

Discussion

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Let me begin by emphasizing that I view Walsh's (1982a) paper as a useful and skillfully-executed piece of work. In **particular**, I strongly agree with the paper's basic contention, namely, that crucial econometric relationships among monetary, financial, and real variables will tend to shift systematically, when policies or policy procedures are altered, unless great care is taken in the formulation and estimation of these relationships. This point is of substantial importance in the analysis of policy and policy implementation, and Walsh demonstrates it quite effectively. In addition, his paper includes several useful observations concerning previous research efforts. I have some reservations, however, about aspects of the specific applications appearing in Sections IV and V. My reasons for these **reactions** are explained in what follows.

Analysis of Borrowing Behavior

Walsh's basic point amounts, of course, to an application of the "Lucas critique" — so called because of the enormously influential exposition in Lucas (1976) — to the effects of changes in the Fed's operating procedures. It may therefore be useful to recall that the critique is widely agreed to be applicable wherever the econometric (or theoretical) relationships in question are defective in either of two ways. The first of these is the failure to take account of forecasting or expectational behavior on the part of rational individuals or firms, whose expectational parameters' will adjust when policies or procedures change since the latter will bring about changes in the dynamic,

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1. Parameters that appear in representations of expectations in terms of state variables observable to the agent.

stochastic behavior of variables that the agents take as exogenous and attempt to forecast. The second type of defect stems from a failure to express the relationships in terms of agents' fundamental objectives and constraints, because these are much more likely to be policy-invariant than are supply and demand **functions**.² As Walsh says, for an adequate response to the Lucas critique, typically "only preferences and technology are assumed to be policy invariant."

It is the second of these defects that leads Walsh to doubt the conclusions regarding operating procedures developed in a paper by James Hoehn and myself (McCallum and Hoehn, 1982). In that paper, in which we derive minimum mean-square money stock control errors under different operating procedures, we use a macroeconomic model that **fully** incorporates rational expectations but relies upon relationships of the supply-demand variety, not justified by explicit maximization analysis. I would agree with Walsh that the persuasiveness of our analysis is lessened by this aspect of the model.

As it happens, however, the model of intraweek interest rate determination presented in Walsh's Section IV is open to exactly the same criticism. In particular, an important component of the model is the set of equations relating bank borrowing in the three "days" of each week to current and expected end-of-week spreads between the federal funds rate and the discount rate [equations (3) and (3')]. Neither these equations, nor analogous ones describing security demands, are justified by analysis of maximization problems involving banks' objectives and constraints. Instead, the equations are simply posited as plausible and conventional relationships. Thus, just as in McCallum and Hoehn (1982), there is no compelling reason to believe that the parameters — or even the functional forms — would be policy-invariant.

In this respect, the analysis of bank borrowing behavior previously developed by Goodfriend (1981) goes somewhat farther than Walsh's in the direction suggested by the Lucas critique. In particular, Goodfriend poses explicit objective and cost functions for the representative bank, and uses these to derive decision rules. The precise specification is too simple — assumes away too many aspects of reality — to **form** the basis for an operational, empirically-implementable model. Nevertheless, it serves well to illustrate the point at hand — that changes in

2. This basic point is emphasized by Lucas and Sargent (1981) and by Sargent (1982), among others.

policies or procedures will systematically tend to shift the relationship between borrowing and the current spread.

A few brief additional comments on Walsh's model are warranted. First, its decomposition of the week into distinct subperiods is an interesting step that may prove fruitful in modeling reserve demand. But, second, a satisfactory model will clearly need to describe **inter-week** movements as well. Third, the formulation in (7) of the Fed's policy rule is rather awkward and implies an indeterminate price-level in the case with $\mu_1 = 0$. Finally, the assumption that excess reserves always equal zero would be inappropriate for the analysis of some feasible operating procedures.

Analysis of Money Demand

Let us now turn to Walsh's section V and his analysis of the effects of policy procedures on the parameters reflecting money-demand behavior. The money-demand function is derived in the context of a portfolio choice problem, with the representative demander depicted as holding only money and bonds. The second of these assets offers the holder a higher nominal return and neither asset provides transactions services, but money tends to be held nevertheless because the nominal return on bonds — the differential between the returns on bonds and money — is random. The implied money demand function is one in which the fraction of wealth held in the form of money is negatively related to the expected nominal rate of return on bonds. As the slope of this relationship depends upon the conditional variance of the nominal bond return, any policy action that affects this variance **will also** affect the slope of the money-demand function. Consequently, Walsh argues that "a change to a reserve aggregates operating procedure induces a shift in the money demand function. . . [that] tends to amplify the increase in interest rate volatility which would accompany a reserve aggregates policy" (p. 27).

While this argument is skillfully conducted, I find it rather unsatisfying to certain respects. The first and most important of these concerns the assumption, implicit in the model, that no "bonds" exist that are risk-free in nominal terms. Most monetary theorists would, I believe, accept the contrary judgement of Barro and Fischer (1976, pp. 139-140) that "there are assets, such as time deposits, that have precisely the same risk characteristics as money and yield higher returns. Accordingly, although the . . . portfolio framework has provided the basis

for significant advances in the field of finance . . . , it does not explain the demand for money."³ And if the main distinction between money and short-term, interest-bearing paper assets resides in their relative transactions-facilitating properties, not their risk characteristics, then it is unclear that a change in interest rate volatility will shift the parameters of the money demand function in the manner suggested by Walsh.

