ECONOMETRICA

VOLUME 48 JANUARY, 1980 NUMBER 1

MACROECONOMICS AND REALITY[1]

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Existing strategies for econometric analysis related to macroeconomics are subject to a number of serious objections, some recently formulated, some old. These objections are summarized in this paper, and it is argued that taken together they make it unlikely that macroeconomic models are in fact over identified, as the existing statistical theory usually assumes. The implications of this conclusion are explored, and an example of econometric work in a non-standard style, taking account of the objections to the standard style, is presented.

THE STUDY OF THE BUSINESS cycle, fluctuations in aggregate measures of economic activity and prices over periods from one to ten years or so, constitutes or motivates a large part of what we call macroeconomics. Most economists would agree that there are many macroeconomic variables whose cyclical fluctuations are of interest, and would agree further that fluctuations in these series are interrelated. It would seem to follow almost tautologically that statistical models involving large numbers of macroeconomic variables ought to be the arena within which macroeconomic theories confront reality and thereby each other.

Instead, though large-scale statistical macroeconomic models exist and are by some criteria successful, a deep vein of skepticism about the value of these models runs through that part of the economics profession not actively engaged in constructing or using them. It is still rare for empirical research in macroeconomics to be planned and executed within the framework of one of the large models. In this lecture I intend to discuss some aspects of this situation, attempting both to offer some explanations and to suggest some means for improvement.

I will argue that the style in which their builders construct claims for a connection between these models and reality—the style in which "identification" is achieved for these models—is inappropriate, to the point at which claims for identification in these models cannot be taken seriously. This is a venerable assertion; and there are some good old reasons for believing it;[2] but there are also some reasons which have been more recently put forth. After developing the conclusion that the identification claimed for existing large-scale models is incredible, I will discuss what ought to be done in consequence. The line of argument is: large-scale models do perform useful forecasting and policy-analysis functions despite their incredible identification; the restrictions imposed in the usual style of identification are neither essential to constructing a model which can perform these functions nor innocuous; an alternative style of identification is available and practical.

Finally we will look at some empirical work based on an alternative style of macroeconometrics. A six-variable dynamic system is estimated without using

[1] Research for this paper was supported by NSF Grant Soc-76-02482. Lars Hansen executed the computations. The paper has benefited from comments by many people, especially Thomas J. Sargent and Finn Kydland.

[2] T. C. Liu [15] presented convincing arguments for the assertion in a classic article.

theoretical perspectives. Under a sophisticated neo-monetarist interpretation, a restriction on the system which implies that monetary policy shocks could explain nearly all cyclical variation in real variables in the economy is tested and rejected. Under a more standard macroeconometric interpretation, a restriction which is treated as a maintained hypothesis in econometric work with Phillips curve "wage equations" or paired wage and price equations is also rejected.

1. INCREDIBLE IDENTIFICATION

A. *The Genesis of "A Priori Restrictions"*

When discussing statistical theory, we say a model is identified if distinct points in the model's parameter space imply observationally distinct patterns of behavior for the model's variables. If a parameterization we derive from economic theory (which is usually what we mean by a "structural form" for a model) fails to be identified, we can always transform the parameter space so that all points in the original parameter space which imply equivalent behavior are mapped into the same point in the new parameter space. This is called normalization. The obvious example is the case where, not having an identified simultaneous equation model in structural form, we estimate a reduced form instead. Having achieved identification by normalization in this example, we admit that the individual equations of the model are not products of distinct exercises in economic theory. Instead of using a reduced form, we could normalize by requiring the residuals to be orthogonal across equations and the coefficient matrix of current endogenous variables to be triangular. The resulting normalization into Wold causal chain form is identified, but results in equations which are linear combinations of the reduced form equations. Nobody is disturbed by this situation of multiple possible normalizations.

Similarly, when we estimate a complete system of demand equations, we recognize that the set of equations, in which each quantity appears only once, on the left-hand-side of one equation in the system, and all prices appear on the right of each equation, is no more than one of many possible normalizations for a system of equations describing demand behavior. In principle, we realize that it does not make sense to regard "demand for meat" and "demand for shoes" as the products of distinct categories of behavior, any more than it would make sense to regard "price of meat" and "price of shoes" equations as products of distinct categories of behavior if we normalized so as to reverse the place of prices and quantities in the system. Nonetheless we do sometimes estimate a small part of a complete demand system together with part of a complete supply system—supply and demand for meat, say. In doing this, it is common and reasonable practice to make shrewd aggregations and exclusion restrictions so that our small partial-equilibrium system omits most of the many prices we know enter the demand relation in principle and possibly includes a shrewdly selected set of exogenous variables we expect to be especially important in explaining variation in meat demand (e.g., an Easter dummy in regions where many people buy hams for Easter dinner).

While individual demand equations developed for partial equilibrium use may quite reasonably involve an array of restrictions appropriate to that use, it is evident that a system of demand equations built up incrementally from such partial-equilibrium models may display very undesirable properties. In effect, the shrewd restrictions which are useful for partial equilibrium purposes, when concatenated across many categories of demand, yield a bad *system* of restrictions.

This point is far from new, having been made, e.g., by Zvi Griliches [**9**] in his criticism of the consumption equations of the first version of the Brookings model and by Brainard and Tobin [**3**] in relation to financial sector models in general. And of course this same point motivates the extensive work which has been done on econometrically usable functional forms for complete systems of demand equations and factor demand equations.

The reason for re-emphasizing the dangers of one-equation-at-a-time specification of a large model here is that the extent to which the distinctions among equations in large macromodels are normalizations, rather than truly structural distinctions, has not received much emphasis. In the version of the FRB-MIT model reported in [**2**], for example, a substantial part of the interesting behavioral equations of the financial sector are demand equations for particular assets. Consumption, of course, is represented by demand equations, and the supply of labor and demand for housing also in principle represent components of a system of equations describing the public's allocations of their resources. Thus the strictures against one-equation-at-a-time specification which are ordinarily applied to the financial or consumption equations of a model as a subgroup, really apply to this whole set of equations.

If large blocks of equations, running across "sectors" of the model which are ordinarily treated as separate specification problems, are in fact distinguished from one another only by normalization, what "economic theory" tells us about them is mainly that any variable which appears on the right-hand-side of one of these equations belongs in principle on the right-hand-side of all of them. To the extent that models end up with very different sets of variables on the right-hand-sides of these equations, they do so not by invoking economic theory, but (in the case of demand equations) by invoking an intuitive, econometrician's version of psychological and sociological theory, since constraining utility functions is what is involved here. Furthermore, unless these sets of equations are considered as a system in the process of specification, the behavioral implications of the restrictions on all equations taken together may be much less reasonable than the restrictions on any one equation taken by itself.

The textbook paradigm for identification of a simultaneous equation system is supply and demand for an agricultural product. There we are apt to speak of the supply equation as reflecting the behavior of farmers, and the demand equation as reflecting the behavior of consumers. A similar use of language, in which labor supply equations are taken to apply to "workers," consumption equations to "consumers," asset demand equations to "savers," sometimes obscures the distinction in macromodels between normalized and structurally identified equations.[3] On the other hand, the distinction between "employers" and "investigators" on the one hand, and "consumers" and "workers" on the other, does have some structural justification. There certainly are policies which can drive a wedge between supply and demand prices for transactions between the "business" and "household" sectors, which is roughly the distinction with which we are concerned. Furthermore, if business behavior is taken to be competitive, the business sector simply traces out the efficient envelope of available technology in response to demand shifts. Then the distinction between business and households becomes the distinction between "nature" and "tastes" on which identification in the supply-demand paradigm rests. The idea that weather affects grain supply and not (much) grain demand, while the ethnic and demographic structure of the population affects grain demand but not (much) grain supply, is a powerful source of identifying restrictions. The same nature-tastes distinction is a source of powerful identifying restrictions in large macromodels, but the number of such restrictions available is not large relative to the number of equations and variables in large macromodels.

B. *Dynamics*

The fact that large macroeconomic models are dynamic is a rich source of spurious "a priori" restrictions, as we shall see below, but it also weakens the few legitimate bases for generating identifying restrictions alluded to in the previous section. If we accept the modern anti-interventionist school's argument that dynamic macroeconomic models ought not to violate the principle that markets clear,[4] then dynamics do not raise new problems in this respect; the business sector singlemindedly pursues profit, according to the directions of the observable price vector, so that the difference between the business sector of a dynamic model and that of a static model is only in whether the efficiency frontier traced out has dynamic elements. If instead we take the view that prices themselves may adjust sluggishly, we enter the wilderness of "disequilibrium economics." This phrase must, it seems to me, denote a situation in which we cannot suppose that business behavior is invariant under changes in the public's tastes. The reason is that business behavior, when markets don't clear, must depend not only on hypothetical business demands and supplies given current prices, but also on the nature of whatever rationing is currently going on—e.g. on the excess demand of Walrasian theory. If the degree of excess demand or supply in the labor market enters

[3] In Hurwicz's [**12**] abstract discussion of structural systems it is apparent that an equation system identified by normalization is not an identified structure. An identified structural equation is one which uniquely remains invariant under a certain class of "interventions" in the system. In the supply and demand paradigm, the natural class of interventions to consider is excise taxes. It is not impossible that a system of demand equations be structural in Hurwicz's sense—McFadden [**19**] has provided an instance of a structural interpretation of a sort of demand equation, in which the identifying interventions are deletions or additions in the list of available commodities. But nothing like McFadden's analysis exists or is likely to be developed to justify structural distinction between labor supply and consumption, for example.

[4] This position is set forth persuasively by Lucas [**16**].

employer behavior, then by that route any variable which we think of as connected to labor supply decisions enters the dynamic labor demand equation.

J. D. Sargan [**25**] several years ago considered the problem of simultaneous-equation identification in models containing both lagged dependent variables and serially correlated residuals. He came to the reassuring conclusion that, if a few narrow-looking special cases are ruled out, the usual rules for checking identification in models with serially uncorrelated residuals apply equally well to models with serially correlated residuals. In particular, it would ordinarily be reasonable to lump lagged dependent variables with strictly exogenous variables in checking the order condition for identification, despite the fact that a consistent estimation method must take account of the presence of correlation between lagged dependent variables and the serially correlated residuals. Though consistent estimation of such models poses formidable problems, Sargan's analysis suggested that identification is not likely to be undermined by the combination of lagged dependent variables and serial correlation.

Recent work by Michio Hatanaka [**11**], however, makes it clear that this sanguine conclusion rests on the supposition that exact lag lengths and orders of serial correlation are known a priori. On the evidently more reasonable assumption that lag lengths and shapes of lag distributions are not known a priori,[5] Hatanaka shows that the order condition takes on an altered form: we must in this case cease to count repeat occurrences of the same variable, with different lags, in a single equation. In effect, this rule prevents lagged dependent variables from playing the same kind of formal role as strictly exogenous variables in identification; we must expect that to identify an equation we will have to locate in other equations of the system at least one strictly exogenous variable to serve as an instrument for each right-hand-side endogenous variable in the given equation.

Application of Hatanaka's criterion to large-scale macromodels would probably not suggest that they are formally unidentified. The version of the FRB-MIT model laid out in [**10**], e.g., has over 90 variables categorized as strictly exogenous, while most equations contain no more than 6 or 8 variables. However the Hatanaka criterion, by focusing attention more sharply on the distinction between endogenous and strictly exogenous variables, might well result in models being respecified with shorter lists of exogenous variables. Many, perhaps most, of the exogenous variables in the FRB-MIT model [**10**] or in Fair's model [**6**] are treated as exogenous by default rather than as a result of there being good reason to believe them strictly exogenous. Some are variables treated as exogenous only

[5] By saying that it is evidently more reasonable to assume we do not know lag lengths and shapes a priori, I do not mean to suggest that one should not impose restrictions of a reasonable form on lag lengths and shapes in the process of estimation. However, we should recognize that truncating lag distributions is part of the process of estimaton—lag length is itself estimated one way or another—and that when our model is not identified without the pretense that we know lag length to begin with, it is just not identified. A similar point applies to "identifying" simultaneous equations models by imposing "a priori" constraints that coefficients which prove statistically insignificant are zero. Setting such coefficients to zero may be a justifiable part of the estimation process, but it does not aid in identification.

because seriously explaining them would require an extensive modeling effort in areas away from the main interests of the model-builders. Agricultural price and output variables, the price of imported raw materials, and the volume of exports are in this category in the FRB-MIT model. Other variables are treated as exogenous because they are policy variables, even though they evidently have a substantial endogenous component. In this category are the Federal Reserve discount rate, federal government expenditures on goods and services, and other variables. It appears to me that if the list of exogenous variables were carefully reconsidered and tested in cases where exogeneity is doubtful, the identification of these models might well, by Hatanaka's criterion, fail, and would at best be weak,[6] even if the several other sources of doubt about identifying restrictions in macromodels listed in this paper are discounted.

C. *Expectations*

It used to be that when expected future values of a variable were thought to be important in a behavioral equation, they were replaced by a distributed lag on that same variable. Whatever else may be said for or against it, this practice had the advantage of producing uncomplicated effects on identification. As the basis in economic theory for such simple treatments of expectations has been examined more critically, however, it has become apparent that they are unsound, and that sound treatments of expectations complicate identification substantially. Whether or not one agrees that economic models ought always to assume rational behavior under uncertainty, i.e. "rational expectations," one must agree that any sensible treatment of expectations is likely to undermine many of the exclusion restrictions econometricians had been used to thinking of as most reliable. However certain we are that the tastes of consumers in the U.S. are unaffected by the temperature in Brazil, we must admit that it is possible that U.S. consumers, upon reading of a frost in Brazil in the newspapers, might attempt to stockpile coffee in anticipation of the frost's effect on price. Thus variables known to affect supply enter the demand equation, and vice versa, through terms in expected price.

But though analysis of rational expectations raises this problem for us, by carrying through with that analysis we may achieve identification again by a new route. The rational expectations hypothesis tells us expectations ought to be formed optimally; by restricting temperature in Brazil to enter U.S. demand for coffee *only* through its effect on the optimal forecast of price, we may again identify the demand equation. Wallis [**33**] and Sargent [**26**] (among others) have shown how this can be done. Lucas [**16**] in fact suggested that this be done in some of the earliest work on the implications of rational expectations for macroeconomics.

[6] In this case of serial correlation of undetermined form and lagged dependent variables with undetermined lag lengths, the model is identified by the relation between structural parameters and the distributed lag regressions of endogenous variables on strictly exogenous variables. When the strictly exogenous variables have low explanatory power, estimates of the endogenous-on-exogenous regressions are likely to be subject to great sampling error, and the identification may be said to be weak.

It is my view, however, that rational expectations is more deeply subversive of identification than has yet been recognized. When we follow Hatanaka in removing the crutch of supposed a priori knowledge of lag lengths, then in the absence of expectational elements, we find the patient, though perhaps wobbly, re-establishing equilibrium. At least the classical form of identifying restriction, the nature-vs.-tastes distinction that identifies most supply and demand models, is still in a form likely to work under the Hatanaka criterion. In the presence of expectations, it turns out that the crutch of a priori knowledge of lag lengths is indispensable, even when we have distinct, strictly exogenous variables shifting supply and demand schedules.

