Identification and Estimation of Money Demand

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In most natural sciences (physics, chemistry, biology) theories are validated by controlled experiment. However, in other natural sciences (astronomy, meteorology), and in most social sciences, including economics, the data are characteristically generated not by experiment but by measurement of uncontrolled systems. In economics, theories take the form of restrictions on the models assumed to generate the data, and statistical methods replace experimental controls in testing these restrictions. And here is the difficulty: in economics, particularly macroeconomics, the theory used to derive tests ordinarily does not generate a complete specification of which variables are to be held constant when statistical tests are performed on the relation between the dependent variable and the independent variables of primary interest. Accordingly, in such cases there will be a set of often very different candidate regression-based tests, each of which has equal status with the others since each is based on a different projection of the same underlying multivariate model. Except in the unlikely event that the explanatory variables are mutually orthogonal, the conditional regression coefficients, which generally form the basis for the test statistic, will depend on the conditioning set. We conclude from this that, if a theory which does not generate a complete specification of the regression test is nonetheless to have testable implications, these implications must be robust over the permissible alternative specifications. If the restrictions indicated by the theory are satisfied in some projections, but not in others that have an equal claim to represent implications of the theory, one cannot conclude that the theory has been confirmed.

The fact that the observable implications of valid theories must obtain over a broad (but usually incompletely specified) set of regressions rather than for a single regression introduces a large and unavoidable element of imprecision into hypothesis testing in macroeconomics. Generally it appears to be appropriate to weaken the statistical criterion for rejecting theories. Consider, for example, the theory of money demand, which will engage our attention in this paper. The Tobin-Baumol square root formula implies that the elasticity of money demand with respect to the interest rate is exactly one-half. But which interest rate? Should wealth be held constant? Inflation? In view of such uncertainties it would be inappropriate to insist in a literal-minded fashion on rejecting the Tobin-Baumol model if in some regression the measured interest elasticity differed from one-half by more than two standard deviations, and only then. Obviously a more flexible approach is called for. The practice has been to conclude that the statistical evidence is consistent with the Tobin-Baumol model as long as the interest rate coefficient is negative. If it is negative and significant, or negative and insignificantly different from minus one-half, that would provide somewhat stronger confirmation. But a positive coefficient, particularly a significantly positive coefficient, would be viewed as raising questions about the validity of the theory. In macroeconomics generally, as in the money demand application, the typical response to specification uncertainty has been to regard a theory as supported if the signs of the estimated coefficients agree with those expected from theory, and as disconfirmed otherwise. There is no theoretical justification for this procedure, but it seems to be a reasonable course to follow.

The point that economic theory ordinarily generates incompletely specified statistical

*University of California, Santa Barbara. We have received helpful comments from Andrew Abel, Robert Clower, Michael Darby, Robert Engle, Stephen Goldfeld, David Laidler, Edward Leamer, Robert Lucas, Frederic Mishkin, and Edward Prescott. Thomas Hall provided able research assistance.

tests, and that therefore conclusions based on empirical tests must be robust to at least some respecifications, is obvious when stated directly and in abstract form. But consider its implications when combined with some equally well-known facts about the sociology of scientific research. In any science the rewards go disproportionately to the researcher who proposes a new theory which is empirically confirmed, and not to the analyst who proposes, tests, and rejects another equally appealing theory. This bias is an inevitable consequence of the fact that plausible but wrong theories are easier to come upon than new, correct and important theories. But the bias has the consequence that incentives are created for the scientist to become an advocate for his theory, presenting all the evidence he can find in its favor and leaving to others the task of mobilizing contrary evidence.[1]

To some extent the search of the scientific community for adequate explanation may be likened to that of the legal community for justice: in a court case the lawyers on each side are understood to be engaged in special pleading, subject only to minimal requirements prohibiting perjury, and so forth. For the most part in the natural sciences the fact that scientists act as advocates does not seriously impede communication since there is little latitude for selectivity in reporting experimental evidence, short of outright falsification. But in economics the advocacy nature of scientific work creates special problems, in part because macroeconomic theory does not generate completely specified empirical tests corresponding to the controlled experiments of the natural sciences. Acting as an advocate for his theory, the economist conducting an empirical study is motivated to examine all or a large subset of the many possible regressions constituting tests of his theory and to report only those results most favorable to the theory. Often significance tests and fit statistics are conducted and interpreted as if the reported regression constituted the one statistical test unambiguously implied by theory, even though both researcher and reader are aware that the conventional tests are invalid if the reported regression is in fact the outcome of a specification search. The reader, of course, knowing that a specification search underlies the reported tests, discounts heavily or completely the researcher's claims for validation of a theory for which he is obviously acting as an advocate. At best, the reader holds in abeyance his evaluation of the researcher's claims until he can conduct his own test for robustness by varying the regression specification over what seem to him to be equally acceptable tests of the theory and determining the degree to which the outcome of the test conducted by the researcher is affected by such respecification. Particularly in macroeconomics, therefore, we often have what is very nearly a zero-communication information equilibrium. The researcher has the motive and opportunity to represent his results selectively, and the reader, knowing this, imputes a low or zero signal-to-noise ratio to the reported results.

We do not suggest that the advocacy system is the culprit here. Legal scholars do not believe that justice would be better served if lawyers sacrificed their clients' interests in favor of their own perceptions of justice, just as most economists do not believe that corporate executives would in fact serve the public welfare by subordinating to their conception of it their obligations to shareholders. Similarly, we see little point in urging that researchers in economics forswear their roles as advocates. Rather, we believe that what is needed is a form of communica-

[1]As is well known, economics journals strongly reinforce the advocacy element in communication among researchers by publishing negative results rarely, and fully reported specification searches almost never. The few exceptions to this rule are generally papers authored by scholars of established reputation (in money demand theory, see for example the papers of Stephen Goldfeld, 1973; 1976, and David Laidler, 1980). For these an exception is made, presumably because of the widespread respect for their judgment and ability to conduct a specification search that will be of general interest. One may or may not agree with the details of the specification search reported in such papers as Goldfeld (1973) (see the discussion below); however, the point here is that such papers provide a more complete report of specification searching than is commonly the case, and the reader is therefore in a better position to determine for himself whether he shares the author's conclusions. But for the majority of papers reported in the journals, no such independent judgment is possible.

tion that is less amenable to selective reporting, so that economists, still acting as advocates, can conduct and report tests in a way that will not be so highly discounted by readers.

Gary Chamberlain and Edward Leamer and Leamer's book have developed analytical results consistent with such a reporting style, and Leamer and Herman Leonard have discussed how they can be used to achieve more effective communication. Leamer's idea is to represent specification uncertainty by dividing the explanatory variables in a regression into two classes: the "focus variable" and the "doubtful variables." The researcher's purpose is assumed to be to estimate or perform tests on the coefficient of the focus variable. However, the researcher is assumed to be a priori uncertain about whether the effect of other variables should or should not be controlled, and he wishes to ascertain the sensitivity of the estimated focus coefficient to the inclusion or noninclusion of such doubtful variables in the regression. Accordingly, Leamer proposed reporting the extreme values of the focus coefficient over a suitably defined region of the parameter space, where the parameter space includes the coefficients of both focus and doubtful variables. Such a reporting procedure, which would replace the prevailing practice of selective reporting of the results of a specification search, would allow the researcher to demonstrate the robustness of his conclusions with respect to a clearly defined set of respecifications. Accordingly, it offers some prospect of improving the informativeness of communication among researchers in macroeconomics.

