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Are Forecasting Models Usable for Policy Analysis?*

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In one of the early papers describing the applications of vector autoregression (VAR) models to economics, Thomas Sargent (1979) emphasized that while such models were useful for forecasting, they could not be used for policy analysis. Recently this point has been vigorously reasserted, by Sargent himself (1984) and by Edward Leamer (1985) among others.

The point has required vigorous reassertion because VAR models are used widely, and few who use them stay pure—in the sense of never thinking about their implications for policy—for very long. There are several reasonable ways to generate conditional forecasts from VAR models. Once one has seen how easy it is to obtain a forecast conditional on a certain configuration of policy variables, it is tempting to make such forecasts and difficult to be sure that they do not influence one's opinions on policy choices. Robert Litterman, a leader in demonstrating the value of VAR models in forecasting, has also developed methods to use them to make forecasts conditional on policy choices and even to use them in computing optimal policy rules. (See, for example, Litterman 1982, 1984.)

A similar paradox concerns the position of the large commercial econometric forecasting models. I was present at a recent round table discussion in which a distinguished theoretical economist offhandedly asserted that, since these commercial models are intended for forecasting, no one takes them seriously as economics. The rational expectations critique of the use of such models in policy analysis is now more than ten years old and, in a good part of the economics profession, has the position of the established orthodoxy. Yet when actual policy choices are being made at all levels of the public and private sectors, forecasts from these large models, both conditional and unconditional, remain pervasively influential. The rational expectations school began with a program for providing an alternative to such models for quantitative policy analysis, but the program has had little practical impact.

Why is this? Are the people who actually make policy unable to understand the futility of using VAR models or conventional econometric models as an aid to policy choice? Or are the abstract arguments against using such models this way missing something important?

Why Making Policy With a Forecasting Model Is Supposed to Be a Mistake

There are two related versions of the argument against using forecasting models for policy analysis. One is that such models are nothing more than summary descriptions of the historical data, usually based on sample correlations. While such a description can be extrapolated into a useful forecast, supposing that it can be the basis for projecting the effects of policy choice amounts to taking correlation to indicate causation, which we all understand to be fallacious.

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The argument in this form applies directly to VAR models, which are forthrightly descriptive statistical models that do nothing more than summarize correlations in a convenient way. The argument also applies indirectly to the use of large commercial models, since many economists (myself included) regard the economic interpretations those models give their own equations as strained and fragile. Despite the fragility of those interpretations, the models are clearly useful in forecasting. Economists who accept this form of the argument against forecasting models as policy tools may be ready to reject the use of commercial models for policy analysis despite admitting their value for forecasting.

Sargent (1984) puts forward a second version of the argument against using forecasting models for policy analysis. He observes that VAR models usually incorporate policy variables into the model symmetrically with other variables, treating them all as random variables. He agrees that, in principle, policy choices are random variables, with uncertainty about them an important influence on actual behavior. However, when we choose a policy, we do not usually roll the dice; instead, we ordinarily have to make a unique choice which to us has a deterministic character. We cannot reasonably think about such a choice within the confines of a model in which policy is determined by some random mechanism.

When properly interpreted, both versions of the argument are correct, even tautological. It is impossible to use a statistical model to analyze policy without going behind the correlations to make an economic interpretation of them. Generating such an interpretation is what econometricians call *identification* of a model. And one cannot analyze the choice of policy variables without cutting through the seamless web of a model in which all policy variables are determined inside the model. (In fact, these arguments even apply to the use of a model for forecasting.)

But these correct arguments do not constitute an objection to the use of forecasting models to guide policy choice. They only point out that when we find a way to use such a model to guide policy choice we are, implicitly or explicitly, supplementing it with an identifying interpretation. To argue against using such models to guide policy choice, we have to compare their identifying interpretations to alternatives. The real choice is between methods of policy analysis based on spare identifications, in which simple, weak identifying assumptions just sufficient to bring a forecasting model to bear on a policy issue are imposed, versus alternatives based on elaborate, strong identifying assumptions which lead to models with poor forecasting properties.

Reduced Form, Structure, and Identification

These three terms—reduced form, structure, and identification—are used in different ways by economists and are used outside of economics in still other ways. In one common pattern of usage, a *reduced form* is a model that describes how some historical data, which we can call X, was generated by some random mechanism. When we estimate a reduced form model, we construct some statistics that summarize the full data set X. The reduced form model can be thought of as a rationale for a particular kind of data summary.