The behavioral interpretation of this identification problem can be displayed in a very simple example.[7] Suppose a firm is hiring an input, subject to adjustment costs, and that input purchase decisions have to be made one period in advance of actual production. Suppose further that the optimization problem has a quadratic-linear structure (justifying certainty-equivalence) and that the only element of uncertainty is a stochastic process shifting the demand curve. In this situation, the firm's hiring decisions will depend on forecasts of the demand-shift variable. But suppose that the demand-shift process is a martingale-increment process—that is, suppose that the expected value of all future demand shifts is always the mean value of that variable. Then the expected future demand curve is always the same, input hiring decisions are always the same, and we obviously cannot hope to estimate from observed firm behavior the parameters of the dynamic production function.

Special though it may seem, this example is representative of a general problem with models incorporating expectations. Such models will generally imply that behavior depends on expected values of future prices (or of other variables). In order to guarantee that we can discover from observed behavior the nature of that dependence on future prices, we must somehow insure that expected future prices have a rich enough pattern of variation to identify the parameters of the structure. This will ordinarily require restrictions on the form of serial correlation in the exogenous variables.

Of course, in a sense these problems are not fresh. If we want to estimate a distributed lag regression of y on x we must always restrict x not to be identically zero. The new element is that when we try to estimate a distributed lag regression of y on x and expected future x, the variation in the expected future x will always be less rich than that in the past x, so that the required restrictions are likely to be an order of magnitude more stringent in rational expectations models. To take a slightly more elaborate example, suppose our behavioral model is

$$(1) \qquad c^*y(t) = b^{-*}p(t) + b^{+*}\hat{p}_t(t),$$

where "*" denotes continuous-time convolution, $\hat{p}_t$ is the stochastic process of expected values of p given information available at time t, $b^+(s) = 0$ for $s > 0$, and

[7] Robert Solow used essentially the same example in published comments [**31**] on earlier work of mine.

$b^-(s)=0$ for $s<0$, and $c(s)=0$, $s<0$.[8] To be explicit about the notation, (1) could be written as

$$c*y(t)=\int_0^\infty b^-(s)p(t-s)\,ds+\int_0^\infty b^+(-s)\hat{p}_t(t+s)\,ds. \tag{2}$$

Now suppose that the only information available at t is current and past values of p, and suppose further that p is a stationary first-order Markov process, i.e. that p can be thought of as generated by the stochastic differential equation

$$\dot{p}(t)=-rp(t)+e(t), \tag{3}$$

with e a white noise process. It then follows that

$$\hat{p}_t(t+s)=e^{-rs}p(t), \qquad \text{all } s>0. \tag{4}$$

Therefore (1) takes the form

$$c*y(t)=b^-*p(t)+p(t)g(b^+), \tag{5}$$

where the function g is given by $g(b^+)=\int_0^\infty b^+(s)\,e^{-rs}\,ds$. While we can expect to recover $g(b^+)$ from the observed behavior of y and p, knowledge of $g(b^+)$ will not in general determine b^+ itself unless we have available enough restrictions on b^+ to make it a function of a single unknown parameter. First-order Markov processes are widely used as examples in econometric discussions because of their analytic convenience, and they do not of course pose any identification problem for the estimation of b^-—the past of p will show adequately rich variation to identify b^- even if our parameter space for b^- is infinite-dimensional. This distinction, the need for enough restrictions to make b^+ lie in a one-dimensional space while b^- need only be subject to weak damping or smoothness restrictions for identification, is the order-of-magnitude difference in stringency to which I referred above.

At this point two lines of objection to the above argument-by-example may occur to you. In the first place, might we not be dealing with a hairline category of exceptional cases? For example, what if we ruled out all finite-order Markov processes for p in the preceding example? This is a small subset of all stationary processes, yet ruling it out would invalidate the dimensionality argument used to show b^+ not to be identified. In the second place, isn't it true that in most applications, c, b^+, and b^- are not separately parameterized, so that information about c and b^-, which we agree is available, will help us determine b^+? The latter line of objection is correct as far as it goes, and will be discussed below. The former line of objection is not valid, and the next paragraphs contain an argument that this identification problem is present no matter what stationary process generates p in the example. Since the argument gets technical, readers with powerful intuition may wish to skip it.

[8] Though it does not matter for our argument, in actual examples c, b^-, and b^+ may be generalized functions, so that $c*y$, e.g., may be a linear combination of derivatives of y.

If, say, b^+ is square-integrable and p is a stationary process with bounded spectral density, then the term $b^{+}*\hat{p}_t(t)$ in (1) itself is a stationary process. Furthermore, the prediction error from using $\hat{b}^+$ in (1) when b^+ is correct is a stationary stochastic process with variance given by

$$s^2(\hat{b}^+, b^+) = \|\hat{b}^+ - b^+\|_R^2, \qquad \text{where}$$

$$(6) \qquad \|f\|_R = \left[\int\int f(s)f(u)R(s,u)\,ds\,du\right]^{1/2} \qquad \text{and}$$

$$R(s,u) = E[\hat{p}_t(t+s)\hat{p}_t(t+u)].$$

Now under fairly weak restrictions requiring some minimal rate of damping in the autocorrelation function of p, we will have an inequality of the form

$$(7) \qquad |R(s,u)| < R_1(s)R_2(u-s), \qquad \text{for } u > s,$$

where $R_1(s) \to 0$ monotonically as $s \to \infty$ and R_2 is integrable.[9]

We define the translation operator by $T^s f(t) = f(t-s)$. Then from (7) and the definition of $\| \ \|_R$ in the second line of (6) we get, for $f(t) = 0$, $t < 0$,

$$(8) \qquad \|T^s f\|_R^2 \leq R_1(s) \int\int f(v)f(u)R_2(u-v)\,du\,dv.$$

Therefore $\|T^s f\|_R \to 0$ as $s \to \infty$, and we have proved the following proposition.

PROPOSITION: *If the moving average representation of p has a weighting function which is $O(s^{-2})$, then no translation-invariant functional is continuous with respect to the norm $\| \ \|_R$ defined in the second line of* (6).

Obviously this means that the L_2 and L_1 norms are not continuous with respect to $\| \ \|_R$. Putting this result in somewhat more concrete terms, we have shown that when p meets the conditions of the proposition, we can make the effect of estimation error on the fit of equation (1), given by $s^2(\hat{b}^+, b^+)$, as small as we like, while at the same time making the integrated squared or absolute deviations between b^+ and $\hat{b}^+$ as large as we like. The fit of equation (1) cannot be used to fix the shape of b^+, under these general conditions.

Somehow, then, we must use information on the relation of c and b^- to b^+ or other prior information to put substantial restrictions on b^+ a priori. Restriction on the relation of c and b^- to b^+ are especially promising, since c and b^- are in general identified without strong prior restrictions. For example, a symmetry restriction, requiring c^{-1} and b^+ to be mirror images, which does emerge from some optimization problems, would be enough to identify b^+. On the other hand, many behavioral frameworks leave parameters which economists would not ordinarily fix a priori dependent on the difference in shape between b^+ and c^-,

[9] The process p has the moving average representation $p(t) = a*e(t)$, with e white noise. Then $R(s,u) = \int_s^\infty a(v)a(v+u-s)\,dv$ for $s < u$. If we then assume, for example, that $a(s)s^2$ is bounded, which would follow if p were assumed to have a spectral density with integrable fourth derivative, then it is not hard to verify that R_1 can be taken to have the form $A(1+s)^{-3}$ and R_2 the form $B(1+u-s)^{-2}$.

which is precisely what will be hard to estimate. The following example illustrates the point.

Suppose firms maximize the expected discounted present value of revenue, given by

$$(9)\qquad \int_s^\infty e^{-\rho(t-s)}(Q(t)-P(t)(\delta K(t)+\dot{K}(t))-\Theta(\dot{K}(t)+\delta K(t))^2)\,dt$$

subject to

$$(10)\qquad Q(t)=\alpha K(t)-\lambda K^2(t).$$

The interpretation is that $P(t)$ is the price of the fixed factor input K, δ is the depreciation rate, ρ is the interest rate, and Θ determines the output foregone as the rate of gross investment increases.

The first-order conditions for a solution to this equation give us

$$(11)\qquad (D^2-\rho D-(\lambda/\Theta)-\delta\rho-\delta^2)K=(\delta+\rho-D)P/2\Theta-\alpha/2\Theta,$$

where D is the derivative operator. Firms taking P as exogenous will, at each s, choose a solution to (11) from time s onward, using $\hat{P}_s(t+s)$ in their computations in place of P itself. Since the P_s series and the problem's initial conditions change with s, (11) itself does not apply to observed K and P. If, however, we assume that firms have enough foresight not to choose solutions to (11) along which K diverges exponentially from its static optimum value, then we will find the following equation holding at each s:

$$(12)\qquad (D+M_1)K(s)=(D+M_2)^{-1}(\delta+\rho-D)\hat{P}_s(s)/2\Theta-(\alpha/2\Theta M_2),$$

where M_1 and M_2 are the two roots (with signs reversed) of the polynomial in D on the left of (11). These two roots will always be of opposite sign, and M_2 is negative, so that $(D+M_2)^{-1}$ operates only on the future of the function to which it is applied. It is not hard to verify that the roots have the form

$$(13)\qquad M_1=\tfrac{1}{2}[\sqrt{\rho^2+4((\lambda/\Theta)+\rho\delta+\delta^2)}-\rho];\qquad M_2=-\rho-M_1.$$

In the case $\rho=\delta=0$, we get $-M_1=M_2$, so that from knowledge of M_1 we obtain M_2 and thereby the entire operator applied to expected future P in (12). The only way identification could be frustrated would be for expected future P to show no variation at all, so that K itself became constant. It should be noted, that this could, of course, happen without P being constant. If P were a moving average process of the form $P=a^*e$, with $a(s)=0$ for $s>T$, and if at time t firms know only the history of P up to time $t-T$, then $\hat{P}_s$ is identically equal to P's unconditional mean. In the more interesting case where the information set includes current P, identification problems arise only with ρ and $\delta\neq 0$.

With ρ and δ non-zero, equation (12) involves five coefficients, all functions of the five unknown parameters of the model. If $\hat{P}_s$ were a stationary process, we would be justified in following our instincts in declaring all structural parameters identified. However, in fact identification depends on there being sufficient independent variation between the time path of expected future *levels* of P and expected future *derivatives* of P. With ρ or δ nonzero, the operator applied to $\hat{P}_s$ on the right side of (12) differs from that applied to K on the left by more than a reflection. Even if we know ρ a priori (by reading the financial press), first-order Markov behavior for P implies that δ is not identified (assuming still that past P makes up the information set). In the first-order Markov case with $\dot{P} = -rP + e$, we have $(d/dt)\hat{P}_s(t) = -r\hat{P}_s(t)$ for all $t > s$. Thus (12) becomes, when we replace $\hat{P}_s$ by its observable counterpart,

$$(D + M_1)K(s) = -(\delta + \rho + r)P(s)/2\Theta(M_2 + r) + (\alpha/2\Theta M_2). \tag{14}$$

The separate coefficients on $\hat{P}_s$ and its derivative in (12) have merged into one, leaving the structural parameters unidentifiable from the relation of the observable variables. In particular, one can see by examining (14) and (13) that one could vary δ, Θ, α, and λ in such a way as to leave coefficients in (14) unchanged even for fixed ρ and r, so that knowing r from the data and ρ a priori will not suffice to identify the model.

D. *Concrete Implications*

Were any one of the categories of criticism of large-model identification outlined in the preceding three sections the only serious criticism, it would make sense to consider existing standard methodology as a base from which to make improvements. There is much good work in progress on estimating and specifying systems of demand relations. Some builders of large models are moving in the direction of specifying sectoral behavior equations as systems.[10] There is much good work in progress on estimating dynamic systems of equations without getting fouled up by treating knowledge of lag lengths and orders of serial correlation as exact. There is much good work treating expectations as rational and using the implied constraints in small systems of equations. Rethinking structural macromodel specification from any one of these points of view would be a challenging research program. Doing all of these things at once would be a program which is so challenging as to be impossible in the short run.

On the other hand, there is no immediate prospect that large-scale macromodels will disappear from the scene, and for good reason: they are useful tools in forecasting and policy analysis.

How can the assertion that macroeconomic models are identified using false assumptions be reconciled with the claim that they are useful tools? The answer is that for forecasting and policy analysis, structural identification is not ordinarily needed and that false restrictions may not hurt, may even help a model to function in these capacities.

Textbook discussions sometimes suggest that structural identification is necessary in order for a model to be used to analyze policy. This is true if "structure"

[10] For example, Fair [**6**] takes this approach in principle, though his empirical equations are specified with a single-equation approach to forming lists of variables. Modigliani [**20**] reports that the MPS model (like the Fair model) has interest rates turning up in many household behavioral equations.

and "identification" are interpreted in a broad way. A structure is defined (by me, following Hurwicz [**12**] and Koopmans [**13**]) as something which remains fixed when we undertake a policy change, and the structure is identified if we can estimate it from the given data. But in this broad sense, when a policy variable is an exogenous variable in the system, the reduced form is itself a structure and is identified. In a supply and demand example, if we contemplate introducing an excise tax into a market where none has before existed, then we need to be able to estimate supply and demand curves separately. But if there has previously been an excise tax, and it has varied exogenously, reduced form estimation will allow us accurately to predict the effects of further changes in the tax. Policy analysis in macromodels is more often in the latter mode, projecting the effect of a change in a policy variable, than in the mode of projecting the effect of changing the parameters of a model equation.

Of course, if macroeconomic policy-makers have a clear idea of what they are supposed to do and set about it systematically, macroeconomic policy variables will not be at all exogenous. This is a big if, however, and in fact some policy variables are close enough to exogenous that reduced forms treating them or their proximate determinants as exogenous may be close to structural in the required sense.[11] Furthermore, we may sometimes be able to separate endogenous and exogenous components of variance in policy variables by careful historical analysis, in effect using a type of instrumental variables procedure for estimating a structural relation between policy variables and the rest of the economy.

Lucas' [**16**] critique of macroeconomic policy making goes further and argues that, since a policy is not really just one change in a policy variable, but rather a rule for systematically changing that variable in response to conditions, and since changes in policy in this sense must be expected to change the reduced form of existing macroeconometric models, the reduced form of existing models is not structural even when policy variables have historically been exogenous—institution of a nontrivial policy would end that exogeneity and thereby change expectation formation rules and the reduced form.

There is no doubt that this position is correct, if one accepts this definition of policy formation. One cannot choose policy rules rationally with an econometric model in which the structure fails to include realistic expectation formation. However what practical men mean by policy formation is not entirely, probably not even mainly, choice of rules of this sort. Policy makers do spend considerable effort in comparing projected time paths for variables under their control. As Prescott and Kydland [**23**] have recently shown, making policy from such projections, while ignoring the effect of policy on expectation-formation rules, can lead to a very bad time path for the economy, under some assumptions. Or, as Sargent and Wallace [**29**] have shown, it can on other assumptions be merely a charade, with the economy's real variables following a stochastic process which cannot be affected by any such exercises in choice of time paths for policy variables.

[11] We shall see below, for example, that in Germany and the U.S. money supply, while not entirely exogenous, has an exogenous component which accounts for much of its variance.