Suppose that we conduct an empirical investigation in the manner recommended by Leamer, and assume that it is concluded that the parameter restrictions implied by the theory do not appear to be satisfied in the majority of the conducted regression tests. In that case one of two conclusions must follow. The first, of course, is that the theory is incorrect. The second possible explanation, which will occupy us in this paper, is that least squares projections do not provide a suitable analogue to the controlled experiments of the natural sciences. A number of econometric problems may lead to such failure of experimental design, but that which will interest us primarily is simultaneity. If simultaneous equations problems are present, the estimated coefficients of least squares projections do not correspond to the structural parameters which are restricted by the theory. Consequently, the empirical acceptance or rejection of these restrictions has no implication for the theory. In any particular case, the judgment as to whether empirical rejection of the theory is due to the incorrectness of the theory or to the existence of simultaneous equations (or other statistical) problems will depend on the strength of the researcher's prior belief in the correctness of the theory relative to that of his prior belief in the validity of the least squares specification: if the researcher is certain that the least squares specification is acceptable, he will conclude from his empirical results that the theory being tested is false, while if he is certain that the theory is correct he will conclude that simultaneity problems are present and serious.

In this paper we consider the application of these ideas to the estimation of models of money demand. We will take it as an implication of theory that the demand for money depends on an interest rate, representing the opportunity cost of holding real balances, and a measure of transactions. Further, we will assume without discussion that theory implies a negative coefficient for the interest rate no matter what measure of transactions is used, and no matter what other variables are included in the regression. Thus we obtain the robustness required in view of the fact that theory does not specify precisely which variables do and do not enter the money demand regression. We wish to test this restriction empirically, and to obtain an estimate of the interest elasticity of money demand.

It is noted in Section I that the majority of empirical studies of money demand have in fact reported success in estimating a significant negative coefficient for the interest rate. But we strongly suspect that many of these studies are based on selective reporting of the results of a specification search, leading us to discount the reports of successful testing and implementation of the theory pending some demonstration of robustness. In Section II we review Leamer's algorithm, preparatory to using it to provide the required appraisal of the robustness of the estimated interest rate coefficient to alternative specifications. Leamer's procedure is implemented in Section III. We take the Treasury bill rate and the savings and loan passbook rate as the focus variables, and include as doubtful variables a variety of other variables frequently appearing in estimated money demand models in the reported literature. It is concluded that the estimated interest elasticity is extremely sensitive to the inclusion or noninclusion of the doubtful variables, is not clearly negative, and is in any case much closer to zero than is indicated in much of the reported literature. It follows that the results reported in the literature are not in fact robust, reflecting instead highly selective reporting of a specification search that converges toward regions of the parameter space that contain "significant" negative estimated interest elasticities. Our results suggest that researchers who report successful empirical testing of the restrictions implied by theory do so primarily by building in their conclusions, and that in fact the data do not provide confirmation for the theory.

As indicated above, our failure to confirm the theory motivates a reexamination both of the economic theory being tested and of the econometric assumptions underlying the test itself. We follow only the second of these lines in this paper.[2] In Section IV the treatment in the existing literature of simultaneity problems in estimating the demand for money is reviewed. It is found that such discussions, which are extremely perfunctory and superficial, amount to dismissing the problem without serious analysis. We view this as a consequence of the fact that thoughtful discussion of simultaneity problems has no role to play in the process of persuasion, particularly if the conclusion is that such problems are not minor. Accordingly, to the extent that researchers act as advocates for a particular conclusion based on least squares estimation, they have strong motivation to dismiss simultaneity problems as unimportant or to omit discussion of such problems altogether. Further, investigators can always suppress estimated regressions which give evidence of simultaneity problems—principally wrong signs—in their published reports, thereby avoiding the need to confront the issue. Finding that the discussion of simultaneity problems in the established literature is inadequate, we are led in Section V to inquire whether such problems can be resolved by any method not discussed in the literature. After considering and rejecting several plausible possibilities, we conclude that we are unable to devise a statistical procedure that will identify a demand relation.

[2]In this connection, it is worth noting that Robert Clower and Peter Howitt have argued that the comparative statics properties of the Tobin-Baumol model do not carry over to more general settings.

I. The Demand for Money

The consensus account of the theory of the demand for money has changed little in the last forty years. In David Laidler's words: "The picture that emerges from the preceding brief [summary of the literature] is one of steady if unspectacular progress, of a gradual refinement of our understanding of the properties of the demand-for-money function, and of a considerable broadening of the empirical basis of that understanding" (1980, p. 223). In contemporary formulations, just as in Keynes's *General Theory*, the demand for real money is assumed to depend negatively on a short-term interest rate, representing a proxy for the opportunity cost of holding money, and positively (and sometimes proportionally) on a transactions measure such as real *GNP*. The absence of fundamental development in the theory of money demand is at least partly due to the fact that empirical studies have for the most part reported exceptional success in testing and otherwise implementing the received theory, implying no need for reexamination of the model. At least until the recent "missing money" episode, most studies have concluded that the money demand equation is stable, that the estimated coefficient of correlation is very high, that the estimated regression coefficients have the indicated signs and approximately the magnitudes expected from theory, and that sampling error in coefficient estimation is acceptably small. As Laidler put it, "... the frequency with which a positive real income (or wealth) elasticity of demand for money, a negative opportunity cost elasticity, and a unit price level elasticity of demand for money have been found to be well determined is quite remarkable" (1980, p. 221). Elsewhere Laidler observed that "There is an overwhelming body of evidence in favor of the proposition that the demand for money is negatively related to the rate of interest. Of all the issues in monetary economics, this is the one that appears to have been settled most decisively" (1977, p. 130). Given the consonance of the theoretical and empirical evidence on the fundamentals of money demand, most empirical investigators have felt justified in proceeding to explore elements of the regression specification which are of secondary importance. Bearing in mind the development presented in the introduction, however, we are unwilling to take entirely at face value the striking agreement reported in the literature between theory and evidence, particularly in view of the high degree of consensus on the underlying theory. With regard to the coefficient of the interest rate, is one to conclude that the evidence in fact strongly confirms the prediction from theory that it is negative? Or is it a fact that researchers have acted as advocates for the consensus theory by conducting specification searches and reporting only the evidence consistent with their prior belief in the correctness of the theory? As indicated in the introduction, our view is that the second case is closer to the truth than the first.

A convincing argument for our view cannot be based on the existing literature since our evidence must consist primarily of what is excluded from published articles rather than what is included. However, some prima facie evidence is available from the literature. If, as we argue, the conclusions reported in empirical studies of the demand for money reflect prior beliefs more than sample evidence, then differences among authors in conclusions should be traceable to identifiable differences in priors. Of course, the existence of a generally accepted consensus on the theory of money demand means that such an association is difficult to document since there exist so few differences in priors. Nonetheless, such differences in conclusions as do exist turn out to bear a pronounced correlation with differences in priors.

For Keynesians, a negative estimated interest elasticity is an absolute necessity, since otherwise the comparative statics properties of *IS-LM* type models may be reversed.[3] Monetarists, on the other hand, do not regard bond interest rates as the only, or even the most important, variable linking monetary changes with expenditures. Consequently, the issue of the interest rate elasticity is not of primary importance to monetarists. Compare, for example, Milton Friedman: "In my opinion no 'fundamental issues' in either monetary theory or monetary policy hinge on whether the estimated elasticity can for most purposes be approximated by zero or is better approximated by $-.1$ or $-.5$ or -2.0, provided it is seldom capable of being approximated by $-\infty$" (1969a, p. 155). Also, monetarist doctrine stresses the dependence of nominal interest rates on expected inflation. Consequently, monetarists could rationalize a positive association between changes in the money stock and changes in interest rates as reflecting the effect of monetary changes on expected inflation. It follows that monetarists will be much more willing than Keynesians to maintain an agnostic attitude, letting the data tell their own story, or alternatively to conclude that aggregate data do not give much information about the interest rate elasticity.

What do we find when we look at the evidence? The majority of studies of money demand were performed by economists who accept the Keynesian orthodoxy, and in these studies it is invariably found that, as expected, the coefficient of the interest rate is indeed negative and significant. To our

[3]For example, if the *LM* curve is negatively sloped and steeper than the *IS* curve, an increase in government spending will decrease rather than increase *GNP*.

knowledge the authors of only three of the major studies of money demand reported that they were unable to isolate a significant interest rate effect. These, significantly, are Friedman (1969b; first published 1959), Laidler and Michael Parkin, and Sam Peltzman, all monetarists.[4] It is true that both Friedman and Laidler subsequently succeeded in finding the usual negative coefficient (Friedman, 1969a, first published 1966; Laidler, 1977), but this only confirms our point that the monetarist view of the transmission mechanism implies that monetarists are likely to be much more willing than Keynesians to reverse their field on the question of the interest rate elasticity if the evidence appears to require it.