A second reservation concerning the argument involves its use in explaining the large increase in interest rate (and money stock) volatility that has been observed subsequent to the particular change in operating procedures that was effected on October 6, 1979. While it might be that some of the increased volatility has resulted from the type of parameter shift described by Walsh, the fact that an increase occurred does not provide highly convincing evidence in favor of the parameter-shift hypothesis since it is also predicted or explained in other ways. The model in McCallum and Hoehn (1982), for example, implies that an increase in money stock control errors will result from the adoption of a reserve aggregate instrument when lagged reserve requirements are in effect,⁴ and the same model suggests a large increase in interest rate volatility. These predictions could also be obtained from less explicit models and should not be surprising intuitively since the use of a reserve instrument in combination with lagged reserve requirements amounts to an indirect (and thus inherently error-ridden) method of using an interest rate instrument.

Another reservation involves the complete macroeconomic model developed in Walsh (1982b), which is used to provide a justification for some of the claims in Section V of the paper under discussion. The problem with this model is that it is not, as claimed, a bona-fide general equilibrium model. More specifically, the various behavioral relationships in that model are not derived by means of a unified analysis in which all agents maximize well-specified objective functions and all markets clear.⁵ While the portfolio balance relation is obtained from a

3. in an earlier paper (1982c), Walsh uses an overlapping generations framework to derive a money demand function similar to that of the present paper. The second asset (besides money) in this model is real capital, the return to which is random. If a risk-free interest-bearing bond were marketed by the government, no "money" would be demanded since it provides no transactions services. For an extended discussion of the role of overlapping-generations models in monetary economics, see McCallum (1983).

4. The qualifying clause should be emphasized; the model suggests that a reserve instrument would be likely to permit better monetary control than an interest rate instrument under a contemporaneous reserve requirements regime.

5. The meaning of the latter requirement will be discussed below.

maximizing analysis, the aggregate supply and demand relations are simply posited. Thus it is not demonstrated that the three relations are consistent with each other. Here, as in Section IV, Walsh stops short of a complete response to the Lucas critique — complete in the sense described by Lucas and Sargent (1981) or Sargent (1981).

General Comments

Having expressed several complaints or reservations about some details of Walsh's examples, let me now reiterate that I think the general theme of his analysis is not only correct but important. It is important, that is, to base policy on analysis using models that are designed to be policy-invariant. It simply makes no sense to do otherwise. In this regard I am compelled to say that I would agree with Walsh's suggestion that the Lucas critique is applicable to the Johannes-Rasche (1979) evidence concerning the accuracy of their procedure for monetary control. I am sorry to have to say that, for I happen to believe that their reserve-based procedure would in fact work very well, but the logic of the point is inescapable.

I would emphasize, however, that the point applies as well to all other existing analyses of which I am aware⁶ of the effects of different operating procedures — analyses both empirical and theoretical. And it certainly applies to policy analyses based on so-called "vector autoregressions" (VARs), a fact that I mention **because of** the prominent role of VARs in some recent discussions of policy [e.g., Friedman (1982) and Gordon and King (1982)]. To me, it is surprising that well-informed economists would at this date consider using **VARs for** policy purposes, since they are even less appropriate than the traditional econometric models discussed in Lucas's critique. From the papers in question, moreover, it appears that the basic defense for using the VAR procedures in this way is that they were developed by a brilliant econometrician, Christopher Sims. But of course that fact provides no logical justification at all. And Sims's (1982) own recent emphasis on the fact that most "policy" **actions** do not constitute changes in **policies** — i.e., policy rules or regimes — does nothing to validate the use of the method (which measures the effects of isolated actions) for predicting the effects of changes in policies.

Since I have applauded Walsh's progress in terms of the Lucas-Sargent program for developing policy-invariant models, and have

6. Including those in the Federal Reserve System Staff Study (1981).

criticized him primarily for not progressing more rapidly, some final comments about that program are in order. The first thing that needs to be said about the methodology is that, despite its emphasis on competitive general equilibrium theory, it does not **carry** any implication that monetary policy is necessarily "ineffective" in the sense in which that term has been used in the rational expectations literature (McCallum 1979, 1980). Second, recommending the use of equilibrium models is not the same as asserting that the behavior of the economy is well-described by *flexible-price* equilibrium models. As Taylor's (1982) paper for this conference points out, these models are difficult to reconcile with the data. What is needed is an extended equilibrium analysis that explains the existence and nature of nominal contracts and thus predicts how they will respond to changes in policy.⁷ Third, the mere step of writing down an explicit optimizing model is (obviously) not a guarantee of success. If the model includes a poorly-specified objective function or constraint, it will be a poor model, explicitness notwithstanding. The virtue of the equilibrium-analysis program is that it involves a particular form of analytical discipline, *i.e.*, it encourages one to think carefully about the behavior of individual agents and about the way in which the actions of many such agents interact. This discipline is valuable, and Walsh's paper should be commended for **trying** to bring more of it to the consideration of alternative operating procedures.

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7. As the development of models of this type will be extremely difficult, one possible reaction by policymakers might be to abandon the whole attempt and **formulate** policy without reliance on *any* model or models. Such a reaction would reflect, I think, a fundamental misunderstanding. A model is; after all, nothing but a coherent analytical framework designed to facilitate thinking about some complex system. The only alternative to using a model of *some* type is to be incoherent. The actual issue relevant to the attitude in question is whether to use formal or informal models. The main advantage to a conscientious policymaker of formal, explicit models is that they enable the user (and others) to search for flaws — *i.e.*, logical inconsistencies and counterfactual assumptions.

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