how workers' welfare and the distribution of income and wealth were affected by the inflation. He also analyzes the extent to which the hyperinflation amounted to a tax on foreign held balances of paper marks. Among the most interesting sections of the book is that concerning revaluation of debts in the post-stabilization period. The issue was whether the state should apply the principle of *caveat vendor* to hyperinflation creditors or the state should revalue the debts on the grounds that "circumstances" had altered in "unforeseeable and fundamental ways." For the obvious reasons the Reich Finance Ministry was opposed to any revaluation. The issue occupied the Reichstag repeatedly during the years following stabilization.

Overall, I found this a valuable book — of interest both to economic historians and monetary economists. I suspect that it will be often cited as scholars work out the implications of Holtfrerich's archival work for various hyperinflation theories. Indeed, I wish Holtfrerich had done more of this himself. I would have liked to have read a section entitled "How my research bears on the work of Phillip Cagan and others." But the absence of such a section was one of very few disappointments in an otherwise well written and interesting book.

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Financial Innovation and the Money Supply.

By T. M. Podolski, New York: Basil Blackwell Inc., 1986. Pp. ix, 253. \$45.00.

The financial system is constantly changing as a result of new regulations and laws as well as technological advances. Fitting all of these financial innovations into monetary theory and explaining their ramifications for monetary policy is a challenging and important task. Financial innovations such as eurodollar markets, N.O.W. accounts, electronic funds transfer system, and a growing Federal Funds Market will have an effect on all schools of monetary theory. This book carefully analyzes the historical development of many financial innovations and their relationship to traditional monetary theory. In addition, the book explores the empirical literature on how innovations are related to money supply and money demand.

The book begins with a historical chapter on monetary theory. This chapter discusses the theories of Keynes, Hicks, Friedman, and Schumpeter as well as other economists historical perspectives on monetary theory. This chapter lays the groundwork for the analysis of financial innovations in later chapters. Chapter two describes the monetary system. Chapter three is on the measurement of money supply. This chapter which is the best chapter in the book surveys the empirical literature on near money and its effect on monetary conditions. There is a nice discussion of the Divisia indexes of money supply which were developed to deal with the problem of near money. Chapters four, five, and six describe various financial innovations. Chapter seven explores the historical and political reasons for financial innovation. These reasons are then contrasted with the possibility that financial innovations are the result of lower transactions costs from changing technology. Chapter eight is the conclusion.

Throughout the book the author does a careful job of presenting the chronological development of various financial innovations. The book's main purpose is to relate historical views of monetary theory to financial innovations which are now occurring. Throughout the book the author describes financial innovations and then relates them to the supply and demand for money as described in chapter one. In this way the historical literature is extended into the contemporary environment. In this process the historical development of the various financial innovations are described thoroughly. The changes in the financial system are then analyzed from the perspective of a number of historical views on monetary theory. An example is the excellent discussion of N.O.W. accounts and their effect on the monetary system.

The book has almost no discussion of such modern theories as rational expectations or a modern microfoundations approach to monetary theory. The occasional comments on rational expectations appear to be afterthoughts, and are never analyzed extensively. Topics such as the recent literature on dynamic inconsistency in the rules versus discretion issue in monetary theory are never mentioned. Likewise the current microfoundations approaches to macroeconomic theory are not discussed in any detail. Many of the innovations discussed in the book seem to lend themselves to modeling along the lines of the modern microeconomic foundations of macroeconomics literature. Podolski points out that more microeconomic research on financial innovations is needed and I certainly agree. The book definitely motivates interest in the financial innovations; however, I would like to have seen more discussion of the modern theoretical literature.

A typical example would be the discussion of futures markets which appears on pages 127–129. The book notes that the first financial futures market was set up in Chicago in 1975, and that there has been a proliferation of financial futures since that time. Of course, it also noted that these allow financial institutions to hedge interest rate risk. The discussion was interesting, and it roused my curiosity about how the introduction of financial futures would be related to contemporary monetary theory. However, the presentation was too brief, and I would have liked to have seen more discussion of complete markets there. In addition I would be interested in a model or discussion of how the introduction of complete markets would effect money supply or money demand. Unfortunately the large number of financial innovations which are discussed limits the amount of space devoted to each.

Alternatively, in the case of eurodollars, the discussion is carried out in several different places. The expanded discussion allows the author to suggest several different ways that growth of the eurodollar market can effect the economy. I found the author's suggestion that the international money markets might be more "contestable" to be interesting. I would like to have seen more discussion of this point.

In summary the book considers many financial innovations and provides a historical analysis of each, and relates them to monetary theory. The issues raised are interesting, however I would like to have seen fewer topics covered, leaving more space for discussion of how these relate to new ideas in monetary theory.

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The Michigan Model of World Production and Trade.

By Alan V. Deardorff and Robert M. Stern, Cambridge: The MIT Press, 1986. *Pp.* xiv, 274. \$30.00.

This volume presents the Michigan Model, which Deardorff and Stern have been working on since 1974, and some of the simulations they have used it for.

Theirs is a linearized model which identifies 22 tradeable and 7 nontradeable industries in 34 countries, plus an aggregate sector representing the rest of the world. All sectors are taken to be competitive. The capital stock is fixed and immobile between sectors. Various assumptions are made about money wages. They are variously held fixed, allowed to clear the labor market sector by sector, allowed to clear such country's aggregate labor market, and permitted to respond slightly to prices. Production in each sector is a CRS fixed proportions function of a value added aggregate within each commodity classification, where the value added aggregate is itself a CRS variable proportions function of labor and capital and each of the commodity aggregates are CRS variable proportions functions of domestic and foreign-produced commodities. Each country's demand for each foreign produced commodity does not distinguish between country of origin. Utility is Cobb-Douglas in nontradeable consumption levels and aggregates of home and imported tradeable goods. Sometimes domestic absorption is fixed, and sometimes domestic expenditure varies to keep aggregate employment fixed.

An alternative closure which also provides interesting information is to hold the real wage constant, and then allow enough flexibility in prices or expenditures to close the system. Andy Stoeckel [3], in a study which he directed at the Australian Bureau of Agricultural Economics, used this closure rule to measure the cost in terms of jobs to the European Community of its common agricultural policy which involves import restrictions and subsidies to EC agriculture along with sales of the surplus at distressed prices on world markets. Such closure in effect translates efficiency-reducing policies into job losses, so the message one gets from Stoeckel-type exercises is to do away with efficiency-reducing policies if you want to save jobs, whereas the message a naive policymaker who doesn't really understand the mechanics of the model gets from a fixed-money wage and fixed absorption closure is the mercantilistic one that for high employment we should pursue a positive trade balance.

I am puzzled why the authors refer to their estimates of welfare change as partial equilibrium. They are in fact just as general equilibrium as their estimates of changes in output or employment levels. They are partial, however, in that they measure one-period consumer benefit without taking into account the effect of changed reserve accumulation on future welfare and that they attach a zero value to the leisure of the employed. Also, in thinking about the utility of linearized models one should not forget that as Dixon, Parmenter, Sutton and Vincent [1] note, there is a process of coefficient updating by which linearized models can be forced to provide estimates of multipliers for finite changes in exogenous variables to any