Rational expectations macroeconomic models provide another case in point. In these models, money affects aggregate real activity because market participants are by assumption unable to distinguish between nominal and real shocks. Interest rates, therefore, are not directly involved in the linkage between monetary shocks and changes in real activity (however, it could be argued that, in such rational expectations models as that of Robert Lucas, interest rates are implicitly involved). Consequently, researchers associated with the rational expectations tradition, like monetarists but unlike Keynesians, would not be impelled to find a negative relation between monetary changes and interest rate changes. And, indeed, Frederic Mishkin reported that "many different empirical tests [which he conducted]... uniformly *do not support* the proposition that increases in the money stock are correlated with declines in short rates" (emphasis in text). It is seen, then, that economists who subscribe to the view that the interest elasticity of money demand is negative and significant are likely to be those who think about macroeconomics in terms of models of the *IS-LM* type, while economists who do not see in the data a clear structural relation between money and interest rates tend also not to rely on such a relationship in their thinking about business fluctuations. Unless this association is to be viewed as coincidence, it appears to follow that prior beliefs play a more important role in generating posterior conclusions than is commonly acknowledged.

The most convincing evidence of the role of specification uncertainty and specification search in facilitating the combination of prior with sample information is found not by comparing the results of researchers with different priors, but by examining the informal discussion of provisional results by a given researcher, particularly in papers in which the specification search is fully reported. Generally in the money demand literature the practice is to conduct hundreds of regressions in batches of several dozen regressions each.[5] At each stage the regressions with wrong signs are discarded, and those most consistent with the researcher's preferences are interpreted and used as the basis for the next series of regressions. Much reliance is placed on estimated fit statistics in selecting the most promising regressions, although the discussion here is generally circumspect since it is well known that these statistics have no clear interpretation when the statistics themselves provide the principal guide for the specification search. Throughout the researcher relies heavily on his prior information as to what constitute "reasonable" and "plausible" specifications, which views the reader is presumed to share. Thus, the existence of considerable specification uncertainty, as in the demand for money, in practice has the effect of increasing the precision and detail of reported conclusions since it gives the researcher wide latitude for a specification search, which in turn facilitates the imposition of highly detailed prior information. Obviously the appropriate consequence of specification uncertainty would be just the opposite: to decrease the scope of the in-

[4]Friedman (1969a, p. 142, fn. 1) subsequently pointed out that in (1969b) he had concluded only that he was unable "to find any close connection between changes in velocity and any of a number of interest rates," not that a zero value could definitely be assigned to the interest elasticity.

[5]A coauthor of one of the major studies of money demand estimates that he ran more than 500 regressions in preparing his article.

ferences which can with confidence be drawn from the data.

Stephen Goldfeld's 1973 paper provides an excellent example of the infusion of extensive prior information into money demand estimation via a guided specification search. It is worth emphasizing that we single out Goldfeld's paper only because he reported the specification search more extensively than is usual; in much of the literature the same type of specification search is conducted, but only the end product is reported. Also, Goldfeld's paper is the most frequently cited of the recent empirical studies of money demand and appears to have established the precedent, followed in much of the subsequent literature, of reporting scores of regressions (in Goldfeld's case, over 70) rather than a dozen or so as formerly. We present two examples of what we believe to be Goldfeld's propensity to overinterpret the data and to use specification uncertainty as a means to impose highly articulated prior information on the data. At one point (pp. 598–607) Goldfeld wished to ascertain whether a Koyck lag with separate provision for autocorrelation (via the Cochrane-Orcutt technique) adequately represents the dynamic effect of lagged explanatory variables on money demand. To make this determination he reestimated the demand for money (characteristically, the experiment was repeated for currency and demand deposits separately) by using the Almon technique in place of the Koyck transformation. As with the Koyck specification, the Cochrane-Orcutt procedure was used to correct for serial correlation of the errors. Inasmuch as both regressions yielded a coefficient of correlation of 0.995, and since the estimated lag structures were only slightly different (they will, of course, be exactly identical with probability zero), it would seem that there is nothing to choose between the two specifications. Goldfeld's interpretation was different:

> The Almon responses to income changes are uniformly slower than the Koyck responses. For interest rates, the Almon response is slower for several quarters but then overtakes the Koyck response. Evidently, constraining all the responses to the same shape in the Koyck version produces an inappropriate average response which masks individual differences... . In summary, a modest amount of evidence suggests that the Koyck formulation of equation (4) is a bit too restrictive. [p. 607]

Although this conclusion is carefully modulated, and is then qualified by a call for additional research on the subject, it still appears to us to constitute drastic overinterpretation of regression results. For another example, consider Goldfeld's discussion of the long-run income elasticity of money demand (pp. 583–89). The estimated income elasticity is 0.68; the standard error of estimate was 0.00432 and the root mean-squared error based on a dynamic within-sample simulation was 0.0093. With the elasticity constrained to unity, the corresponding figures were 0.00457 and 0.0112. Based on these differences (and, it is true, also on a more pronounced deterioration in out-of-sample performance under the constrained version), Goldfeld concluded that "the results seem to suggest that the relevant income elasticity... is significantly less than unity" (p. 589). We believe that given the extent of specification uncertainty in the demand for money it is inappropriate to discriminate between alternative equations on the basis of such small differences in the quality of fit.

It is obvious that when an intensive specification search is employed to isolate a money demand equation which fits the sample period (and possibly also tracks the data for a few quarters beyond the sample period), a marked deterioration in performance should not be surprising when the equation is applied to data other than those used to fit the equation and conduct the specification search. That this has been the case with the equations which Goldfeld (and everyone else) estimated in the early 1970's is well known. In 1974–76 empirical money demand equations of the received variety overpredicted the money stock by a large margin. That the misses should be statistically significant according to the standard error of estimate of the preferred equation is hardly surprising in view of the fact that the *SEE* of the preferred equation is strongly biased toward zero by the specification search. In our view an appropriate lesson to be learned from the "missing money" episode would have been that it is pointless to conduct a specification search in the manner of Goldfeld and to continue to ignore issues of identification and simultaneity. However, Goldfeld and others instead took at face value the evidence that a statistically significant shift in the money demand equation had taken place, and were therefore led to conduct yet another search for an equation that would fit the more recent data and otherwise display satisfactory characteristics (see Goldfeld, 1976, and also Jared Enzler, Lewis Johnson, and John Paulus).[6]

Instances of overinterpretation of regression results similar to those of Goldfeld could be multiplied indefinitely from virtually any of the recent empirical analyses of money demand, but there seems little point in doing so here. Evidently the problem is that the presence of specification uncertainty has induced analysts of money demand to make extensive but completely informal use of prior information. The unsystematic nature of this combining of prior and sample information makes it virtually impossible for the reader to specify what prior beliefs he has to agree to in order to assent to the conclusions of the empirical exercise. Further, it is difficult for the analyst with priors which differ from those of the author of an article to extract useful information from the reported results. Obviously what is required is a more formal and explicit means of representing prior information, or the lack of it, about model specification. Leamer's analytical procedure and reporting style are one way of fulfilling this requirement.

