can be quickly grasped in its organizing theme: *temporal agglomeration.* This is just another expression for the business cycle, but it brings about the potential similarities between temporal and *spatial* phenomena. The book begins with the premise that the same factors that lead to large heterogeneity in the geographical concentration of population and economic activity are also partly responsible for the heterogenous distribution of activity over the business cycle. Perhaps my main criticism is that this insightful postulate is never tested or analyzed in detail, and is simply used as an extremely entertaining motivation for increasing returns.

This book consists of three chapters. The first one is, in my view, the most interesting of them. After highlighting the large concentration of activity over different hours, seasons, years and even decades, Robert E. Hall argues that the basic smooth concave production/concave preferences setup of standard macroeconomic analysis is ill-suited to explain the uneven distribution of activity over time and space. The large exogenous productivity shocks and massive coordination failures required in the standard model to generate realistic fluctuations can be replaced, or their required magnitude reduced, by the presence of increasing returns in production. This simple modification of the standard assumptions renders, through a Jensen's inequality argument, a positive correlation between the mean and variance of output. Temporal agglomeration is then a natural—and possibly efficient—outcome which is bounded only by the eventually offsetting concavity of preferences.

One of the main empirical implications of these increasing returns is that Solow's total factor productivity measure should be procycli-

cal; a property known to hold for U.S. data. In my own work with Richard K. Lyons we have given further support to Hall's findings and conjectures, and shown that the source of aggregate increasing returns seems to be external rather than internal to firms. Whether the cyclicality of the Solow residual is primarily due to aggregate increasing returns or to other factors—mainly labor hoarding and technology shocks—is still unsettled. However one must at the very least recognize that the ideas expressed in Chapter I of this book give new life to the issue.

The second chapter of the book turns into "noise" measurement. There noise is defined as that part of investment fluctuations that cannot be accounted for by current and expected deliveries of goods. Starting from a certainty equivalent model which yields the current stock of capital as a linear function of expected deliveries, Hall exploits the similarity of his setup with that of standard asset pricing equations to compute a perfect foresight value of the stock. The difference between the actual and perfect foresight stocks is due to two factors: noise and expectation errors. By projecting this difference on variables known at the time agents made their investment decisions, one is left with a measure of noise. The volatility of this measure gives a lower bound to the actual volatility of noise since it only contains that part of noise which is orthogonal to expectations errors. Using this simple but elegant procedure, Hall analyzes the behavior of output, fixed investment and inventories in postwar U.S. and concludes that a large fraction of the high frequency movement in these variables is accounted for by noise.

Chapter II begins by defining noise as fluctuations that arise *spontaneously* within the business sector, which seems to imply that noise is a shock, or possibly "animal spirits." Over the course of the chapter and especially in the conclusion this potentially misleading interpretation is qualified and noise is seen as the sum of a specification error and a spontaneous component. My preferred reading of the results in this chapter is that standard models can describe long-run relationships fairly well but fail to describe the short-run behavior of aggregate time series. The measures of noise provided in this chapter are an imaginative estimate of how large the component is that macroeconomists have yet to explain.

The final chapter provides a generalized aggregate demand/aggregate supply framework to characterize the effects of noise on prices, output and interest rate determination. The main virtues of the proposed apparatus are that it makes explicit the many places where monetary nonneutralities can enter in the standard aggregate demand/aggregate supply model, and it expands the latter model by letting the interest rate affect aggregate supply. Among other things, Hall shows that in this expanded context changes in the demand for goods have ambiguous effects on the price level since they shift both aggregate demand and aggregate supply.

Overall, this book is enticing, with plenty of imaginative descriptions and coining of new expressions, like *temporal agglomeration, call option models of sticky prices* and many others. I would be surprised if readers came away in full agreement with all the points Hall makes in *Booms and Recessions in a Noisy Economy*, but I would be even more surprised if readers did not find the book highly stimulating.

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Comparative performance of U.S. econometric models. Edited by LAWRENCE R. KLEIN. New York and Oxford: Oxford University Press, 1991. Pp. x, 325. $45.000. ISBN 0–19–505772–4. *JEL 92–0511*

The construction and use of large-scale econometric models has been a major component in the development of econometrics and applied economics over the last fifty years or more. To an outsider, not immediately involved with these models, they appear to be extremely complex and are difficult to comprehend and to appreciate. Although many are rather complicated they still remain very simplistic approximations to the actual economy. To help with their understanding, a Model Comparison Seminar is held at irregular intervals and this has developed several ways to compare models. In general, it is agreed that identifying differences of specification is less important than asking how the performances of the models compare, and this is certainly the important question to their potential users. This book provides seven chapters that compare U.S. mod-