I do not think, however, that practical exercises in conditional projection of effects of policy are either charades or (usually) Prescott and Kydlund's case of "Peter White" policy making.[12] Suppose it were true that the policy rule did make a difference to the economy. There are many ways to argue that this is true in the face of Sargent's [**28**] or Sargent and Wallace's [**29**] analysis, all being suggestions for forms of non-neutrality of money. To be concrete, suppose that the real variables of the economy do follow a stochastic process independent of the money supply rule but that for some reason the rate of inflation enters the social utility function.[13] Then the optimal form for macropolicy will be stabilization of the price level.[14] If we could agree on a stable model in which all forms of shock to the aggregate price level were specified a priori, then it would be easy in principle to specify an appropriate function mapping past values of observed macrovariables into current levels of policy variables in such a way as to minimize price variance. However, if disturbances in the economy can originate in a variety of different ways, the form of this policy reaction function may be quite complicated. It is much easier simply to state that policy rule is to minimize the variance of the price level. Furthermore, if there is uncertainty about the structure of the economy, then even with a fixed policy objective function, widely understood, the form of the dependence of policy on observed history will shift over time as more is learned about (or as opinions shift about) the structure of the economy. One could continually re-estimate the structure and, each period, re-announce an explicit relation of policy variables to history. However it is simpler to announce the stable objective function once and then each period solve only for this period's policy variable values instead of computing a complete policy reaction function. This is done by making conditional projections from the best existing reduced form model, and picking the best-looking projected future time path. Policy choice is then most easily and reliably carried out by comparing the projected effects of alternative policies and picking the policy which most nearly holds the price level constant. Accurate projections can be made from reduced form models fit to history because it is not proposed to change the policy rule, only to implement effectively the existing rule.

[12] Peter White will ne'er go right/ Would you know the reason why?/ He follows his nose where'er he goes/ And that stands all awry.—Nursery Rhyme.

[13] It is a little hard to imagine why the rate of inflation should matter if it affects no real variables. A more realistic and complicated scenario would suppose that there are costs to writing contingencies into contracts, and enforcing contracts with complicated provisions, so that a macropolicy which stabilizes certain macroeconomic aggregates—prices, wages, unemployment rates, etc.—may simplify contract-writing and thus save resources. This has been made the basis of an argument against inflation by Arthur Okun [**22**].

[14] Discussion of such a policy seems particularly appropriate in the Fisher-Schultz lecture, as Irving Fisher supported such a policy: "The more the evidence in the case is studied, the deeper will grow the public conviction that our shifting dollar is responsible for colossal social wrongs and is all the more at fault because those wrongs are usually attributed to other causes. When these who can apply the remedy realize that our dollar is the great pickpocket, robbing first one set of people, then another, to the tune of billions of dollars a year, confounding business calculations and convulsing politics, and, all the time, keeping out of sight and unsuspected, action will follow and we shall secure a boon for all future generations, a true standard for contracts, a stabilized dollar" [**7**].

In fact, it appears to be a mistake to assume that the economy's real variables follow a process even approximately unrelated to nominal aggregates. Thus stabilization of the price level alone is not likely to be the best policy. However, it is not clear that the existing pattern of policy in most countries, in which there is weight given to stabilization of inflation, unemployment, and income distribution, is very far from an optimal policy. Simply implementing policy according to these objectives in the way the public expects is a highly nontrivial task, and one in which reduced-form modeling may be quite useful.

To summarize the argument, it is admitted that the task of choosing among policy regimes requires models in which explicit account is taken of the effect of policy regime on expectations. On the other hand, it is argued that the choice of policy regime probably does have important consequences, and that an optimal regime and the present regime in most countries are both most naturally specified in terms of the effects of policy on the evolution of the economy, rather than in terms of the nature of the dependence of policy on the economy's history. Effectively implementing a stable optimal or existing policy regime therefore is likely appropriately to involve reduced-form modeling and policy projection.

But I have argued earlier that most of the restrictions on existing models are false, and the models are nominally over-identified. Even if we admit that a model whose claimed behavioral interpretation is spurious may have a useful reduced form, isn't it true that when the spurious identification results in restrictions on the reduced form, the reduced form is distorted by the false identifying restrictions? The answer is yes and no. Yes, the reduced form will be infected by false restrictions and may thereby become useless as a framework within which to do formal statistical tests of competing macroeconomic theories. But no, the resulting infection need not distort the results of forecasting and policy analysis with the reduced form. Much recent theoretical work gives rigorous foundation for a rule of thumb that in high dimensional models restricted estimators can easily produce smaller forecast or projection errors than unrestricted estimators even when the restrictions are false. Of course very false restrictions will make forecasts worse, but in large macromodels restrictions very false in the sense of producing very bad reduced-form fits are probably usually detected and eliminated. Thus models whose self-proclaimed behavioral interpretation is widely disbelieved may nonetheless find satisfied users as tools of forecasting and policy projection.

Because existing large models contain too many incredible restrictions, empirical research aimed at testing competing macroeconomic theories too often proceeds in a single- or few-equation framework.[15] For this reason alone it appears worthwhile to investigate the possibility of building large models in a style which does not tend to accumulate restrictions so haphazardly. In addition, though, one might suspect that a more systematic approach to imposing restrictions could lead to capture of empirical regularities which remain hidden to the standard procedures and hence lead to improved forecasts and policy projections.[16]

[15] Modigliani [**20**] has used the MPS model as an arena within which to let macroeconomic theories confront each other, however.

[16] The work of Nelson [**21**] and Cooper and Nelson [**5**] provides empirical support for this idea.

Empirical macroeconomists sometimes express frustration at the limited amount of information in economic time series, and it does not infrequently turn out that models reflecting rather different behavioral hypotheses fit the data about equally well. This attitude may account for the lack of previous research on the possibility of using much less parsimoniously parameterized multiple-equation models. It might be expected that in such a model one would find nothing new except a relatively larger number of "insignificant" t statistics. Forecasts might be expected to be worse, and the accurate picture of the relation of data to theory one would obtain might be expected to be simply the conclusion that the data cannot discriminate between competing theories.

In the next section of this paper we discuss a general strategy for estimating profligately (as opposed to parsimoniously) parameterized macromodels, and present results for a particular relatively small-scale application.

2. AN ALTERNATIVE STRATEGY FOR EMPIRICAL MACROECONOMICS

It should be feasible to estimate large-scale macromodels as unrestricted reduced forms, treating all variables as endogenous. Of course, some restrictions, if only on lag length, are essential, so by "unrestricted" here I mean "without restrictions based on supposed a priori knowledge." The style I am suggesting we emulate is that of frequency-domain time series theory (though it will be clear I am not suggesting we use frequency-domain methods themselves), in which what is being estimated (e.g., the spectral density) is implicitly part of an infinite-dimensional parameter space, and the finite-parameter methods we actually use are justified as part of a procedure in which the number of parameters is explicitly a function of sample size or the data. After the arbitrary "smoothness" or "rate-of-damping" restrictions have been used to formulate a model which serves to summarize the data, hypotheses with economic content are formulated *and tested* at a second stage, with some perhaps looking attractive enough after a test to be used to further constrain the model. Besides frequency-domain work, such methods are implicit or explicit in much distributed lag model estimation in econometrics, and Amemiya [**1**] has proposed handling serial correlation in time domain regression models in this style.

The first step in developing such an approach is evidently to develop a class of multivariate time series models which will serve as the unstructured first-stage models. In the six-variable system discussed below, the data are accepting of a relatively stringent limit on lag length (four quarters), so that it proves feasible to use an otherwise unconstrained (144 parameter) vector autoregression as the basic model. In the larger systems one will eventually want to study this way, some additional form of constraint, beyond lag length or damping rate constraints, will be necessary. Finding the best way to do this is very much an open problem. Sargent and I [**28**] have published work using a class of restricted vector time series models we call index models in macroeconomic work, and I am currently working on applying those methods to systems larger than that explored below. Priestly, Rao, and Tong [**24**] in the engineering literature and Brillinger [**4**] have suggested related classes of models. All of these methods in one way or another aim at limiting the nature of cross-dependencies between variables. If every variable is allowed to influence every other variable with a distributed lag of reasonable length, without restriction, the number of parameters grows with the square of the number of variables and quickly exhausts degrees of freedom. Besides the above approaches, it seems to me worthwhile to try to invent Bayesian approaches along the lines of Shiller's [**30**] and Leamer's [**14**] work on distributed lags to accomplish similar objectives, though there is no obvious generalization of those methods to this sort of problem.

The foregoing brief discussion is included only to dispose of the objection that the kind of analysis I carry out below could not be done on systems comparable in size to large-scale macromodels currently existing.

What I have actually done is to fit to quarterly, postwar time series for the U.S. and West Germany (F.R.G.) on money, GNP, unemployment rate, price level, and import price index, an unconstrained vector autoregression. Before describing the results in detail, I will set out the two main conclusions, to help light the way through the technical thickets to follow.

Phillips curve equations or wage-price systems of equations are often estimated treating only wages or only wages and prices jointly as endogenous. The "price" equation is often treated as behavioral, describing the methods firms use to set prices for the products, while the wage or Phillips curve equations are often discussed as if they describe the process of wage bargaining or are in some way connected to only those variables (unemployment in particular) which we associate with the labor market. In the estimated systems for both the U.S. and F.R.G., the hypothesis that wage or price or the two jointly can be treated as endogenous, while the rest of the system is taken as exogenous, is decisively rejected. Estimates conditioned on this hypothesis would then be biased, if the equations did have a structural interpretation. On the other hand, the estimated equations, having been allowed to take the form the data suggests, do not take the forms commonly imposed on them. Unemployment is not important in the estimated wage equations, while it is of some importance in explaining prices. The money supply has a direct impact on wages, but not on prices.[17]

Sargent [**27**] has recently put forward a more sophisticated version of the rational expectations macromodel he had analyzed in earlier work. He shows that the implication of his earlier model that a variable measuring real aggregate labor or output should be serially uncorrelated is not a necessary adjunct to the main policy implication of his earlier model: that deterministic monetary policy rules cannot influence the form of time path of real variables in the economy. We shall see that such an elaborate model can take two extreme forms, one in which the nature of cyclical variation is determined by the parameters of economic behavioral relations, the other in which unexplained serially correlated shocks to technology and tastes account for cyclical variation. The more satisfying extreme form of the model, with a behavioral explanation for the form of the cycle, implies

[17] Some empirical macroeconomists in the U.S. have begun to reach similar conclusions. Wachter [**32**] has introduced money supply into "wage" equations, and R. J. Gordon [**8**] has taken the view that equations of this type ought to be interpreted as reduced forms.

that the real variables in the economy, including relative prices, ought to form a vector of jointly exogenous variables relative to the money supply, the price level, or any other nominal aggregate. This is very far from holding true in the system estimated here. For the U.S., money supply, and for the F.R.G. the price level shows strong feedback into the real economy.

A. *Methodological Issues*

Since the model being estimated is an autoregression, the distribution theory on which tests are based is asymptotic. However, for many of the hypotheses tested the degrees of freedom in the asymptotic χ^2 distribution for the likelihood ratio test statistic is not a different order of magnitude from the degrees of freedom left in the data after fitting the model. This makes interpretation of the tests difficult, for a number of reasons. Even if the model were a single equation and not autoregressive, we know that F statistics with similar numerator and denominator degrees of freedom are highly sensitive to non-normality, in contrast to the usual case of numerator degrees of freedom much smaller than denominator degrees of freedom, where robustness to non-normality follows from asymptotic distribution theory. This problem is worse in the case where some coefficients being estimated are not consistently estimated, as will be true when dummy variables for specific periods are involved. If constraints being tested involve coefficients of such variables (as do the tests for model stability below), even F statistics with few numerator degrees of freedom will be sensitive to non-normality. In the case which seems most likely, where distributions of residuals have fat tails, this creates a bias toward rejection of the null hypothesis.

There is a further problem that different, reasonable-looking, asymptotically equivalent formulas for the test statistic may give very different significance levels for the same data. In the single equation case where k linear restrictions are being tested, the usual asymptotic distribution theory suggests treating $T \log(1+kF/(T-k))$ as $\chi^2(k)$, where F is the usual F statistic and T is sample size. Where k is not much less than T, significance levels of the test drawn from asymptotic distribution theory may differ substantially from those of the exact F test. Of course k times the F statistic is also asymptotically $\chi^2(k)$ and a test based on k is asymptotically equivalent to the likelihood ratio test. Since treating kF as χ^2 ignores the variability of the denominator of F, such a procedure has a bias against the null hypothesis relative to the F test. The usual likelihood ratio test shares this bias. Furthermore, over certain ranges of values of F, including the modal value of 1.0, the usual likelihood ratio is larger than kF and thus even further biased against the null hypothesis.

In the statistical tests reported below, I have computed likelihood ratios as if the sample size were $T-k$, where k is the total number of regression coefficients estimated divided by the number of equations.[18] This makes the likelihood ratio

[18] That is, the usual test statistic, $T(\log|D_R|-\log|D_U|)$ is replaced by $(T-k)(\log|D_R|-\log|D_U|)$, where D_R is the matrix of cross products of residuals when the model is restricted; D_U is the same matrix for the unrestricted model.

tend to be smaller than kF in the single-equation case, though whether this improves the applicability of the distribution theory much is certainly debatable. In any case we shall see that most hypotheses entertained are rejected, so this modification of the usual likelihood ratio test in favor of the null hypothesis would not change the main results.

The procedures adopted here are obviously only ad hoc choices; and the problem of finding the appropriate procedure in situations like this deserves more study.[19]

B. *Stability Over Time and Lag Length*

The six data series used in the model for each country—money, real GNP, unemployment, wages, price level, and import prices—are defined in detail in the data appendix. Each series except unemployment was logged, and the regressions all included time trends. For Germany, but not the U.S., seasonal dummy variables were included. Most but not all the series were seasonally adjusted. The period of fit was 1958–76 for Germany, 1949–75 for the U.S.

The estimated general vector autoregressions were initially estimated with lag lengths of both four and eight, and the former specification was tested as a restriction of the latter. In both countries the shorter lag length was acceptable. The $\chi^2(144)=166.09$ for the U.S. and $\chi^2(144)=142.53$ for the F.R.G. The corresponding significance levels are about 0.20 and 0.50. In all later work the shorter lag length was used.

The sample-split tests we are about to consider were all executed by adding a set of dummy variables to the right-hand-side of all regressions in the system, accounting for all variation within the period being tested. The likelihood ratio statistic was then formed as described in Section 2.A, comparing the fit of the system with and without these dummy variables. Because non-normal residuals are not "averaged" in forming such a test statistic, the statistics are probably biased against the null hypothesis when degrees of freedom in the test statistic are small. On the other hand, they are probably biased in favor of the null hypothesis when the degrees of freedom approach half the sample size, at least when compared with the single-equation F statistic.