II. Extreme Value Analysis

Consider the regression

$$y_t = x_t\beta + \sum_{i=1}^{k} z_{it}\gamma_i + u_t \tag{1}$$

and suppose that our primary interest is in estimating β, the coefficient of the "focus variable" x_t. Specification uncertainty is reflected by the inclusion of the k "doubtful variables" z_{it}. The researcher is not certain that these should be included as conditioning variables, but is unwilling to exclude them a priori. If the researcher is also uncertain that linearity is the appropriate functional form, the z_{it} may be specified to include higher-order or interaction terms. When there are k uncertain variables, 2^k regressions are defined by the inclusion or noninclusion of some or all of the doubtful variables. If k is at all large, only a small subset of these can feasibly be examined by the researcher, and the regressions reported to the public are generally a still smaller subset. But the 2^k regressions defined by on-off combinations of the doubtful variables (i.e., regressions in which each doubtful variable either receives equal billing with the focus variable or is excluded altogether) obviously constitute a subset of a broader class consisting of those regressions which incorporate general linear restrictions among the doubtful variables. Aside from convenience, there is no reason whatever why

[6]We are unable to supply an analysis of the missing money episode because, as indicated in the introduction, we do not know how to estimate structural money demand equations consistently. However, our perspective suggests two observations. First, as indicated in the text, it is not clear that a statistically significant shift in fact occurred. Second, even if such a shift did occur it is not evident that it was demand that shifted rather than supply, or both demand and supply (the arguments of Goldfeld, 1976, and Enzler, Johnson, and Paulus that the shift was one in demand require the maintained assumption that a structural demand equation for the earlier period had been satisfactorily estimated).

In our view it may be more than coincidental that monetarist views became more influential than formerly in the Federal Reserve System around the time of the missing money episode, and it is certainly the case that among the public, monetarist interpretations of Federal Reserve policy became more widespread during those years. If a change in the Federal Reserve's response function was believed by the public to have occurred, a rational expectations argument establishes the presumption of a consequent shift in demand. Apart from the rational expectations argument, however, even if no shift in structural demand actually occurred it is still likely that if a structural supply shift occurred it might be imputed to demand, given the correctness of our contention that estimated money demand equations are probably contaminated by supply equations. We have no way to ascertain the validity of such conjectures, however.

attention should be limited to the less general class of regressions.

A formal Bayesian approach in which beliefs about the doubtful variables were represented by a well-defined prior distribution with the prior location and covariance matrix specified would lead to a well-defined posterior distribution for β. The problem is that while it is usually easy to specify the prior location, it is generally difficult to specify the covariance matrix. The approach used in this paper is based on a result by Chamberlain and Leamer, further elaborated in Leamer, which obviates the need for specification of the prior covariance matrix. The theorem is that specification of the prior location and the sample covariance matrix is sufficient to constrain the posterior means to lie within a particular ellipsoid, the "locus of constrained estimates."[7] To understand intuitively the meaning of the locus of constrained estimates, consider the special case of equation (1) in which there are two doubtful variables:

$$(2) \qquad y_t = x_t\beta + z_{1t}\gamma_1 + z_{2t}\gamma_2 + u_t.$$

We can define a composite variable,

$$w_t(\theta) = z_{1t} + \theta z_{2t},$$

and rewrite equation (2) as

$$y_t = x_t\beta + w_t(\theta)\gamma + u_t.$$

For suitably defined θ this regression coincides with any of the four regressions defined by including both doubtful variables, deleting one or the other, or deleting both. These four points are indicated by $(\hat{\gamma}_1, \hat{\gamma}_2)$, $(0, \hat{\gamma}_2)$, $(\hat{\gamma}_1, 0)$, and the origin in Figure 1. But, as already noted, there is no reason to restrict attention to these four regressions. For each value of θ there is a different regression and corresponding estimate of the focus coefficient $\hat{\beta}(\theta)$. The set of all possible values of (γ_1, γ_2) generated by varying θ over the real line will define the ellipse of constrained estimates, indicated by the dashed line in Figure 1. The significance of this ellipse is that it contains all possible posterior means for the distribution of (γ_1, γ_2) that result from the integration of a prior distribution centered at the origin with the sample (i.e., if the covariance matrix of the prior distribution is varied over all possible 2×2 positive definite or semidefinite matrices, the resulting set of posterior means for (γ_1, γ_2) is just the ellipse of constrained estimates and the points within it).

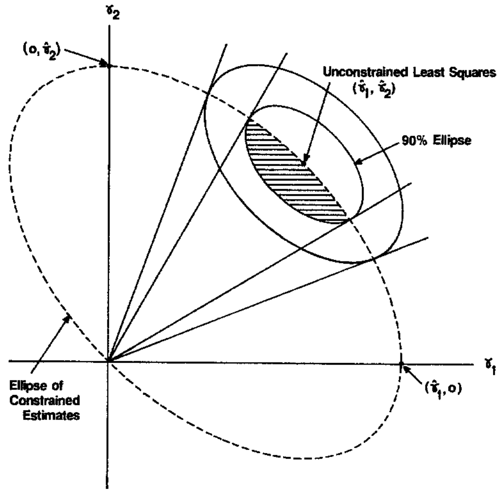

FIGURE 1. ELLIPSOIDS OF CONSTRAINED LEAST SQUARES POINTS

One measure of specification uncertainty is obtained by calculating the extreme values of $\hat{\beta}(\theta)$ on the locus of constrained estimates. If the difference between the extreme values is large relative to the sampling uncertainty, the implication is that uncertainty in model specification is a major contributor (relative to sampling variance) to the overall uncertainty about the value of the focus coefficient. The appeal of this estimation procedure lies in the fact that, although it is Bayesian in spirit, it does not require the analyst to define the relative strength of his prior belief that the coefficient of each of the doubtful variables equals zero; rather, the idea is to evaluate specification uncertainty by searching for the extreme values of the

[7]Specifically, in the regression model $y = x\beta + u$ with sample estimate and prior location b and b_0, respectively, and sample precision matrix H, the mean of the posterior distribution is constrained to lie in the ellipsoid $(b-b_0)'H(b-b_0)/4$.

estimated focus coefficient that can occur over all possible prior covariance matrices.

The major shortcoming of the measure of uncertainty just described is that the extreme values on the locus of constrained estimates may occur at locations in the parameter space which are extremely unlikely in view of the data. To meet this objection, Leamer proposed a related measure of specification uncertainty, one that we will employ. Consider the set of points $(\hat{\gamma}_1, \hat{\gamma}_2)$ defined by the intersection of the points in the interior of the locus of constrained estimates and, say, the 90 percent likelihood ellipse. This set is indicated by the shaded region in Figure 1. The interpretation is that parameter pairs in the region just defined represent all the possible parameter pairs that could be generated as posterior estimates from some prior distribution centered at the origin, subject to the further constraint that only points in the 90 percent likelihood ellipsoid be considered. The 90 percent ellipsoid is generated in the usual way under the assumption that all doubtful variables enter the regression. The measure of specification uncertainty is simply the difference between the extreme values of the estimated focus coefficient over this region. There is, of course, no reason to look at the 90 percent ellipsoid rather than any other; consequently, we will display the extreme values of the focus coefficient over a sequence of likelihood ellipsoids, the idea being to evaluate how specification uncertainty varies for different possible relative weights on the sampling and prior distributions.

III. Empirical Implementation[8]

The equation we consider first relates the demand for real money (M1) to a measure of transactions and to two interest rate variables, the savings and loan passbook rate (RSL) and the ninety-day Treasury bill rate (RTB). Our prior belief (as in, for example, Franco Modigliani, Robert Rasche, and J. Philip Cooper) is that the long-run income elasticity of the demand for money is unity. We estimate the equation in log-linear form, both because in doing so we follow precedent and because the resulting coefficients have an immediate interpretation in terms of elasticities. The data are quarterly and the observation interval is 1952.2 to 1978.4. The equation corresponding to our prior beliefs, in which all the doubtful variables are excluded, would then be estimated by regressing the *log* of the real money stock (M1) less the *log* of real *GNP* on the *log* of the ninety-day Treasury bill rate and (or) the savings and loan passbook rate. There exists a variety of additional variables which may influence the demand for money in addition to, or as alternatives for, those variables already specified. Under the usual estimation procedure as described in Section I, various subsets of these variables would be included in the specification search; here, on the other hand, we label them "doubtful variables" and treat them in the manner outlined in the preceding section. We do not contend that our list of doubtful variables exhausts the set of variables entered in money demand equations in all the studies extant, although we have tried to include those which have received major attention in the most widely cited articles. Expansion of the set of doubtful variables would only increase the estimated extreme values for the interest rate coefficient and therefore strengthen our conclusion. Our set of doubtful variables is listed and briefly described in Table 1. All were entered in *log* form.

Least squares applied to the basic equation with all doubtful variables (except the lagged terms) included yields the following estimates (standard errors in parentheses)

$$(3)\quad ln(M_t/P_t) - ln(GNP_t/P_t) = -\underset{(.813)}{1.818}$$
$$+\underset{(.011)}{0.010}\cdot ln\, RTB_t - \underset{(.069)}{0.175}\, ln\, RSL_t$$
$$-\underset{(.167)}{0.036}\, ln(\dot{P}_t/P_t) - \underset{(.081)}{0.628}\, ln(GNP_t/P_t)$$
$$-\underset{(.055)}{0.009}\, ln\, VCC_t + \underset{(.096)}{0.107}\, ln\, W_t.$$
$$R^2 = .985;\ SEE = .028.$$

[8]The program used in the empirical work reported in this section was SEARCH, developed by Leamer and Leonard.

TABLE 1—DOUBTFUL VARIABLES

1. *Real GNP*. Real *GNP* (i.e., nominal *GNP* divided by the *GNP* deflator) was entered as a doubtful variable to allow for the possibility that the income elasticity of money demand is greater than or less than unity.
2. The current *inflation rate* ($\dot{P}/P$) (the rate of change of the *GNP* deflator) was entered as a doubtful variable; this variable could be excluded on prior grounds if we were certain that financial assets are the only relevant alternatives to money, since in that case the inflation rate would be relevant only insofar as it affects nominal interest rates, which are already included as explanatory variables. However, we wish also to allow for the possibility that commodity inventories are relevant alternatives to fixed yield financial assets, in which case there may be a role for the inflation rate independent of nominal interest rates.
3. The real value of *credit card transactions* (*VCC*) was included to capture the possible negative effect of increased credit card use on money demand.
4. *Real Wealth* (*W*) was included on the grounds that some discussions of the demand for money (see, for example, Friedman, 1969b) emphasize wealth as a major determinant of money demand. Our measure is that used in the MIT-Penn-SSRC econometric model.
5. As indicated in the text, *lagged values* of the above variables are included in one specification together with lagged values of the focus variables to allow for lagged adjustment to the equilibrium.

Thus, if all doubtful variables are included the estimated interest elasticity (the sum of the coefficients of *RTB* and *RSL*) is -0.165 and the interval -0.013 to -0.317 (i.e., -0.165 ± 0.152, the sampling uncertainty of the sum of the coefficients of *RTB* and *RSL*) would include the true value with 95 percent probability. These particular estimates would not be of much interest to most investigators since it is obvious that even a rudimentary specification search would produce an equation deemed more worthy of reporting (for example, the coefficients of most of the explanatory variables in (3) are not significantly different from zero). Equation (3) is, of course, not relevant to our analysis since it makes no use of our prior belief that the doubtful variables may be excludable from the regression.

In Table 2, Specification 1 presents the extreme bounds on the sum of the coefficients of the focus variables *RTB* and *RSL* for various values of the data likelihood. The difference between the extreme bounds is a measure of the uncertainty in the interest elasticity of the demand for money that results from uncertainty about the specification. Clearly the specification uncertainty is very large compared to the sampling uncertainty (0.152), and the bounds include positive values for most values of the data confidence.

One of the problems that confronts all empirical studies of money demand is the existence of collinearity among interest rates. In several studies it is concluded that collinearity among interest rates is sufficiently great that little explanatory power is lost if only one interest rate is entered in the regression. It is therefore useful to estimate bounds on the interest elasticity when only one interest rate is included as a focus variable. Accordingly, in Specification 2 we deleted *RSL* from the model. The list of doubtful variables is assumed unchanged. Interestingly, when *RSL* is excluded the extreme bounds of the coefficient of *RTB* become quite narrow except at the prior, and they closely bracket zero for virtually all values of the data confidence. This suggests that one reason why most studies of money demand include more than one interest rate is that only by so doing does the feasible parameter space become sufficiently large that researchers can find a specification that confirms their prior belief of a significant and negative interest rate effect in money demand.[9]

The two specifications considered above were both static, including only contemporaneous values of both focus and doubtful variables. Much of the literature has placed a strong emphasis on the importance of dynamic behavior in money demand, and a

[9]Many studies report intensive searches to find the "right" interest rate or the best combination of interest rates. A frequent contender is ninety-day commercial paper rate (*RCP*). We have also calculated the extreme bounds on the interest elasticity for specifications in which *RCP* is the focus variable and for specifications in which it is included among the doubtful variables together with *RSL*. The results uniformly imply very wide extreme bounds which include zero.

TABLE 2—EXTREME BOUNDS FOR INTEREST ELASTICITY

Specification 1: Sum of Coefficients of *RTB* and *RSL*												
Data Confidence	0.0	0.250	0.500	0.750	0.900	0.950	0.970	0.980	0.990	0.995	0.999	1.00
Upper Bound	−.165	−.034	−.004	0.027	0.058	0.077	0.090	0.100	0.115	0.130	0.161	3.53
Lower Bound	−.165	−.304	−.336	−.370	−.404	−.425	−.439	−.449	−.466	−.482	−.515	−7.73
Specification 2: Focus Variable *RTB*												
Data Confidence	0.0	0.250	0.500	0.750	0.900	0.950	0.970	0.980	0.990	0.995	0.999	1.00
Upper Bound	0.006	.024	.029	.034	.039	.042	.044	.045	.048	.049	.055	2.24
Lower Bound	0.006	−.014	−.019	−.025	−.031	−.034	−.037	−.038	−.041	−.043	−.049	−6.27
Specification 3: Focus Variables $\sum_{i=0}^{2} RTB_{-i}$ and $\sum_{i=0}^{2} RSL_{-i}$												
Data Confidence	0.0	0.250	0.500	0.750	0.900	0.950	0.970	0.980	0.990	0.995	0.999	1.00
Upper Bound	−.177	0.501	0.588	0.682	0.770	0.826	0.863	0.891	0.935	0.976	1.07	3.48
Lower Bound	−.177	−.918	−1.02	−1.12	−1.23	−1.29	−1.33	−1.37	−1.42	−1.47	−1.57	−7.63

wide variety of dynamic specifications are typically considered. To ascertain whether the static nature of the models just considered has any impact on our results we amended the equation by including as explanatory variables the once-lagged, twice-lagged, and three times lagged values of all explanatory variables. Again, the focus variables are *RTB* and *RSL*, and our prior distribution is uninformative about the coefficients of both the current and lagged values of these variables. All other variables are regarded as doubtful, and the prior location for the sum of the coefficients of all lagged values of each of the doubtful variables is assumed to be zero. The extreme bounds for the sum of the coefficients of *RTB* plus those of *RSL*, both lagged and current, are displayed as Specification 3 in Table 2. Obviously the respecification only increases still further the specification uncertainty in the estimate of the interest elasticity of money demand.

These results all support one conclusion: the preponderance of empirical studies of the demand for money which show significant negative interest elasticities reflect the unacknowledged prior beliefs of the researcher and not the information content of the data. Based only on sample evidence and those priors which appear to us to be directly implied by theory, it is next to impossible to say anything about the interest elasticity of money demand. The data are such that a modestly energetic specification search will give back almost whatever interest elasticity one wishes to extract, particularly if more than one interest rate is included and if the specification search involves extended tinkering with dynamic effects.