For both West German and U.S. data, splitting the sample at 1965 (with the dummy variables applied to the post-65 period) shows no significant difference between the two parts of the sample. However, again for both countries, splitting

[19] Some readers have questioned the absence in this paper of a list of coefficients and standard errors, of the sort usually accompanying econometric reports of regression estimates. The autoregressive coefficients themselves are difficult to interpret, and equivalent, more comprehensible, information is contained in the MAR coefficients, which are presented in the charts. Because estimated AR coefficients are so highly correlated, standard errors on the individual coefficients provide little of the sort of insight into the shape of the likelihood we ordinarily try to glean from standard errors of regression coefficients. The various χ^2 tests on block triangularity restrictions which are presented below provide more useful information. However, it must be admitted that it would be better were there more emphasis on the shape of the sum-of-squared-residuals function around the maximum than is presented here. Ideally, one would like to see some sort of error bound on the MAR plots, for example; I have not yet worked out a practical way to do this.

the sample at the first quarter of 1971 or 1958 (using dummy variables for the smaller segment of the sample) shows a significant difference between the two parts of the sample. For the 1971 split the marginal significance levels of the test are 0.003 for Germany and less than 10^{-4} for the U.S. However, as can be seen from Table I, in both countries the difference between periods is heavily concentrated in the equation for price of imports. Testing the five other equations in the system, treating the import variable as predetermined, yields marginal significance levels of 0.07 for the U.S. and 0.15 for Germany.[20]

TABLE I
TESTS FOR MODEL HOMOGENEITY: 1953–1971 vs. 1972–1976 (Germany); 1949–1971 vs. 1972–1975 (U.S.)[a]

Equation	U.S.	Germany
M	$F(16, 54) = 1.84$	$F(20, 47) = 2.88$
RGNP	$= 1.10$	$= 1.94$
U	$= .92$	$= .76$
W	$= .61$	$= .42$
P	$= 1.75$	$= .74$
PM	$= 5.10$	$= 4.10$
Overall	$\chi^2(96) = 160.05$	$\chi^2(120) = 170.76$
first five equations	$\chi^2(80) = 99.16$	$\chi^2(100) = 114.58$

[a] All χ^2 test statistics are computed as reported in footnote 18. They are likelihood ratio test statistics conditioned on the initial observations. The "unrestricted" model is one in which a separate parameter is introduced to explain each variable in each of the periods of the latter time interval. The F test statistics are the corresponding single-equation test statistics computed in the usual way. They are, of course, not actually distributed as F here because of the presence of lagged dependent variables.

The 1971 date was originally chosen to correspond to the beginning of a period of price controls in the U.S. It appears, however, that in both the U.S. and Germany the major source of difference between the periods comes out of the 1973–74 commodity price boom, with little evidence of a strong effect of price controls in the U.S.

For the sample split at 1958, the marginal significance levels are 0.0007 for the U.S. and 0.003 for Germany ($\chi^2(216) = 286$ and $\chi^2(120) = 178$, respectively). However as can be seen from Table II the shift is again concentrated in the price-of-imports equation for the U.S. For the U.S., the marginal significance level of the test for the five other equations is 0.15, though four of the five equations have considerably lower significance levels when we consider the

[20] While the test statistics used in this case have the same form as those for other hypotheses tested in this paper, they differ in not exactly being likelihood ratio tests. This is because they use conditional likelihood given the price of imports, even though it is admitted that the price of imports is only predetermined, not exogenous. The asymptotic distribution theory continues to apply (or not apply) to these statistics as for the bona fide likelihood ratios, however. It may affect the reader's interpretation of these results to know that if the import price variable is omitted from the system in the U.S., the significant change at 1971 appears more evenly spread across the five equations. My initial work with U.S. data was with such a five-equation system, and the import price variable was added to the system with the suspicion that it might concentrate the structural shift.

TABLE II

TESTS FOR MODEL HOMOGENEITY: 1953–1957 vs. 1958–1976 (Germany); 1949–1957 vs. 1958–1975 (U.S.)

Equation	U.S.	Germany
M	$F(36, 46) = .69$	$F(20, 47) = 1.56$
$RGNP$	$= 2.57$	$= .92$
U	$= 1.83$	$= 3.77$
W	$= 1.94$	$= 3.71$
P	$= 2.81$	$= 3.00$
PM	$F(36, 30) = 6.31$	$= .97$
Overall	$\chi^2(216) = 286.69$	$\chi^2(120) = 178.00$
first five equations	$\chi^2(180) = 199.92$	$\chi^2(100) = 152.08$

[a] Same comments apply as for Table I.

individual F tests. For the German data, the 1958 sample split was chosen because Robert J. Gordon, working with similar data in recent research, had foregone attempting the interpolations and splices necessary to extend the period of fit back before 1958. Thus it is quite possible that the shift we detect is mainly caused by noncomparability in the data for the earlier period. At least some of the shift comes from changed coefficients of the seasonal dummy variables in the wage equation, which fits the explanation of noncomparable data.

These tests suggest that, though the equations for price of imports show strong effects of other variables no matter the period to which they are fitted, the equations are not stable. In computing tests of hypotheses, therefore, I have in each case avoided relying on a maintained hypothesis that there is a stable import price equation. On the other hand, in preparing projections of responses of the system to shocks, I have always included an import price equation fit, one way or another, to the whole sample, because the responses of import prices to other variables, though not stable, are strong.

Even when the import price equation is excluded, it is apparent that individual equations often show suspiciously large F statistics for the sample split hypothesis. Whether it is better to treat these mainly as due to non-normality—occasional outlier residuals—while maintaining the hypothesis of a stable linear structure, is a question which deserves further exploration. With as many parameters as are estimated in this model, it is probably not possible without longer time series than are yet available to distinguish clearly between instability in the form of occasional outlier residuals and instability in the form of parameter shifts.

C. *General Descriptions of the Estimated Systems*

Autoregressive systems like these are difficult to describe succinctly. It is especially difficult to make sense of them by examining the coefficients in the regression equations themselves. The estimated coefficients on successive lags tend to oscillate, and there are complicated cross-equation feedbacks. The common econometric practice of summarizing distributed lag relations in terms of their implied long run equilibrium behavior is quite misleading in these systems. The estimated U.S. system, for example, is a very slowly damped oscillatory system. For the first 40 quarters or so of a projection, nominal variables move in phase, as one would expect. But after this period (which is about half a cycle for the system's long oscillations) the cycles in the various nominal variables move out of phase. Clearly the infinitely long run behavior of this system is nonsensical, though over any reasonable economic forecasting horizon the system is quite well-behaved.

The best descriptive device appears to be analysis of the system's response to typical random shocks. Except for scaling, this is equivalent to tracing out the system's moving average representation by matrix polynomial long division. As will be seen below, the resulting system responses are fairly smooth, in contrast to the autoregressive lag structures, and tend to be subject to reasonable economic interpretation.[21]

The "typical shocks" whose effects we are about to discuss are positive residuals of one standard deviation unit in each equation of the system. The residual in the money equation, for example, is sometimes referred to as the "money innovation," since it is that component of money which is "new" in the sense of not being predicted from past values of variables in the system. The residuals are correlated across equations. In order to be able to see the distinct patterns of movement the system may display it is therefore useful to transform them to orthogonal form. There is no unique best way to do this. What I have done is to triangularize the system, with variables ordered as M, Y, U, W, P, PM. Thus the residuals whose effects are being tracked are the residuals from a system in which contemporaneous values of other variables enter the right-hand-sides of the regressions with a triangular array of coefficients. The M equation is left unaltered, while the PM equation includes contemporaneous values of all other variables on the right. An equivalent way to think of what is being done is to note that what we call the M innovation is assumed to disturb all other variables of the system instantly, according to the strength of the contemporaneous correlation of other residuals with the M residual, while the PM residual is only allowed to affect the PM variable in the initial period.

The charts at the end of the paper display, for each shock in the triangularized system, the reponse of all variables in the system.

The biggest differences between countries which emerge from perusal of the Charts are as follows.

(i) In the U.S. money innovations have very persistent effects on both money and other nominal variables. In Germany, money innovations, though larger, are much less persistent. The peak effect of the money innovation on real GNP is much bigger for the U.S. than for Germany.

[21] The moving average representation having smooth weights, in the sense of having weights whose Fourier transform is relatively small in absolute value at high frequencies, is equivalent to the spectral density being relatively small at high frequencies, and thus to the stochastic process itself being smooth. An autoregressive representation having smooth weights yields almost exactly the opposite condition on the spectral density. Thus we ought to expect non-smooth "lag distributions" in these vector autoregressions. The idea that the moving-average weights should be smooth in this sense suggests a possible Bayesian approach to estimating these systems which deserves further investigation.

(ii) Real GNP; innovations are associated with substantial inflation in Germany, not the U.S.

(iii) An unemployment innovation is followed by an apparent expansionary reaction from the monetary authority in the U.S., with a corresponding rise in real GNP and a fall in unemployment to a point farther below trend than the initial innovation was above trend. No such expansionary reaction in the money supply appears in Germany, where instead an unemployment innovation is followed by a drop in the money supply and a period of deflation and below-trend GNP.

(iv) Wage innovations are much bigger in Germany, and generate a temporary accomodating response there, unlike the U.S. The sustained negative movement in real GNP is smaller in Germany than in the U.S.[22]

(v) Price innovations are of negligible importance in the U.S. system. In the German system, price innovations are a major source of disturbance. There they produce a large, sustained drop in real GNP and persistent decline in the real wage, despite a temporarily accomodating response from the money supply.

(vi) Import price innovations have bigger and more persistent real effects in Germany, where the peak effect nearly matches that of price innovations and exceeds that of money innovations.

Common elements of the responses in the two countries are as follows.

(i) Money innovations tend temporarily to increase the real wage and real GNP and to reduce unemployment, with an opposite swing in these variables following.

(ii) Real GNP innovations are of similar magnitude and decay rapidly in their real effects in both countries.

(iii) Wage innovations are followed by sustained drops in real GNP in both countries.

(iv) Import price innovations are followed by movements of the same sign in prices and wages in both countries.

Price, wage, and import-price innovations induce patterns of response in both countries which are consistent with their representing supply shocks—they are followed by declines in real GNP. Under this interpretation it is not surpising that the real variables in Germany's smaller and more open economy should show greater sensitivity to such shocks than the real variables in the U.S. economy. This in turn might in part explain the German money supply's tendency temporarily to accommodate such shocks more than does the U.S. money supply. The German money supply tends to return more quickly to its trend path when it moves away from trend for any reason, and shows no indication of being used as a policy instrument to counteract unemployment. These differences could reflect differences in philosophy of money management, or in the costs and effectiveness of monetary policy actions between the two countries.

Tables III and IV provide a type of summary which is useful in locating the main channels of influence in the model. A variable which was strictly exogenous

[22] For reasons I have not yet discovered, the response to a wage innovation is quite different in a system fit to Gordon's data, which differs from mine mainly in the methods he used for interpolation and splicing. Gordon's data have wage innovations followed by much bigger negative movements in real GNP, and have somewhat smaller negative movements in GNP following a price innovation.

would, if there were no sampling error in estimates of the system, have entries of 1.00 in its diagonal cell in these tables, with zeroes in all other cells in its row of the tables. Exogeneity is equivalent to this condition that a variable's own innovations account for all of its variance. The price variable in Germany and the money supply variable in the U.S. both have more than half their variance accounted for by own-innovations at all time horizons shown, and the German money supply

TABLE III

PROPORTIONS OF FORECAST ERROR k QUARTERS AHEAD PRODUCED BY EACH INNOVATION: U.S. 1949–1975[a]

Forecast error in:	k	Triangularized innovation in: M	Y/P	U	W	P	PM
M	1	1.00	0	0	0	0	0
	3	.96	0	.03	0	0	0
	9	.73	0	.24	.02	0	0
	33	.54	0	.27	.09	0	.09
Y/P	1	.15	.85	0	0	0	0
	3	.35	.59	.04	.01	.01	0
	9	.30	.18	.37	.13	.00	.02
	33	.28	.15	.33	.16	.02	.06
U	1	.02	.35	.63	0	0	0
	3	.14	.49	.32	0	.03	0
	9	.26	.20	.41	.09	.02	.02
	33	.34	.14	.34	.13	.03	.03
W	1	.08	.05	.04	.84	0	0
	3	.17	.06	.07	.55	.09	.06
	9	.45	.02	.05	.25	.08	.16
	33	.64	.02	.19	.07	.02	.07
P	1	0	.04	.15	.24	.56	0
	3	.04	.01	.14	.36	.33	.12
	9	.14	.02	.12	.25	.11	.36
	33	.60	.02	.20	.07	.02	.09
PM	1	0	0	.06	.05	.08	.81
	3	.01	.01	.02	.13	.10	.75
	9	.06	.02	.13	.08	.03	.68
	33	.54	.03	.20	.04	.01	.18

[a] The moving average representation on which this table was based was computed from a system estimate in which the PM equation was estimated by generalized least squares in two steps. An initial estimate by ordinary least squares was used to construct an estimate of the ratio of residual variance in PM during 1949–71 to the residual variance in 1971–75, and this ratio was used (as if error-free) to re-estimate the equation by generalized least squares. This procedure is not in fact efficient, since once the break in residual variance in the PM equation is admitted, the usual asymptotic equivalence of single-equation and multiple-equation autoregression estimates breaks down.

variable has more than 40 per cent of its variance accounted for by own-innovations at all time horizons shown. No other variables have so much variance accounted for by own-innovations, indicating that interactions among variables are strong. The main source of feedback into money supply in the U.S. is unemployment innovations, while in Germany it is price innovations. Feedback into prices in Germany is diffused across all variables in the system. The responses of price to innovations in other variables are reasonable in form, tending to keep

TABLE IV

PERCENTAGES OF FORECAST ERROR k QUARTERS AHEAD PRODUCED BY EACH INNOVATION: WEST GERMANY 1958–1976[a]

Forecast error in:	k	Triangularized innovation in: M	Y/P	U	W	P	PM
M	1	1.00	0	0	0	0	0
	3	.84	.04	.05	.01	.04	.02
	9	.53	.04	.14	.08	.20	.01
	33	.39	.05	.13	.07	.27	.09
Y/P	1	.07	.93	0	0	0	0
	3	.14	.79	.01	.05	0	0
	9	.15	.47	.03	.06	.03	.25
	33	.13	.22	.05	.04	.42	.14
U	1	0	.03	.97	0	0	0
	3	.19	.09	.67	.03	.02	0
	9	.15	.10	.37	.02	.08	.29
	33	.09	.11	.15	.02	.50	.14
W	1	0	.03	.01	.96	0	0
	3	.11	.18	.01	.59	.03	.09
	9	.23	.23	.02	.23	.24	.05
	33	.21	.13	.08	.15	.31	.12
P	1	.02	.02	0	.10	.86	0
	3	.03	.06	.05	.09	.76	0
	9	.05	.13	.03	.05	.68	.06
	33	.08	.10	.04	.05	.67	.06
PM	1	.06	0	.02	0	.02	.89
	3	.04	0	.02	.01	.08	.85
	9	.10	.04	.09	0	.16	.61
	33	.06	.08	.04	.02	.57	.23

[a] Here the moving average representation was computed from a system estimate which made no allowance for non-stationarity over the period. Since stability over the sample period is sharply rejected by a test, the results here have to be taken as a kind of average of the different regimes which prevailed in the sample. The numbers reported here, like the plotted MAR's, apply to data with the two-sided interpolation referred to in the data appendix for price. Correction of the interpolation method to make it one-sided would make small but noticeable changes in the T table. The largest change would be increases of between .05 and .07 at the 33-quarter horizon in the proportion of variance in all variables but money and price itself accounted for by price innovations. For the U and PM rows these increases in the P column come almost entirely from the PM column, so that there are corresponding decreases in the proportion of variance accounted for by PM.

price roughly in line with the wage variable, so that it seems unreasonable to impose price exogeneity as a constraint on the system.[23]

In the U.S., over long horizons, money innovations are the main source of variation in all three price variables—wages, prices, and import prices. This is not true in Germany, reflecting the fact that money innovations do not persist long enough in Germany to induce the kind of smooth, neutral response in the price variables which eventually dominates in the U.S. data.

Table V displays the forecast standard errors over various forecasting horizons implied by the model when sampling error in the estimated coefficients is ignored. Actual forecast errors will of course be substantially bigger, even if the model's parameters do not change, because the statistical estimates are imperfect. Yet

[23] In fact, a test of the hypothesis that price is exogenous in West Germany yields an $F(20, 47) = 2.28$ and thus a marginal significance level of .01.

TABLE V

FORECAST STANDARD ERRORS, k QUARTERS AHEAD[a]

	k	U.S.	West Germany
M	1	.004	.011
	3	.010	.020
	9	.022	.029
	33	.055	.036
Y/P	1	.008	.009
	3	.016	.013
	9	.032	.018
	33	.036	.032
U	1	.002	.003
	3	.005	.003
	9	.010	.006
	33	.012	.011
W	1	.004	.008
	3	.008	.013
	9	.016	.023
	33	.037	.033
P	1	.004	.007
	3	.009	.011
	9	.018	.023
	33	.043	.035
PM	1	.014	.015
	3	.038	.029
	9	.075	.043
	33	.158	.077

[a] These figures are computed from the same MAR's used in computing Tables III and IV. They use the formula for the t-step-ahead expected squared forecast error in variable i:

$$s^2(i, t) = \sum_{j=1}^{p} \sum_{v=0}^{t-1} a_{ij}(v)^2 s_j^2,$$

where there are p variables in the system, $s_j^2 = s^2(j, 1)$ is the variance of the jth innovation, and $a_{ij}(v)$ is the coefficient on the vth lag of the jth innovation in the MAR equation for variable i.

even pretending, as this table does, that the estimated trend coefficients are known exactly, we see that forecast error rises steadily as the forecasting horizon lengthens, for nearly every variable. For a stationary process, forecast standard error tends to some upper bound as the horizon increases. Only real GNP and unemployment in the U.S. show much sign of this sort of behavior in this table, indicating that the estimated system is very slowly damped.

D. *Tests of Specific Hypotheses*[24]

Suppose we treat (y, m) as a vector process, where y is a vector of quantities and relative prices determined in the private sector and m is the money supply.

[24] The ideas expressed in this section are in part due to Thomas J. Sargent.

Assuming that (y, m) has no perfectly linearly predictable components, we can write

$$(15) \qquad y(t) = a^*e(t) + \lambda c^*f(t),$$

where $f(t) = m(t) - \varepsilon[m(t)|m(t-s), y(t-s), s>0]$ is the innovation in $m(t)$ and $e(t) = y(t) - \varepsilon[y(t)|m(t), m(t-s), y(t-s), s>0]$ is that part of the innovation in $y(t)$ whch is orthogonal to $f(t)$. Here "$\varepsilon[X|Z]$" means "best linear predictor of X based on Z," which coincides with conditional expectation only under normality assumptions.

There is a class of classical rational expectations models which imply that no form of policy rule for determining m can affect equation (15) except by affecting $a(0)$, the matrix λ, and the variance in f. Further these models imply that when the variance in f is kept at zero, $a(0)$ is invariant to changes in the policy rule.

To see how this conclusion might be derived, suppose that the ith type of economic agent chooses $x_i(t)$ according to an attempt to maximize some objective function which depends on $x_j(s)$ and $p_j(s)$ for all j and s (p_j is a price relative to some fixed numeraire). It is critical to this argument that money balances, even real money balances, not be included in X. This is a strong neutrality assumption. If real money balances were in X, *nominal* interest rates would have to enter p.) We assume the first-order conditions describing the solution to the jth agent's maximization problem are given by

$$(16) \qquad G_j(p, x, u_j, t) = 0,$$

where u is a vector of shifts in the objective functions of various agents in the economy. The whole past and future of p, x, and u_j enter (16) in principle, and we assume that the only effect of the t argument is to change the time origin of decision making—i.e., if $Lp(s) = p(s-1)$, then $G_j(Lp, Lx, Lu_j, t+1) = G_j(p, x, u_j, t)$.

We take the symbol "E_{tj}" to mean "expected value conditional on the information available to agents of type j at time t." If there is uncertainty, we assume that actual values of $x_j(t)$ are chosen by solving

$$(17) \qquad E_{tj}[G_j(p, x, u_j, t)] = 0,$$

as would be appropriate if the jth type of agent has an objective function which is a von Neumann-Morgenstern utility function. The system of equations of the form (17), together with market-clearing conditions (determining which "supply" x_j's have to add up to which sums of "demand" x_j's) are assumed to determine $x(t)$ and $p(t)$ at each t. In general the solution for $y(t) = (x(t), p(t))$ will involve all aspects of all the individual conditional distributions for future u_j's which enter the system. To reach our conclusions we need the drastic simplying assumption that only the first moments of these conditional distributions affect decisions, as would be true if all the objective functions in the system were quadratic. Thus we assume that (17) can be solved to yield a system of the form

$$(18) \qquad y(t) = H_t({}_{tj}\hat{u}, \text{ all } j, {}_{tj}\hat{y}(s), \text{ all } s<t, \text{ all } j)$$

where ${}_{tj}\hat{u}$ is a vector of functions of time with ith element ${}_{tj}\hat{u}_i(s) = E_{tj}[u_i(s)]$ and ${}_{tj}\hat{y}(s) = E_{tj}[y(s)]$. As with G in (17), we assume that H_t depends on time only through shift of time origin, so that

$$H_t(u, y(s), s < t) = H_{t+1}(Lu, Ly(s), s < t+1).$$

The economic substance of (18) can be summarized as an assertion that the only route available by which monetary policy can influence the levels of real variables in the system is by its possible effects on expected future levels of real shocks to the economy (the u's). Such effects are possible, according to this type of model, because some agents may observe some prices in terms of money more quickly than they observe relative prices. Thus if the monetary authority has a richer information set than some agents, it may be able to improve private-sector forecasts by making the money supply (and hence the aggregate price level) move in appropriate ways. Also, by introducing fluctuations in the aggregate price level which are not related to movements in u, the monetary authority can reduce the quality of private forecasts.[25] The versions of these models which imply that monetary policy is impotent assume that every private information set in the economy includes all the information available to the monetary authority.

Suppose we assume in particular that monetary policy is based on information contained in the history of the monetary aggregate, m, and the history of y alone. That is, $m(t) = F(y(t-s), m(t-s), s > 0) + f(t)$. Though we allow a random component $f(t)$ in $m(t)$, the assumption that the policy-makers' information set is restricted to the history of y and m is taken to mean that $f(t)$ is independent of $y(t+s) - E_{t-1}(y(t+s))$ for all s, where "E_{t-1}" means "conditional expectation given $y(s)$, $m(s)$ for $s \leqq -1$."

If equation (18) is linear and if ${}_{tj}\hat{y}(t-s) = y(t-s)$ for $s > 0$ (as is implied by our assumption that all private agents know the past history of y), we obtain from (18):

$$(19) \qquad E_{t-1}[y(t)] = H_t(E_{t-1}(u(s)), \text{all } s;\ y(t-s) \text{ for } s > 0).$$

Under our assumptions about policy, knowledge of past values of m can be of no help in forecasting u once past u is known (u is causally prior as a vector, in Granger's sense, relative to m). Now equation (15) is part of the joint moving average representation of the process (y, m), and we therefore have by construction

$$(20) \qquad E_{t-1}y(t) = \sum_{s=1}^{\infty} a(s)e(t-s) + \sum_{s=1}^{\infty} c(s)f(t-s).$$

By the definition of an innovation, we can use (19) to write

$$(21) \qquad y(t) = H_t(E_{t-1}(u(s)), \text{all } s;\ y(t-s) \text{ for } s > 0) + a(0)e(t) + \lambda c(0)f(t)$$

where f is, as in (15), the innovation in m when (y, m) is treated as a vector process and e is the component of the innovation in y orthogonal to f. Under our

[25] It is not obvious to me, however, that when different agents have different information sets the economy must be worse off with lower quality private forecasts.

assumptions about policy, $f(t)$ must be unrelated to the real disturbance process u. We assume further that from the past history of y and m, agents can calculate actual past values of u.[26] Then it is not hard to show that $e(t)$ must in fact be a linear transformation of the innovation vector for u. Thus the component of the right-hand-side of (21) which depends on the $E_{t-1}(u(s))$ series is a fixed linear combination of past values of $e(t)$. The weights in that linear combination depend on the structure of the u process only. Using these conclusions (and the linearity of H) to rewrite (21) we get

$$b_1^* y(t) = b_2^* e(t) + a(0)e(t) + \lambda c(0) f(t). \tag{22}$$

Assuming b_1 is invertible, we arrive finally at an interpretation of (20): $b_1^{-1} * b_2(s) = a(s)$ for $s > 0$, $b_1^{-1}(s)c(0) = c(s)$ for $s > 0$. Since b_1 and b_2 do not depend on the form of the monetary policy rule, the main conclusion announced at the beginning of this section follows. That $a(0)$ is invariant to changes in deterministic policy rules follows from (18) and our information assumptions, since when all private information sets include the information on which policy is based and $f(t) = 0$, all t, (18) determines $y(t)$ without regard to the form of the policy rule.

Up to this point, the theory which has been invoked has generated no explicit restrictions on the joint autoregressive representation of m and y, despite the fact that the theory clearly has strong implications for policy. The theory does, however, allow us to interpret the estimated MAR. Note that b_1 in (22) is determined by the coefficients on lagged y in H_t in (21), and that H_t in turn has been determined by the coefficients of the G_j functions in (16). Thus b_1 is determined by the parameters of the utility functions and production functions of economic agents. The lag distribution b_2, on the other hand, arises from the forecasts of u which enter H_t in (21). While b_2 is affected by the form of H, and hence by utility and production functions, it is zero if $u(t)$ is serially uncorrelated, regardless of the form of H_t.

Since c, the time path of y's response to m innovations, is just $b_1^{-1}c(0)$, it follows that c can change only in limited ways (via changes in the vector $c(0)$) in response to changes in the money supply rule.

Obviously if b_2 is zero and b_1 is a scalar, (22) implies that $y(t)$ is serially uncorrelated. In words, if there are no dynamics in utility functions or production functions (b_1 scalar) and if the shocks to utility functions, production functions, and endowments are serially uncorrelated ($b_2 = 0$), then this model implies that real variables are serially uncorrelated. The notion that market-clearing rational expectations models imply that real variables are serially uncorrelated has received a good deal of attention in the literature. Hall [**10**], e.g., explored it treating unemployment as the leading example of a real variable. Hall's simple model is a special case of the one considered here, in which b_1 is assumed to be scalar. Because of the scalar-b_1 assumption, Hall concludes that if real variables

[26] This is probably not restrictive. If u could not be deduced from past y and m (e.g., if it was of too high dimension) it could probably be redefined to satisfy our assumption without altering the argument.

are in fact strongly serially correlated, then the market-clearing rational expectations model has to "explain" serially correlated real variables via nonzero b_2. As he points out, this amounts to "explaining" the business cycle as serial correlation of unexplained origin in unmeasurable influences on the economy; such a theory does not really explain anything. Furthermore, it does in particular rule out the possibility that nearly all observed cyclical variation in real variables is attributable to monetary policy aberrations (i.e., to f) and therefore limits the potential gain to be expected from monetarist policy prescriptions.

The latter part of Hall's argument does make sense. However, Hall's conclusions depend on the notion that strong serial correlation in y is evidence of strongly nonzero b_2. In fact, it is easy to see from (22), as has been pointed out by Sargent, that large serially correlated movements in y can be explained without resort to powerful, serially correlated movements in u, simply by admitting the existence of dynamic elements in technology or tastes—i.e., nonscalar b_1. If serial correlation in y is explained by nonscalar b_1 without resort to nonzero b_2, however, a testable implication of the theory for the joint (y, m) autoregression still emerges: y should be causally prior relative to m. Formally, this is because with $b_2 = 0$, (22) expresses the innovation in y as a linear combination of current and past y's alone, without using past m's. Another way to put the same thing is to observe that, with $b_2 = 0$, the best linear one-step ahead forecast of $y(t)$ is $\Sigma_{s=1}^{\infty} b_1(s)y(t-s)$. That this formula not involve lagged m is precisely Granger's definition of m not causing y.

A test for block-exogeneity of the real sector thus has special interest in the context of this model. If the test were passed, the implication would be either that variance in u is small relative to that in f or that u does not have large serial correlation. In either case, serially correlated cyclical movements would be accounted for largely by the parameters of the objective functions G_j. If the test were not passed, the implication would be that b_2 is nonzero and the parameters of G_j do not account for the observed pattern of serial correlation. Note that this test does not bear on whether the rational expectations, market-clearing, neutral money theory is true—it only examines how well it accounts for the observed cyclical variability of the economy. It could be that b_2 is strongly nonzero and that u has large variance, yet still be true also that the model considered here is correct. In this case it could not be expected that changing monetary policy to reduce the variance in f, as most monetarists would suggest, would change the cyclical variability of the economy very much. But it would remain true that activist monetary policy could have only very limited effect in increasing the stability of the economy.[27]

Note that there is a certain paradoxical quality to a test for block-exogeneity of y as a test of the power of rational expectations market-clearing theory. That

[27] The model does not imply that policy has no real effects. By changing the variance of f, policy can in general affect $a(0)$ and λ, and with a given arbitrarily chosen objective function for policy it is unlikely that $f = 0$ will be the optimum choice. On the other hand, if the objective function of policy makers is related to those of economic agents in a reasonable way and important externalities are not present, it is likely to turn out that $f = 0$, making the private economy's forecasts as accurate as possible, is the optimal policy.

theory, in the form presented here, does suggest that setting $f = 0$, i.e. setting the level of the money supply according to a non-discretionary rule, would be good policy. In this sense the theory justifies monetarist conclusions. Yet we test the theory by looking for Granger causation of y by m—if we *find* "causation" of y by m, we *reject* the monetarist theory.[28] An old-fashioned monetarist, used to interpreting regressions of GNP on money as structural equations, would rightly find this conclusion ridiculous. To the extent that money does have important real effects which are not compensated by the operation of frictionless price adjustment and rational expectations, one would expect to find Granger-causality running from m to y. If, however, this is the source of a substantial component of the m-to-y covariance, then monetary stabilization policy has important effects and simple mechanical rules for setting m may be far from optimal.

To summarize, one can interpret block exogeneity tests within at least three frameworks of maintained hypotheses. Under rational expectations and inertialess prices, rejection of exogeneity of y implies that much cyclical variation is not reaction to monetary shock. Active stabilization policy can never be very helpful in this framework, but with y not exogenous, the implication is that it has not historically been the main source of cyclical variability. A "standard monetarist" who believed that money was very important but did not accept inertialess prices and rational expectations would find y-exogeneity hard to explain. In fact, the income on money regressions associated with this framework are insupportable as structural relations, unless m, not y, is Granger-causally prior. However this approach implies that mechanical monetary rules are unlikely to be optimal. Finally an unregenerate Keynesian, rejecting not only inertialess prices and market clearing but also the idea that money is a policy instrument of dominant importance, could interpret y exogeneity as indicative of a completely passive monetary policy, accounting for m-to-y time series correlations without resort to causal effects of autonomous policy-induced change in m on y. Rejection of y exogeneity thus weakens the "unregenerate Keynesian" position as well as the "rational expectations market-clearing" position.

In this case, as I think ought to be the case in most macroeconometric work, the data will obviously not determine directly the outcome of debate between various schools of thought; it does, however, influence the conflict by defining what battlefield positions must be.