IV. Identification and Simultaneity

A determination that a regression is well specified must be made on a priori grounds. In our case, least squares estimation of the demand for money is justified only if there is no feedback from the error to the principal explanatory variables: the interest rate and *GNP*. Since the Federal Reserve chooses the setting of the interest rate partly or largely in order to influence the behavior of the money stock, and therefore largely in response to past and current levels of the money stock, there appears in fact to be a strong prior presumption for the existence of correlation between the explanatory variables and the error, implying that the estimate of the coefficients of the focus variables will be inconsistent. Despite this fact, the overwhelming majority of studies of money demand rely primarily or exclusively on ordinary least squares estimators and, surprisingly, the proportion of studies that do not even mention the simultaneity problem appears to be increasing.

Those studies which do at least discuss the problem may be grouped into three classes. The first consists of those studies which bring up the simultaneity problem only to dismiss it in a perfunctory manner on the grounds that other investigators (usually Goldfeld, 1973) have concluded that parameter estimates are not much affected when correction for simultaneity is made, implying that least squares estimation is justified. The second group adopts a position that is interesting on methodological grounds. In these studies it is acknowledged that simultaneity may be a serious problem, but it is contended also that results derived as if it were nonexistent are still of interest (compare, for example, Eugene Fama, 1979a, b). Hence the need seriously to evaluate alternative possible treatments of simultaneity is obviated.

The third group, consisting of those studies which explicitly discuss simultaneity, however briefly, is of primary interest for our purpose. Laidler (1977) devoted four pages in a 182-page book to discussion of identification and simultaneity. After pointing out that the data will not in general identify either demand or supply, he argued that they will in fact identify demand if

> ... the supply function of money shifts independently of the demand-for-money function, [i.e., if] the supply-of-money function contains at least one variable that does not appear in the demand function. It is not hard to establish that this is the case, for the level of reserves made available by the central bank to the commercial banking system figures prominently in any theory of the supply of money and does not appear in any theory of the demand for money. There is also ample evidence that this variable shifts around over time, permitting us to be sure that we can obtain observations taken at different points on the demand-for-money function. [pp. 115–16]

The simplicity and directness of this argument are disarming, and one wishes that it could be accepted. But the condition that supply shifts independently of demand means that reserves must be excluded not only as an explicit explanatory variable on the demand side (Laidler is surely correct that it can reasonably be so excluded), but also as an explanatory variable for the unobserved determinants of money demand. Specifically, it is necessary to assume that the covariance of reserves and the error in money demand are zero. In the absence of this or some other assumption, neither the parameters of money demand nor the covariance of the error with reserves is identified, as may readily be verified. But surely the assumption that random shifts in money demand are not at least partially accommodated by the Federal Reserve is even less plausible than the assumption that such shifts do not result in an interest rate response. In the rest of his book Laidler's discussion of simultaneity is confined to the usual pro forma observation that it does not appear to be a problem because studies which employ simultaneous equations estimators reach conclusions that do not differ greatly from those of studies using ordinary least squares.

Let us now consider the study most often cited as justification for the abbreviated treatment of simultaneity in the large majority of recent empirical studies of money demand: Goldfeld (1973). Here again the discussion of simultaneity problems is agreeably brief; three pages in an article of sixty-one pages suffice for the discussion of both simultaneous equations problems and serial correlation of residuals. The reader is told that "... a casual interpretation of the evidence suggested that simultaneity was not likely to be important..." (p. 621). The suggestion here is that there is some way to interpret the data or the regressions directly to ascertain whether simultaneity problems exist; on the usual view, the determination must be made on a priori grounds. However, Goldfeld also checked these "rough impressions" by choosing a "plausible set of instruments" and reestimating by two-stage least squares. He found that the estimated equation was not greatly altered. Now it is obvious that this conclusion by itself suggests only that ordinary least squares and the instrumental variables estimator have approximately the same inconsistency, not that either is necessarily consistent. We are led to inquire whether the case for believing that Goldfeld's two-stage least squares estimator is consistent is any stronger than the corresponding case for ordinary least squares.

An instrumental variables estimator is approximately consistent if the covariance of the instruments with the error is small relative to the covariance with the observed explanatory variables, a determination which can be made only on a priori grounds since the error is unobservable. In a footnote the reader learns that the "plausible" instruments were population, the discount rate, state and local government spending, and the lagged money stock. It is altogether unclear why we should believe that state and local government spending and especially population covary more with the interest rate than with the error. There is no doubt that the discount rate covaries with the Treasury bill rate, but it may well also be highly correlated with the error. On the traditional interpretation the Federal Reserve uses the discount rate as an instrument to influence the money stock, implying that it will surely respond to random shifts in money demand. If, on the other hand, the discount rate is viewed as merely following the movement of open market rates passively, as is more realistic in recent years, it should be subject to the same inconsistency as open market rates themselves. Similarly, the lagged money stock is a suitable instrument only if it can be argued on prior grounds that its correlation with the error is low relative to that with the interest rate, a question which Goldfeld does not address. We see no reason for supposing that the lagged money stock may serve as an instrument in estimating demand any more than of supply. We are left unconvinced that money demand has been consistently estimated.

Sometimes least squares estimation of money demand is justified on the grounds that the Federal Reserve uses an interest rate as its instrument in the conduct of short-run monetary policy, supposedly implying that the interest rate may be taken exogenous. In its simplest form, this argument rests on an elementary confusion between two senses in which the term "exogenous" is used in the economics literature. In macroeconomic theory a variable, the level of which is set by government as an implementation of economic policy, is represented analytically as an exogenous variable; that is, as a variable not determined within the model, since the latter explains only the behavior of the private sector. Analysis then centers on the effect of policy changes, represented by shifts in the exogenous policy variable(s), on endogenous variables. The definition of exogeneity relevant for statistical estimation, however, is entirely different: a variable in a regression is statistically exogenous only if it is independent (in the probability sense) of the unobserved explanatory variables. Whether a variable which is exogenous in the former sense is also exogenous in the latter and relevant sense depends on how government policy is in fact conducted; that is, on the nature of the government's policy rule. Thus the mere fact that under a "money-market conditions" operating procedure the Federal Reserve formulates policy in terms of an interest rate setting obviously does not by itself justify the assumption that the interest rate is statistically exogenous, and that therefore the demand for money can with validity be estimated by least squares.

We do not suggest that all analysts who have dismissed simultaneity problems by referring to the Federal Reserve's money-market conditions operating procedure are unaware of the elementary point just made, although it is clear that some are. Nonetheless, surprisingly, we do not know of any clear discussion in the literature of just what must be assumed about the Federal Reserve's policy rule if least squares estimation of money demand equations is to be justified (however, see our discussion of the question in the following section). Most treatments, on the contrary, are extremely vague at best. Consider, for example, Laidler's 1980 discussion:

> I believe that many would argue that in the United States, the actual conduct of policy in recent years has in fact been such as to make it appropriate to think of the money supply and the base as responding to demand side factors, and hence, to model the short-run dynamics of the demand for money function in the conventional way utilized so far in this paper [i.e., taking the money stock as the dependent variable]. They would defend the view in the following way. Whatever changes there may or may not have been in the targets and indicators of monetary policy since, shall we say, 1953, its instruments have consistently been interest rates. The monetary authority has attempted to achieve whatever may have been its ends by standing ready to buy and sell government securities at a given price (although not necessarily a pegged price). If over any reasonably short period, say a quarter, real income and prices may be regarded as predetermined, and if the monetary authority, and hence the banking system, stands ready to buy and sell securities at a given price, then there is no obstacle to the economy as a whole adjusting its money holdings towards a desired level at a pace of its own choosing. Given this view of the money supply process, the conventional stock adjustment approach to estimating the demand for money function is correctly specified for the United States, whatever may be the drawbacks to the use of such a specification for other times and places. [pp. 237–38]

First, it is not clear what Laidler intended in distinguishing between a "given" price and a "pegged" price. Second and more important, the introduction of the question of whether or not explanatory variables for the money stock are predetermined is altogether mysterious; the relevant question is whether they are statistically exogenous, not predetermined, and one condition is neither necessary nor sufficient for the other.[10]

Laidler went on to consider an alternative representation of financial market behavior, based loosely on Karl Brunner and Allan Meltzer's "credit market hypothesis," which appeared to Laidler to suggest that the money stock rather than interest rates may be taken as statistically exogenous.[11] In that case the appropriate estimating procedure would be to regress an interest rate measure on the money stock and the other explanatory variables, instead of taking the money stock as the dependent variable, and then solve the resulting estimated equation to obtain a consistently estimated money demand equation written in the usual manner.