The rational expectations market-clearing model involves numerous dubious assumptions. In manipulating it we implicitly or explicitly invoked existence and uniqueness results as well as the obviously false linearity and certainty-equivalence assumptions. By excluding real balances from the G_j, we assumed a strong neutrality property. We also relied on continuous market clearing and a very restrictive (and in my view unrealistic) definition of what policy can accom-

[28] Of course, as pointed out above, we don't actually reject the theory as false. As described above, causation of y by m only implies that the rational-expectations monetarist theory must allocate important business cycle variance to serial correlation in an unexplained residual. What is important, then, is not *whether* y-exogeneity is rejected, but by how big a likelihood ratio it is rejected.

plish.[29] Finally, it is probably in fact important to take account of private costs of acquiring and processing information, instead of, as in this model, treating "information sets" as given. It might be that the policy authority can relieve the private sector of some such costs by correctly processing information in setting its policy.

For all these reasons I do not regard this type of model as a null hypothesis with nonzero prior probability. This type of model is bound to be more or less false, probably in important ways. Nonetheless, it is for the time being the only class of models which generates a behavioral theory of the *stochastic* behavior of economic time series. In interpreting the statistical models we fit in this paper, hypotheses suggested by behavioral models in this class are therefore given special attention.

In neither Germany nor the U.S. is the test for block exogeneity of the real sector passed. The $\chi^2(32)$ statistic for this hypothesis in the German system is 52.10, with a marginal significance level of about 0.01 and for the U.S. data (where the import equation is ignored) we get $\chi^2(24) = 64.63$, with a marginal significance level less than 0.001. This conclusion is of course unsurprising when the strong lagged effects on real variables of price and money innovations in Germany and the U.S., respectively, are taken into account.

On the other hand, the hypothesis that the time form of the system's response to a money innovation (or to a real innovation) should be invariant to the money supply rule, does have a crude plausibility in the light of this system's results. The reaction of money to other variables in the system is very different in the two countries, as we have already pointed out, yet in both countries we get in response to a money innovation a rise in real GNP above trend, a corresponding fall in unemployment, and a rise in the real wage above trend, all lasting $2\frac{1}{2}$–3 years.

It is true that the response in the U.S. is substantially greater in percentage terms in real GNP and smaller in percentage terms in the real wage, and also that the drop in real GNP following the rise is relatively larger (compared to the initial rise) in Germany.

The only instance where the shape of the real variables' response is qualitatively different between countries is the response to an unemployment innovation, and here one has the possible explanation that, due to differences in the nature of the unemployment statistics between the two countries, innovations in unemployment are different things in the two countries. My own best guess, though, is that such measurement error does not account for the differing responses. The differences appear to be naturally explained by the differences in the reaction of

[29] By this I mean that the apparently innocuous assumption that the monetary authority must "set" money supply on the basis of information it has in hand is not realistic. Surely the monetary authority in the U.S. has the option of "leaning against the wind" in the presence of variations in the short interest rate produced by shifts in the demand for money. Such a policy would create correlation between innovations in the money supply and innovations in $u(t)$ without requiring that the authority be able to observe demand shifts in advance, in the sense of getting published data ahead of anyone else. A similar policy would even be possible relative to variations in unemployment: unemployment insurance claims could be paid in part with new currency, thereby creating an automatic link between money and unemployment innovations.

money to the innovation, which contradicts the classical rational expectations hypothesis,[30] and unemployment is connected to real GNP in roughly the same way in all the response patterns for both countries, which casts doubt on the measurement error explanation. Unfortunately, to test the hypothesis with data from these two countries we would need to believe the dubious assumption that differences in monetary policy rule are the only difference, rather than one obvious difference, between these two countries. A study across more countries might be able to reach firmer conclusions.

To estimate "wage and price equations" by single-equation methods and give them a structural interpretation, one needs to believe that the right-hand-side variables in such equations are exogenous. Given the strong feedback from prices and money into real variables in the systems we are discussing, it should be apparent that the usual form of such systems, in which unemployment and deviations of output from trend (sometimes called "capacity utilization") are the main right-hand-side variables other than lagged values of prices and wages, are not likely to pass an exogeneity test. Indeed the hypothesis that unemployment and real GNP are jointly exogeneous is rejected with a $\chi^2(32) = 58.26$ for the U.S. In Germany this hypothesis was inadvertently not directly tested, but an implication of that hypothesis, that money has a zero sum of coefficients in the unemployment equation, is rejected at a marginal significance level of less than 0.01.

Though the usual interpretation of wage and price equations as reflecting wage bargaining and price markup behavior is difficult to sustain if money supply is admitted to these equations, empirical research on these equations including money as an explanatory variable has gone forward recently.[31] The null hypothesis that real GNP, unemployment, and money together form an exogenous block is rejected for Germany with a $\chi^2(36) = 68.27$ and a marginal significance level of less than 0.01. For the U.S., this hypothesis turns out to be acceptable, with $\chi^2(36) = 42.54$ and a marginal significance level of 0.21. This hypothesis amounts to the assertion that for analyzing developments in the real aggregate variables we need not pay attention to relative price movements. Money supply by itself, with the real variables, provides an adequate measure of nominal-real interactions. The better fit of this hypothesis to U.S. experience might reflect relatively smaller importance for supply shocks in the U.S.

E. *Conclusions*

The foregoing small-scale example should have made clear that one can obtain macroeconomic models with useful descriptive characteristics, within which tests

[30] That is, the response of real variables to a money innovation in the U.S. appears to be naturally explained as a systematic tendency of money to increase after a positive unemployment innovation, followed by a private-sector reaction to the money increase which parallels the private-sector reaction to a monetary "surprise." In classical rational expectations models of the sort discussed above, the private sector should not react to predictable movements in the money supply.

[31] See Wachter [**32**] and Gordon [**8**].

of economically meaningful hyotheses can be executed, without as much of a burden of maintained hypotheses as is usually imposed in such modeling. A long road remains, however, between what has been displayed here and models in this style that compete seriously with existing large-scale models on their home ground—forecasting and policy projection. Even with a small system like those here, forecasting, especially over relatively long horizons, would probably benefit substantially from use of Bayesian methods or other mean-square-error shrinking devices to improve on what is obtained with raw estimates of 144 unconstrained coefficients. To be of much use in policy projection, models like these would have to include considerably more than the one policy variable which appears in these two models. In expanding the list of variables in the model, practical methods for limiting the growth in number of parameters as sample size increases will have to be developed, perhaps along the lines of index models.

But though the road is long, the opportunity it offers to drop the discouraging baggage of standard, but incredible, assumptions macroeconometricians have been used to carrying may make the road attractive.

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Manuscript received March, 1979.

APPENDIX I

THE DATA

Money: In the U.S., this is M1, seasonally adjusted, as prepared by the Board of Governors of the Federal Reserve System and published in *Business Statistics* and the *Survey of Current Business* by the Department of Commerce. In West Germany, this is defined as Money = Reserve Money in Federal Bank + Demand Deposits in Deposit Money Banks − Currency in Deposit Money Banks − Bankers' Deposits, and is taken from the International Monetary Fund Publication *International Financial Statistics*.

Real GNP: In the U.S., this is the series published in the same sources listed above for M1 and prepared by the Department of Commerce. It is seasonally adjusted. In West Germany, this is based on a series prepared by the Statistisches Bundesampt/Wiesbaden and published in *Wirtschaft und Statistik*. Besides involving splicing of series based on different index weights, preparation of this series required interpolation to obtain quarterly from published semi-annual data over much of the sample period. The interpolation was carried out by a regression of observed semi-annual data on monthly values of industrial production for the current and three preceding months. Industrial production was the Index der Industriellen Nettoproduktion, from the same source cited above for real GNP. The quarterly data have the form of quarterly estimates of two-quarter moving averages of real GNP.

Unemployment rate: In the U.S., this is the rate for all civilian workers, seasonally adjusted, prepared by the Bureau of Labor Statistics and published in the sources already cited for the U.S. For West Germany, this is a ratio of published numbers of unemployed, the series "Arbeitlose" in the source cited above, divided by the sum of the number unemployed and the number employed. The series for number employed was spliced together from data in *Statistischer Wochendienst*, published by the same organization cited above. For 1964–76 it was Erwerbstatigkeit (abhangige) (i.e., number employed excluding self-employed and family workers) and for 1952–62 it was Beschaftige Arbeitnehmer (i.e. a similar concept but double-counting some multiple job holders). For the intermediate years, and for splicing the series, the series "Ewerbstatigkeit," which includes self-employed and family workers, was used.

Wages: For the U.S., this is a seasonally adjusted index of average hourly compensation of all private nonfarm employees, prepared by the Bureau of Labor Statistics and published in *Business*

Conditions Digest by the Department of Commerce. For West Germany, this is Hourly Earnings in Industry as published in *International Financial Statistics* by the International Monetary Fund, using the Monthly Report of the Deutsche Bundesbank as source. Splicing of segments using different norming years was required.

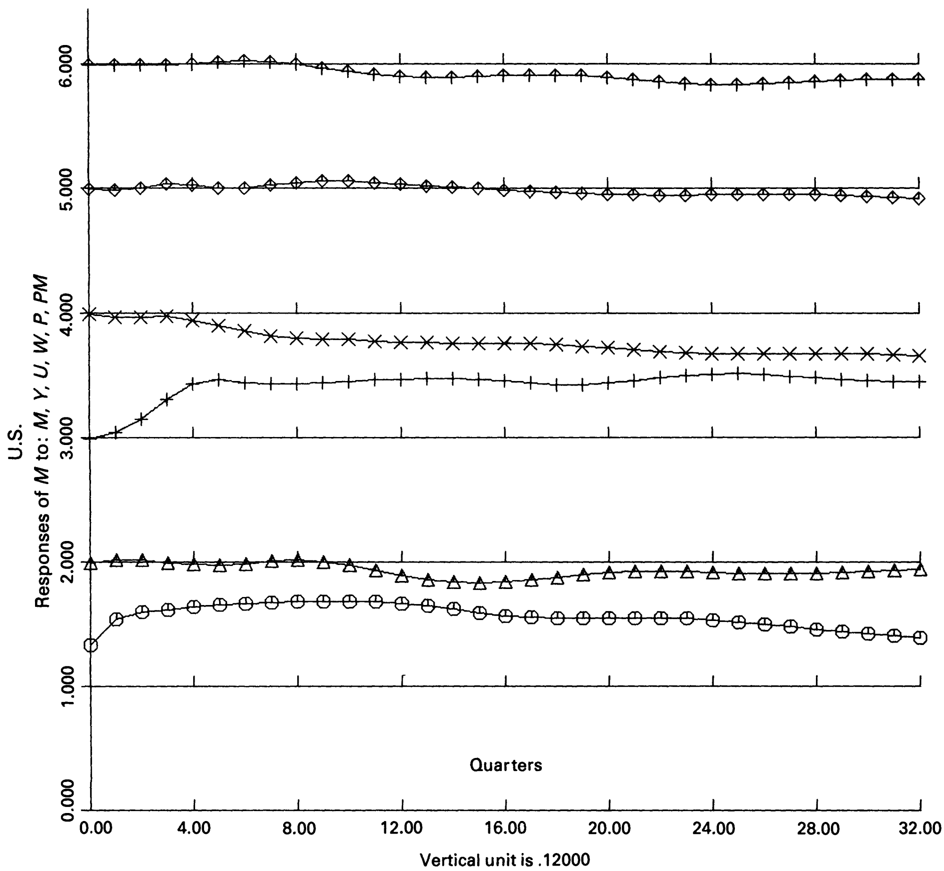
Prices: For the U.S. this is a seasonally adjusted price deflator of Gross National Product of the non-farm business sector, as prepared by the Department of Commerce and published in the *Survey of Current Business*. For West Germany, these data were constructed as a GNP deflator by taking the ratio of current dollar GNP to constant dollar GNP as published in the same source cited for German constant-dollar GNP. As with real GNP, interpolation was required, in this case using monthly data on retail prices (Index der Einzelhandelsprese, Einzelhandel insgesamt, from Wertshaft und Statistik) in the same way as data on industrial prouction were used for interpolating real GNP. A notable difference between the two procedures was that for prices, residuals from the fit of the GNP deflator to retail trade prices showed substantial serial correlation and were therefore used in interpolation. At an early stage of the work this interpolation was two-sided—interpolated values were predicted values from the regression on retail prices plus an average of residuals from the regression one quarter ahead and one quarter behind. Later, it was decided that this might distort the timing of series, so the interpolation was redone using only lagged residuals. This had no important effect on the estimated equations, and hence not all of the restricted regressions used in forming test statistics were repeated with the data interpolated in the latter way. The plots and tables of moving average representations do, however, reflect the latter "one-sided" interpolation method.

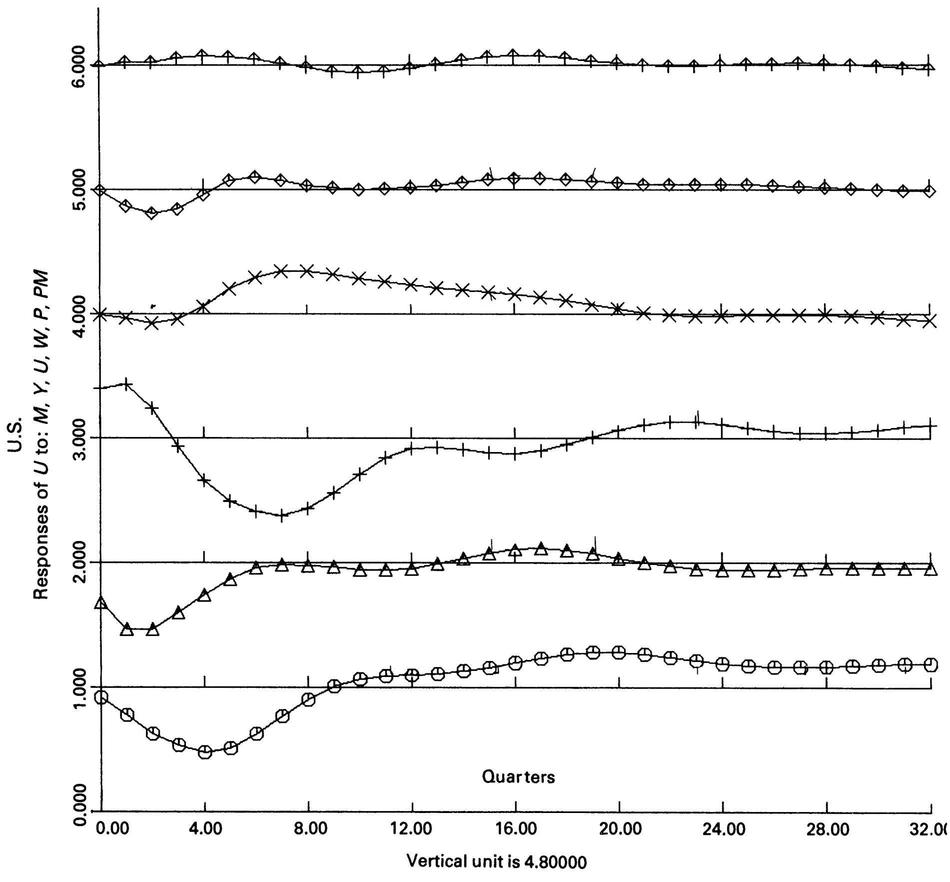
Import Prices: For the U.S., this is the Unit Value of General Imports as published by the Department of Commerce in the Survey of Current Business. For West Germany, this is the series Unit Value of Imports published by the International Monetary Fund in *International Financial Statistics*. Splicing of six overlapping segments reflecting small changes in the definition of the series or changes in base year was required.

APPENDIX II

CHARTS

Notes to the Charts: Each chart displays the response of one variable in one country's model to six initial conditions. The model is in each case a vector autoregression, in which each of the six variables in the system is predicted as a linear combination of past values of all six variables in the system. The variables are ordered as M, Y, U, W, P, PM. The jth simulation sets the value at time 0 of the jth variable in this ordering at the estimated residual standard error of a regression of the jth equation autoregression residual on the autoregression residuals from lower-ordered equations. The initial value of variables ordered lower than j is set to zero, as are all values of all variables for negative t. The $t=0$ values of variables higher than j in the ordering are set equal to the predicted values for those residuals, given the value of the first j residuals, from a regression of the last 6-j residuals on the first j. More formally, if Σ is the estimated variance-covariance matrix of the residuals in the autoregression, the jth simulation sets the time-zero values of the variables to the jth column of the positive, lower-triangular square root of Σ. Finally, an intuitive description is that the jth simulation pertains to a movement in that part of the innovation of the jth variable which is uncorrelated with innovations in the first j-1 variables, with correlations between this part of the jth innovations and $j+s$th innovations being attributed (for positive s) to causal influence of the jth innovation on the $j+s$th. The six numbered horizontal axes on each chart refer to the six simulations, in the order displayed along the left margin of each chart.


U.S.
Responses of M to: M, Y, U, W, P, PM
6.000
5.000
4.000
3.000
2.000
1.000
0.000
Quarters
0.00
4.00
8.00
12.00
16.00
20.00
24.00
28.00
32.00
Vertical unit is .12000


U.S.
Responses of U to: M, Y, U, W, P, PM
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4.000
3.000
2.000
1.000
0.000
Quarters
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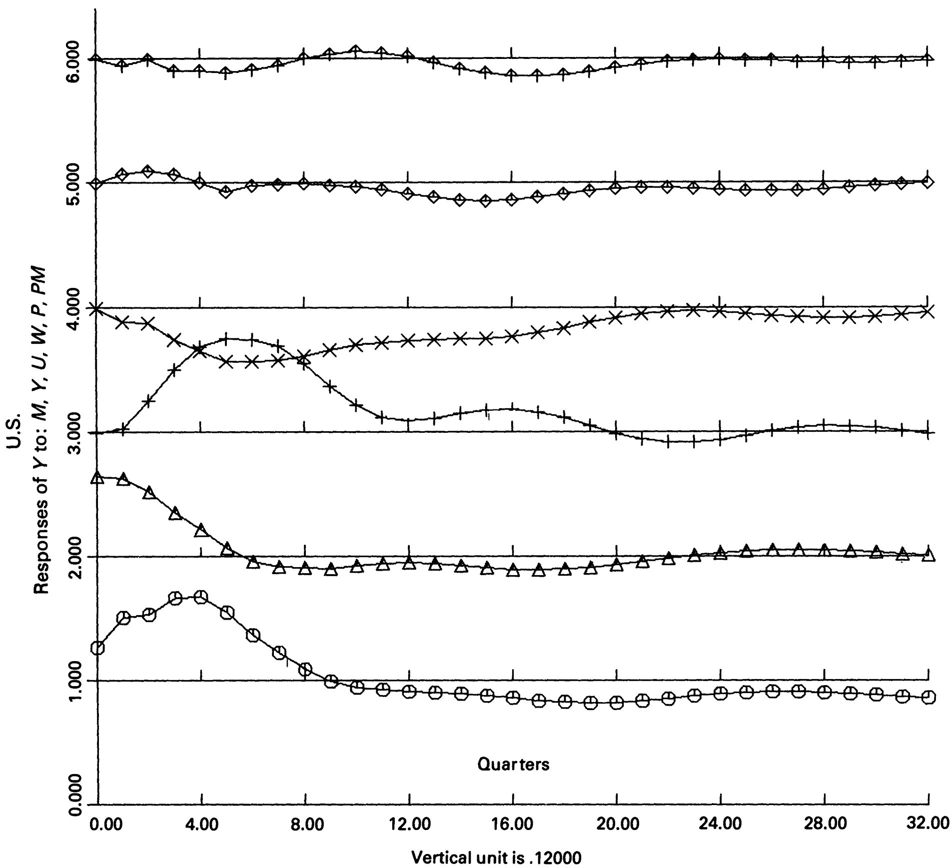
4.00
8.00
12.00
16.00
20.00
24.00
28.00
32.00
Vertical unit is 4.80000


U.S.
Responses of Y to: M, Y, U, W, P, PM
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4.000
3.000
2.000
1.000
0.000
Quarters
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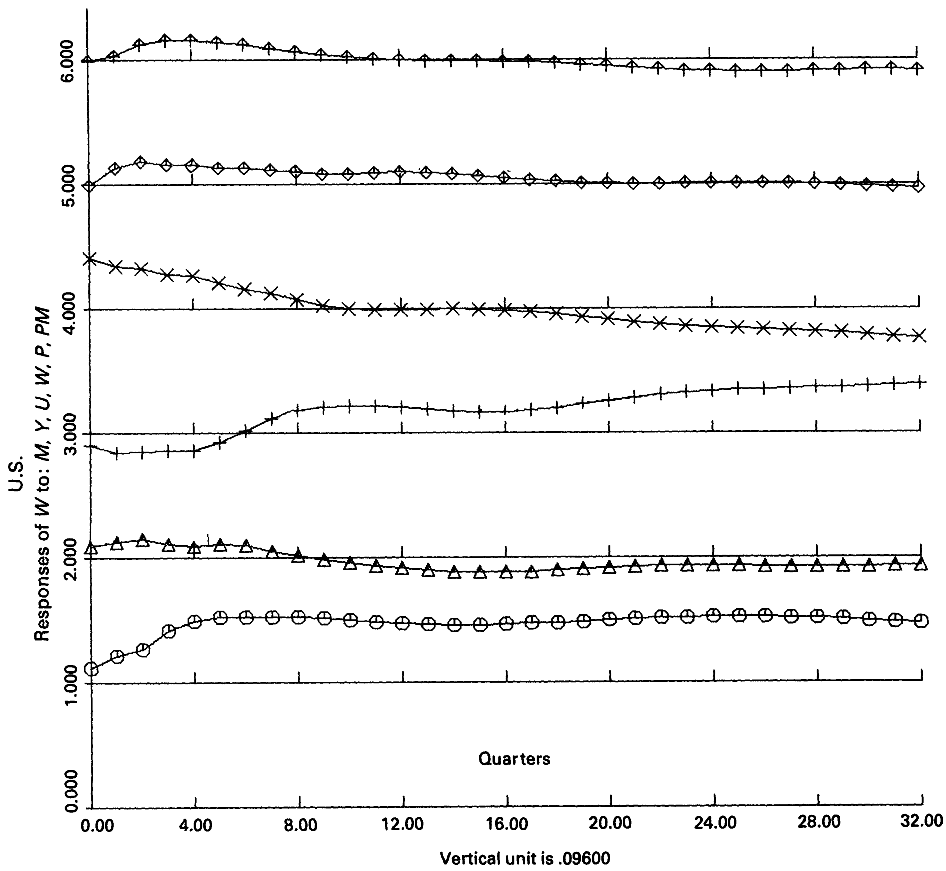
4.00
8.00
12.00
16.00
20.00
24.00
28.00
32.00
Vertical unit is .12000


U.S.
Responses of W to: M, Y, U, W, P, PM
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4.000
3.000
2.000
1.000
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Quarters
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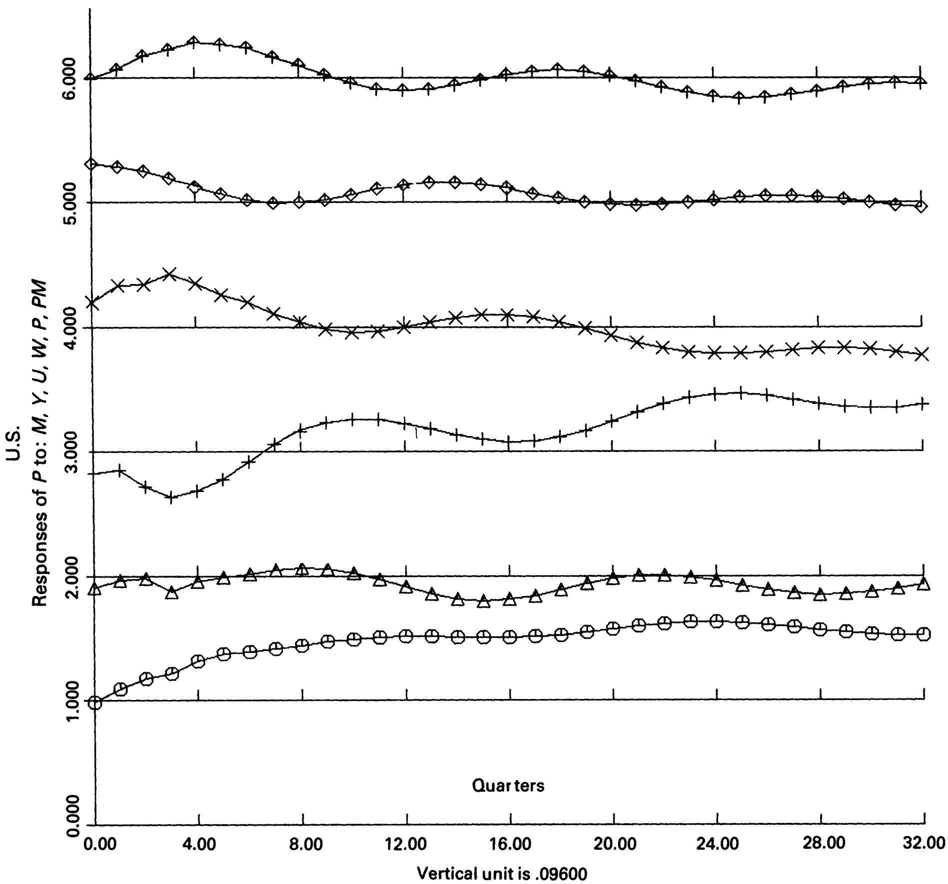
Vertical unit is .09600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Responses of P to: M, Y, U, W, P, PM
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2.000
1.000
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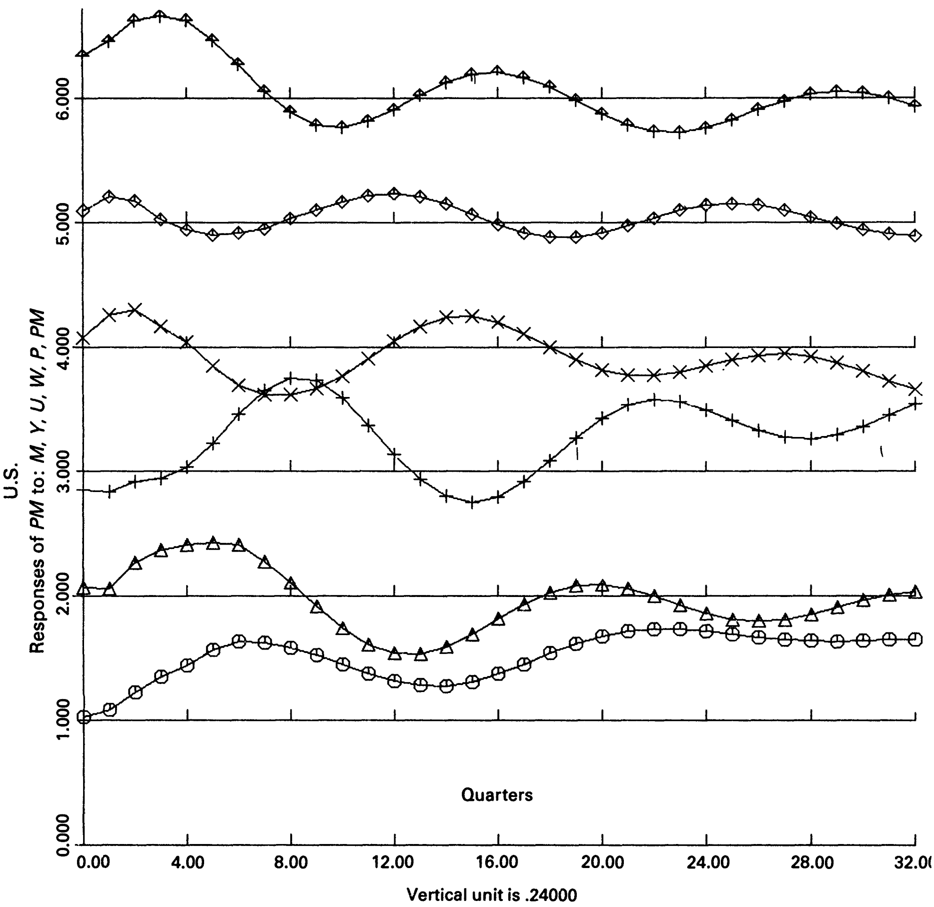
16.00
20.00
24.00
28.00
32.00
Vertical unit is .09600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Responses of PM to: M, Y, U, W, P, PM
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4.000
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2.000
1.000
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Quarters
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8.00
12.00
16.00
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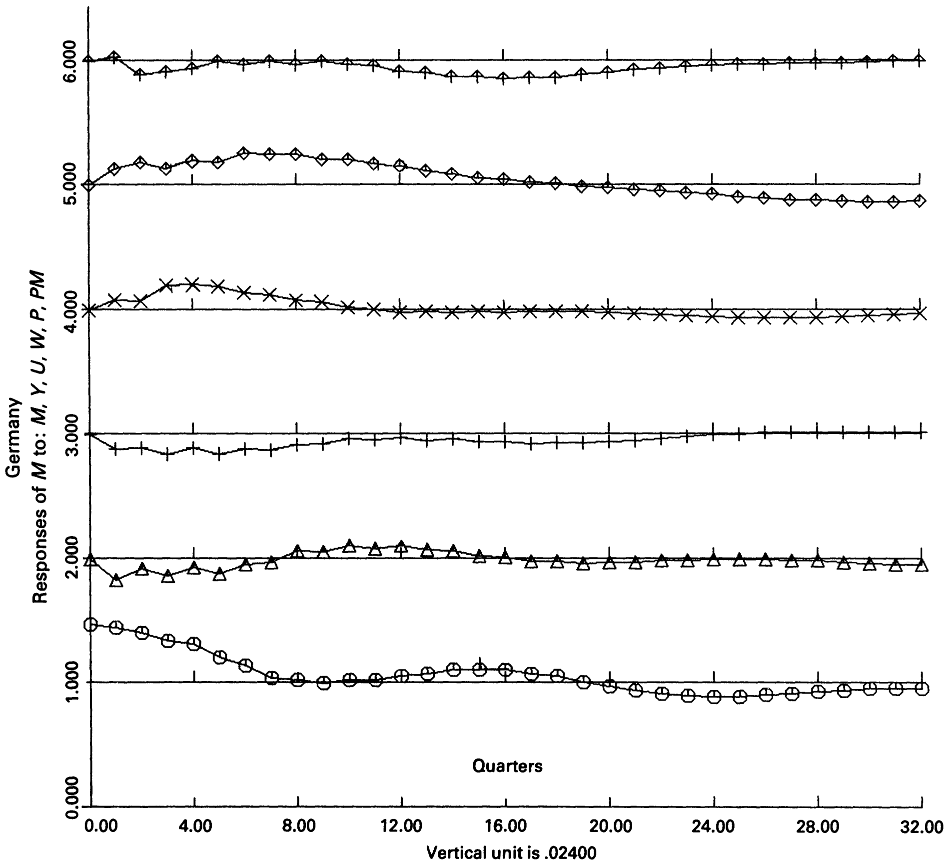
24.00
28.00
32.00
Vertical unit is .24000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Responses of M to: M, Y, U, W, P, PM
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5.000
4.000
3.000
2.000
1.000
0.000
Quarters
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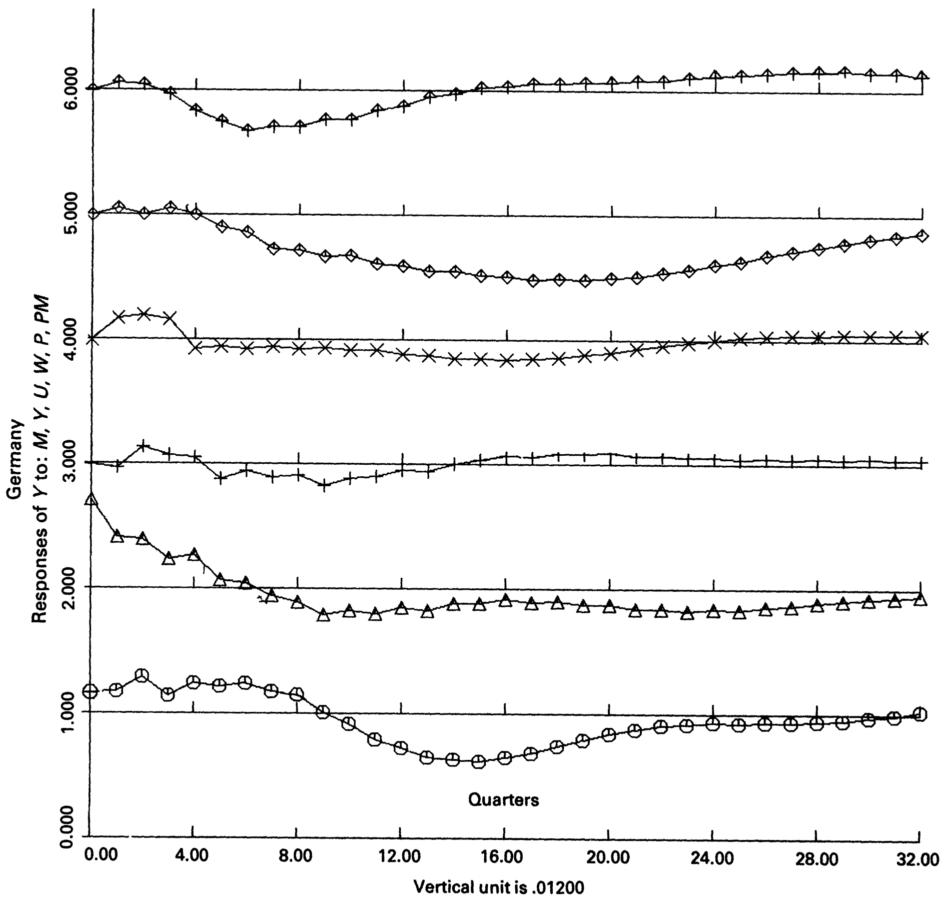
4.00
8.00
12.00
16.00
20.00
24.00
28.00
32.00
Vertical unit is .02400