If one is not certain which direction of normalization is more likely to eliminate simultaneity problems, it would appear reasonable to try both normalizations and ascertain how much difference the direction of normalization makes for the parameter estimates of interest. If the estimates are not too different, the presumption would appear to be that simultaneity problems are not serious. William Poole (1970b) proposed just this approach, remarking that "it can be hoped" that the resulting estimates will bracket the true parameter value (p. 487). Goldfeld (1973) also suggested that estimating money demand in the renormalized form and comparing the results with the original estimates would "shed some light" (p. 622) on the simultaneity question. Now, it is well known that bounds on estimates of variables which are measured subject to error can be obtained in this manner, and this appears to be what Poole and Goldfeld were thinking of, but it is not clear why Poole and Goldfeld think that the simultaneity problem is analogous. In the following section, however, we show that in the case under consideration the probability limits of the parameter estimates under the alternative normalizations do not in fact bracket the true parameter in general.

[10]For discussions of exogeneity, and of the distinction between exogeneity and predeterminedness, see Robert Engle, David Hendry, and Jean-François Richard, and Rodney Jacobs, Leamer, and Michael Ward.

[11]There is a curious lack of parallelism in Laidler's argument. The operative element of the "money market hypothesis" that is held to justify the specification of interest rate exogeneity is that the Federal Reserve responds passively to shifts in money demand. However, Brunner and Meltzer defined the money market hypothesis and credit market hypothesis as rival theories of how financial markets operate, an entirely different matter.

Thus we remain uninstructed on how the renormalization estimation procedure considered by Poole, Goldfeld, and Laidler sheds any light at all on the simultaneity question.

V. Identification and Simultaneity: Alternative Treatments

We have criticized the empirical literature on money demand for minimizing the seriousness of simultaneity problems and for representing obviously inadequate corrections as in fact adequate. The question arises whether any better treatment is available. In this section we consider several apparently reasonable approaches to the problem, but conclude that they are in fact no more plausible than the remedies analyzed and rejected in the preceding section. We conclude that there is no obvious way to formulate models of equilibrium in financial markets in which the demand for money is identified.

Suppose that the demand for money is

$$m=\alpha r+u, \tag{4}$$

where m, the money stock, and r, the interest rate, are both measured as deviations from means, and u is an error. A money supply function may be written most simply as

$$r=\beta m+v, \tag{5}$$

which represents both the money supply function of commercial banks and the response function of the Federal Reserve. The model (4) and (5) is evidently unidentified. To identify the demand equation we must find some variable that shifts supply but can be excluded from the demand relation on prior grounds and is uncorrelated with the error in demand. Most of the obvious candidates (such as reserves, as we pointed out in the preceding section) fail the latter condition. Such lagged endogenous variables as the lagged money stock clearly enter as determinants of the Federal Reserve's response function, but again they cannot plausibly be excluded from the demand side either explicitly as observable explanatory variables for the demand for money or implicitly through the time dependence of the error.

Estimation of disaggregated money demand equations has been suggested as one means of eliminating feedback between the errors and the interest rate. Since the Federal Reserve estimates several major components of the money stock through the Demand Deposit Ownership Survey, disaggregated estimation is feasible.[12] Let us determine what must be assumed in order that least squares estimation of sector demand equations yield a consistent estimate of the interest elasticity coefficients. Suppose the demand for money in the jth sector is

$$m_j=\alpha_j r+w+u_j, \tag{6}$$

where w is an error common to all sectors and u_j is the shock to the jth sector. Assume that the supply relation is of the form

$$r=\beta\sum_j m_j/n+v \tag{7}$$

where n is the number of sectors. For the sake of the argument, suppose that we can somehow assume that w, v and the u_j are all independently distributed with zero means and variances σ_w^2, σ_v^2, and σ_j^2. If $\hat{\alpha}_j$ is the least squares estimate of α_j in (6), we have that

$$plim(\hat{\alpha}_j)=\alpha_j+cov(w+u_j,r)/var(r). \tag{8}$$

From (6) and (7) it is immediate that

$$cov(w+u_j,r) =\beta\left(\sigma_w^2+\sigma_j^2/n\right)\Big/\left(1-\beta\sum_j\alpha_j/n\right) \tag{9}$$

while

$$var(r)=\left(\beta^2\left(\sigma_w^2+\sum_j\sigma_j^2/n^2\right)+\sigma_v^2\right)\Big/\left(1-\beta\sum_j\alpha_j/n\right)^2. \tag{10}$$

Substituting (9) and (10) into (8), we obtain finally

$$plim(\hat{\alpha}_j)=\alpha_j+\beta\left(\sigma_w^2+\sigma_j^2/n\right)\left(1-\beta\sum_j\alpha_j/n\right)\Big/\left(\beta^2\left(\sigma_w^2+\sum_j\sigma_j^2/n^2\right)+\sigma_v^2\right).$$

[12]See Helen Farr, Richard Porter, and Eleanor Pruitt for discussion of the Demand Deposit Ownership Survey.

Obviously even if w, v, and the u_j are independently distributed, ordinary least squares estimation is inconsistent ($1-\beta\Sigma_j\alpha_j/n \neq 0$ is a condition for the existence of a solution to the model). In order to insure consistency it must be assumed both that σ_w^2 equals zero (i.e., that the sector shocks have no common component) and that there are many sectors (n is large). Since the number of sectors for which data are collected in the Demand Deposit Ownership Survey is only four, and since the random shocks in sector demands cannot reasonably be assumed to have no common component, it appears unrealistic to hope that disaggregated estimation will provide a ready resolution to identification and simultaneity problems.

Identification may be sought from prior restrictions on the Federal Reserve's response function. First, as noted in the preceding section, it has been suggested that the assumed existence of a money market conditions control procedure may be used to justify ordinary least squares regression with the money stock as the dependent variable. Under the monetary control procedure in use up to October 1979 the instruction from the Federal Open Market Committee to the Federal Reserve Bank of New York consisted of a setting for the interest rate which was to be adhered to until the next FOMC meeting unless a proviso clause related to the monetary aggregates becomes operative. If 1) the existence of the proviso clause is ignored, 2) the data interval is assumed to be the month rather than the quarter (since the FOMC meets three times within the quarter and generally revises the instrument setting each time), 3) the Federal Reserve's control procedure is viewed as mechanically implemented rather than subject to large judgmental input, as is in fact the case, and 4) the response of commercial banks to current shocks can be ignored, then the response function may be written as

$$r_t = \beta\phi_{t-1} + v_t, \tag{11}$$

Here ϕ_{t-1} is the set of determinants of the interest rate setting, which by assumption does not include any currently determined variables. Here there is no direct feedback from the error in money demand to the interest rate, since the observable determinants of the interest rate in the response function are not affected by the current error in money demand. But ordinary least squares is still inconsistent unless it is also assumed that the errors in demand and supply are independent (otherwise r and u will be correlated) and that the error in demand is not autocorrelated. If u is autocorrelated and if m_{t-1} is a component of ϕ_{t-1}, as is surely the case, the current error in money demand will be correlated with ϕ_{t-1} and hence with the interest rate even though there is no direct causal link between them. Again, the collection of assumptions required to justify the usual estimating procedure for the money demand equation is formidable.

In the preceding section we noted that reversing the direction of normalization has been suggested as a procedure that might "shed light" on the simultaneity question in estimating money demand equations. We are led to investigate the conditions under which the estimates from the alternative normalizations bracket the true parameter in the probability limit. Let us then define $\hat{\alpha}$ as the estimated interest elasticity when the money stock is taken as the dependent variable in a least squares regression, and $\hat{\hat{\alpha}}$ to be the estimated interest rate elasticity when the interest rate is the dependent variable (i.e., when the money stock is included as an explanatory variable, but the resulting estimated equation is resolved for the money stock). The question is whether *plim*$(\hat{\alpha})$ and *plim*$(\hat{\hat{\alpha}})$ bracket α in general, and if they do not do so in general what conditions are required to assure the conclusion. It is immediately apparent that without prior restrictions on the form of the demand and supply equations the property will not obtain. Consider the supply-demand pair

$$m = \alpha r + u \text{ (demand)}$$
$$m = \beta r + v \text{ (supply)}$$

or the pair

$$m = \alpha_1 r + \alpha_2 z + u \text{ (demand)}$$
$$m = \beta_1 r + \beta_2 z + v \text{ (supply)}$$

where z is an exogenous explanatory variable (or a vector of such variables) entering the supply and demand equations. The symmetry of these supply-demand pairs implies that $\hat{\alpha}$ and $\hat{\hat{\alpha}}$ can be associated just as well with the interest-elasticity of supply as with that of demand. It follows trivially that no inference about either α, β, α_1, or β_1 can be made from the models as specified. Thus we have immediately that there is nothing to be learned by comparing the interest rate elasticities estimated from alternative normalizations in the absence of prior exclusion restrictions or other identifying assumptions.

The obvious system to consider is

$$(12) \qquad m=\alpha r+u \text{ (demand)}$$

$$(13) \qquad m=\beta_1 r+\beta_2 z+v \text{ (supply)}$$

in which the exogenous variable z is assumed to be excluded from demand on prior grounds. Then we have

$$\hat{\alpha}=\sum mr/\sum r^2 \text{ and } \hat{\hat{\alpha}}=\sum m^2/\sum mr.$$

In this system it happens to be possible to show that $plim(\hat{\alpha})$ and $plim(\hat{\hat{\alpha}})$ do in fact bracket α under the prior restrictions that α and β_1 are negative. To do so, solve (12) and (13) for their reduced forms and verify that

$$(14) \quad plim(\hat{\alpha})=\left(\sigma_z^2\beta_2^2\alpha+\sigma_u^2\beta_1+\sigma_v^2\alpha\right)\Big/\left(\sigma_z^2\beta_2^2+\sigma_u^2+\sigma_v^2\right)$$

$$(15) \quad plim(\hat{\hat{\alpha}})=\left(\sigma_z^2\alpha^2\beta_2^2+\sigma_u^2\beta_1^2+\sigma_v^2\alpha^2\right)\Big/\left(\alpha\sigma_z^2\beta_2^2+\sigma_u^2\beta_1+\sigma_v^2\alpha\right).$$

Now it is evident from (14) and (15) that $plim(\hat{\alpha})\leq\alpha$ iff $\beta_1\leq\alpha$, while $\alpha\leq plim(\hat{\hat{\alpha}})$ iff $\alpha\beta_1\leq\beta_1^2$. Assuming that α is negative, the condition $\beta_1\leq\alpha$ assures that β_1 is negative as well. Consequently, cancelling β_1 from the inequality $\alpha\beta_1\leq\beta_1^2$ reverses its sense, and therefore reduces it to $\beta_1\leq\alpha$, which we already have. Thus $plim(\hat{\alpha})\leq\alpha\leq plim(\hat{\hat{\alpha}})$ will be satisfied if $\beta_1\leq\alpha<0$. Similarly, $plim(\hat{\hat{\alpha}})\leq\alpha\leq plim(\hat{\alpha})$ will be satisfied if $\alpha\leq\beta_1<0$. It follows that, as long as we can assume that both α and β_1 are less than zero, the two probability limits will in fact bracket the true parameter, as we required to demonstrate. We have a strong prior belief that α is negative, but β_1 cannot be signed on prior grounds. To the extent that β_1 reflects commercial bank behavior, a positive sign is indicated; however, it is likely that β_1 reflects primarily the Federal Reserve response function rather than commercial bank behavior. Even assuming optimal money stock control by the Federal Reserve the response function may be either positively or negatively sloped, depending on the coefficients and error variances of the model (see Poole, 1970b, or LeRoy and David Lindsey, for example). But the major point is that to construct the proof it was necessary to adopt an identifying restriction on prior grounds. If such a restriction is available (an assumption we are not willing to adopt), then it makes more sense simply to estimate consistently in the first place than to obtain estimated bounds on α via the renormalization estimation procedure.

We conclude by considering the implications of assuming that the Federal Reserve's control rule may be given an optimal interpretation. This assumption imposes restrictions linking the parameters of the response function with those of the demand equation, and it is reasonable to explore whether the latter may not be identified thereby. Suppose that the money demand equation is described by the transfer function

$$(16) \qquad m_t=\alpha r_{t-n}+\sum_{j=0}^{\infty} a_j u_{t-j},$$

where the u_t are white noise innovations. This specification assumes that the interest rate affects the demand for money with a lag of n periods; in the cases considered above n equals zero, but it is desirable to achieve greater generality here. Assume that the Federal Reserve wishes to vary r_t, the instrument, so as to minimize an objective function of the form $\Sigma_t(1+\rho)^{-t}(m_t-m_t^*)^2$, where $\{m_t^*\}$ is some preassigned "optimal" trajectory for the money stock. Under lagged reserve accounting the assumption that the Federal Reserve ignores reserves and constructs an optimal monetary control rule from the money demand equation alone involves no loss of generality.[13] The certainty equivalence principle ensures that the loss will be minimized if the interest rate is varied so that

$$E_t(m_{t+n}) = m^*_{t+n}. \tag{17}$$

From (16) we have that

$$m_{t+n} = \alpha r_t + \sum_{j=0}^{\infty} a_j u_{t+n-j}, \tag{18}$$

implying that

$$E_t(m_{t+n}) = \alpha r_t + \sum_{j=n}^{\infty} a_j u_{t+n-j}. \tag{19}$$

By assumption, r_t is set so that $E_t(m_{t+n}) = m^*_{t+n}$, so (19) becomes

$$m^*_{t+n} = \alpha r_t + \sum_{j=n}^{\infty} a_j u_{t+n-j} \tag{20}$$

for optimal r_t. In such a world, which parameters of the demand equation will be identifiable? From (18) and (20), the observed money stock will fluctuate around the optimum according to

$$m_{t+n} = m^*_{t+n} + \sum_{j=0}^{n-1} a_j u_{t+n-j}, \tag{21}$$

implying that the first n of the a_j can be estimated. However, from (20) the remaining a_j and α, the interest elasticity of money demand, cannot be identified even if the m^*_t are known. Thus once again we find that the parameter we are concerned with cannot be estimated.

[13] Poole (1970a) showed that under current accounting an optimal monetary control rule generally involves both reserves and interest rates. The reason reserves enters the optimal combination policy is that in general under current reserve accounting changes in reserves provide information about contemporaneous shifts in money demand. Under lagged reserve accounting, however, changes in reserves demand depend primarily on changes in money demand two weeks earlier. But the money stock two weeks earlier is itself observable, or nearly so, implying that reserves no longer provide information useful for money stock control. This fact implies that an optimal combination policy coincides with a pure interest rate policy, and thus the optimal monetary control rule may be constructed from the money demand equation alone (see LeRoy).

VI. Conclusion

In this paper we have argued two largely independent points. The first is that the negative interest elasticity of money demand reported in the literature represents prior beliefs much more than sample information. The second is that the treatment of simultaneity in the literature is totally inadequate. Thus we are left with the conclusion that we are unpersuaded by existing attempts to estimate a money demand equation, but we are unable to supply an attractive alternative. This outcome is admittedly unsatisfactory, and we do not offer it as a final conclusion. But we believe that no progress can be made in estimating such structural macroeconomic equations as that for the demand for money until we rid ourselves of the habits of data mining, of building in priors through selective reporting, and of casually adopting what Christopher Sims has called "incredible" identifying assumptions to dispose of simultaneity problems.[14]

[14] For an early criticism of the latter practice, see the classic paper of T. C. Liu.

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