Germany
Responses of Y to: M, Y, U, W, P, PM
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5.000
4.000
3.000
2.000
1.000
0.000
Quarters
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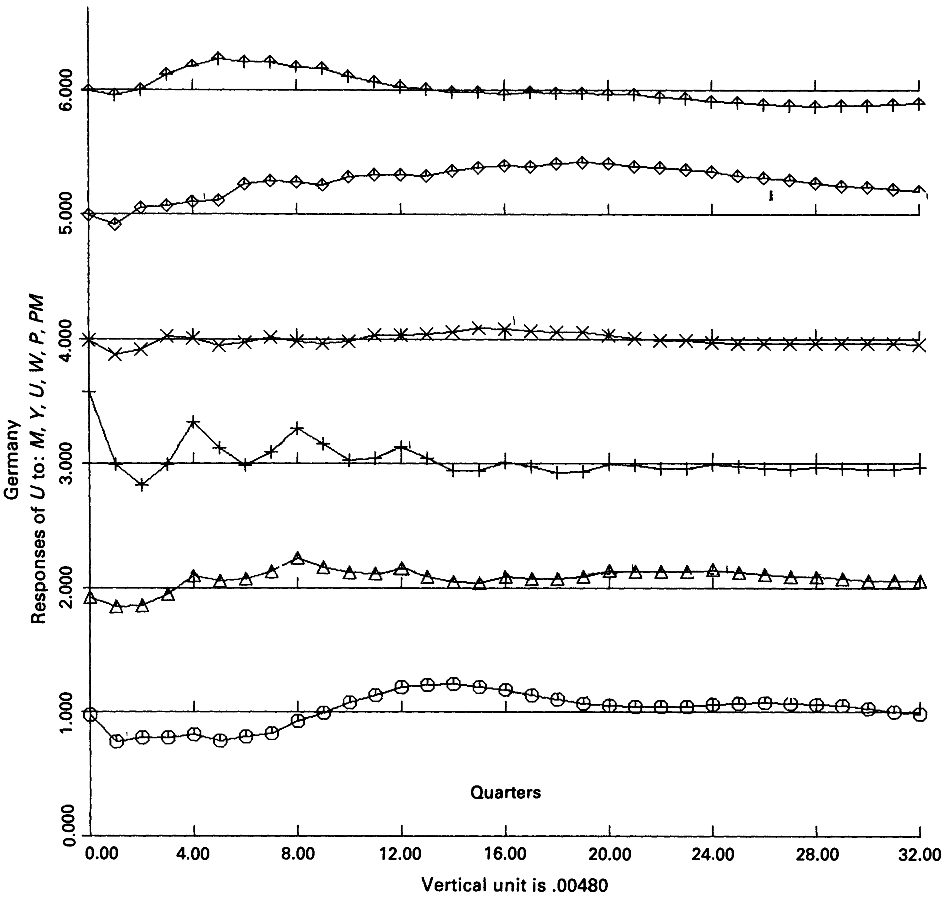
4.00
8.00
12.00
16.00
20.00
24.00
28.00
32.00
Vertical unit is .01200


Germany
Responses of U to: M, Y, U, W, P, PM
6.000
5.000
4.000
3.000
2.000
1.000
0.000
Quarters
0.00
4.00
8.00
12.00
16.00
20.00
24.00
28.00
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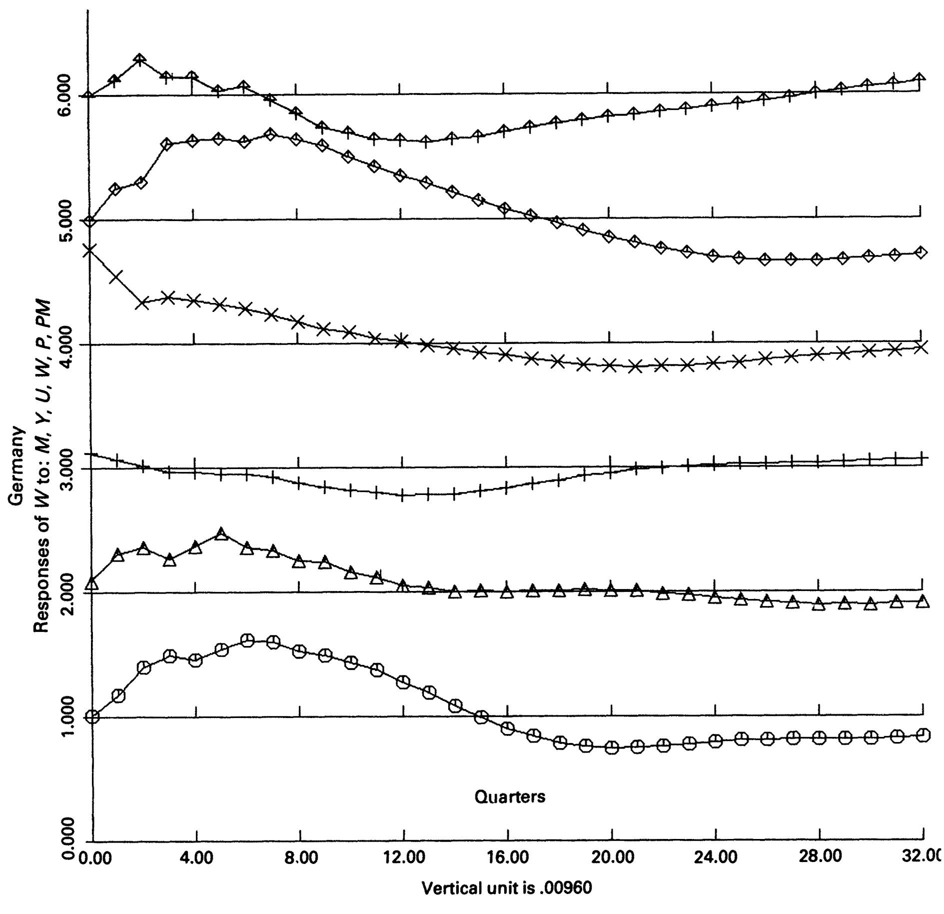
Vertical unit is .00480


Germany
Responses of W to: M, Y, U, W, P, PM
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3.000
2.000
1.000
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Quarters
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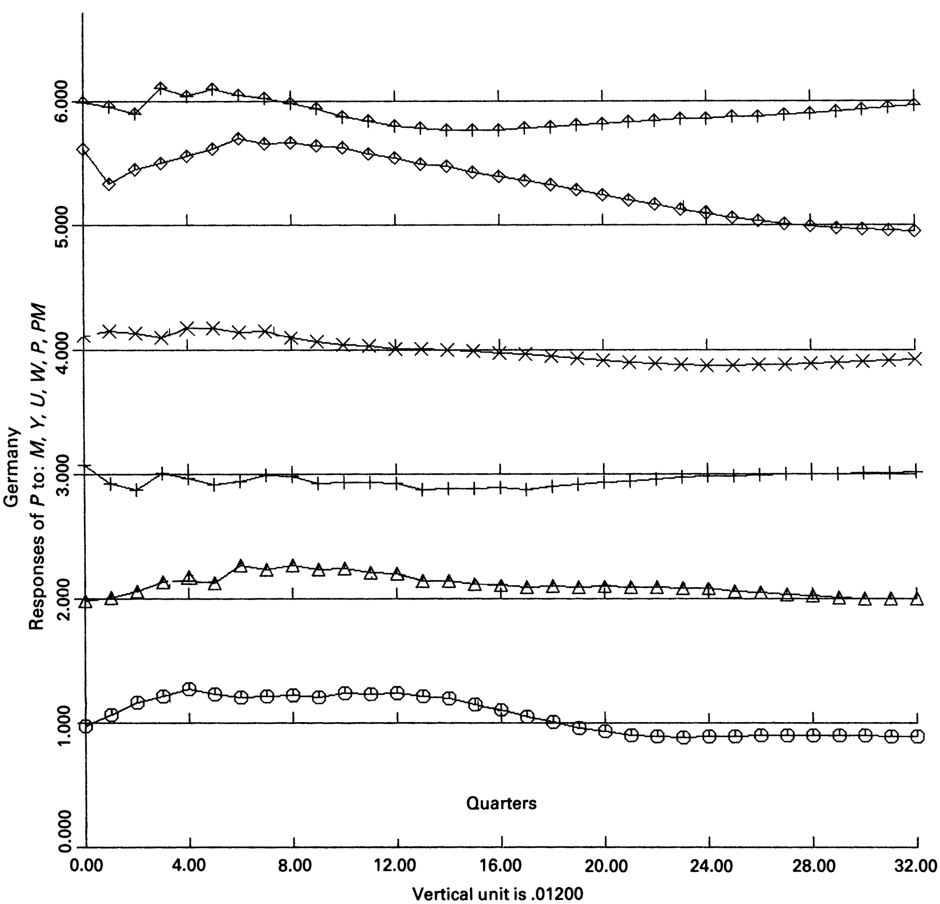
4.00
8.00
12.00
16.00
20.00
24.00
28.00
32.00
Vertical unit is .00960


Germany
Responses of P to: M, Y, U, W, P, PM
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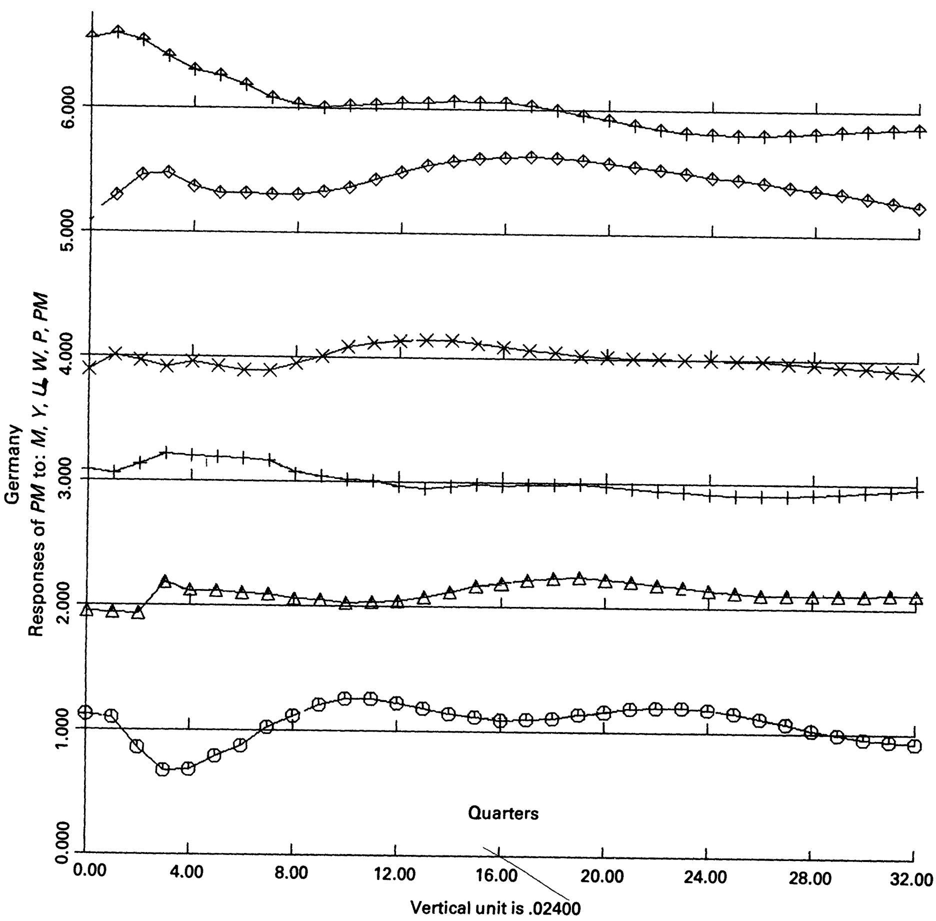
5.000
4.000
3.000
2.000
1.000
0.000
Quarters
0.00
4.00
8.00
12.00
16.00
20.00
24.00
28.00
32.00
Vertical unit is .01200


Germany
Responses of PM to: M, Y, U, W, P, PM
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5.000
4.000
3.000
2.000
1.000
0.000
Quarters
0.00
4.00
8.00
12.00
16.00
20.00
24.00
28.00
32.00
Vertical unit is .02400

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[17] **Can the Inflation of the 1970s be Explained?**
Robert J. Gordon; George Perry; Franco Modigliani; Arthur Okun; Michael Wachter; Pentti Kouri; Edmund Phelps; Christopher Sims
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