A *structure*, or structural model, is a model we can use in decisionmaking. It generates predictions of results Z of various kinds of actions A we might take. The notions of structure and reduced form are implicit in most kinds of use of data to aid decisionmaking, but when an econometrician uses these notions he generally explicitly displays a reduced form as probability distribution $p(X; \beta)$ for the data as a function of reduced form parameters β and displays a structure as a conditional distribution $q(Z \mid A; \alpha)$ of results given actions, depending on unknown structural parameters α. Identifying a model is then asserting a connection between the reduced form and the structure, so that estimates of the reduced form parameters β can be used to determine the structural parameters α.[1]

Reduced to simplest terms, *identification* is the interpretation of historically observed variation in data in a way that allows the variation to be used to predict the consequences of an action not yet undertaken. Making this connection between data and consequences of decisions is never trivial, and it always is more difficult the more remote is the action we contemplate taking from any historically observable event. If we are lucky, there may be much historical random variation in actions that are very similar to the action we now contemplate. Then we may be able to use the data rather directly to decide the likely effect of our actions. But sometimes we must contemplate actions very different from those observed historically, in which case identification becomes more difficult and controversial.

In economics, structural models are often formulated so that every parameter in the vector α has an economic interpretation. That is, the elements of α will be things like

[1]In engineering control theory, *identification* is used to refer to what econometricians would call *estimation* of a certain class of parameters. In statistics, *identification* is the question of whether or not a model's parameters map one-to-one into probability distributions for the data. Both usages are related to the econometric usage described in the text, but there is not much point in explaining the connections here.

"interest-elasticity of demand for money" or "elasticity of substitution between electric power and labor in milk production." In contrast, the elements of the β vector are usually harder to interpret because they reflect combined influences of behavior in many sectors of the economy. There is no unique standard, however, for when a parameter has an economic interpretation. The parameters in a model may have interpretations, yet the model may not be structural, in the sense that we may be unable to use it to predict the consequences of the actions which interest us. Conversely, we may be able to use a model to make such predictions accurately even though some or all of its parameters do not have neat interpretations.

The formal mathematics of asserting a connection between a reduced form model and a nonstructural model that has a satisfying interpretation is no different from the formal mathematics of asserting a connection between the reduced form and a structure. Many economists have come to think of structural models as models with satisfying interpretations for all parameters. The unarguable assertion that predicting the effects of policy requires identification of a structural model thus becomes, via a semantic confusion, a source of serious misunderstanding.[2]

Examples of Identification in Decisionmaking

To bring this abstract discussion down to earth, I present a series of examples of the application of statistical models to decisionmaking.

□ *The Spreadsheet*

Perhaps the simplest and most widely used form of quantitative modeling as an aid to policy choice is spreadsheet financial modeling. Here accounting identities and a few simple relations among accounting categories characterizing average historical behavior are used as a framework for projecting the consequences of business decisions. In this type of modeling, there is no explicit probability model, though the use of average past behavior to extrapolate the future implies some beliefs about the probability structure of the data. The reduced form is implicit in the particular categories used to report and summarize the accounting data in terms of gross margins, returns on equity, productivity ratios, and so forth. The structure generally is formed by assuming that certain historically observed average ratios in the accounting data are likely to persist under various changes in business strategy and hence can be used to project the effects of variations in strategy. Everyone understands that such models are subject to many limitations; nonetheless, because the models impose some consistency on projections of long lists of interrelated numbers, they are helpful. The models' identifying assumptions are seldom the focus of much explicit attention, though users understand that the assumptions are crude approximations and therefore judgmentally discount the model results accordingly.

□ *Clinical Trials in Medicine*

Another example, in some ways even simpler, arises in clinical trials in medicine. We might have results from a study of 200 patients, 100 given the old treatment and 100 given the new treatment. The results might be reported as cross-classified into 16 categories by age, race, and sex of the patient. The complete cross-tabulation can be regarded as a set of reduced form parameter estimates. Say that 80 of those given the new treatment were cured, while only 30 of those given the old treatment were cured. A doctor using this study might take it to mean that the new treatment is better and proceed to prefer it over the old for all his patients. He would be taking the overall proportion of the patients in the study who benefited from each treatment to be a good indicator of the probability that a patient of his will benefit from the treatment. His structural model would implicitly discount the variation across age-race-sex categories as unimportant random variation.

But suppose the doctor's first patient for whom the study is relevant is a 24-year-old black male. The doctor happens to recall that there were only 4 black males between 15 and 25 years old in the study and, further, that those given the old treatment survived and those given the new treatment died. Given this information, the doctor probably will still choose the new treatment, but the choice is not automatic and will depend on details of the data and on the doctor's beliefs about how the treatments work—that is, whether there is any reason to suppose the treatments would work differently in general on young black men.

This kind of question in fact arises with any patient: Is this particular patient "like" the patients in the sample so

[2] The sense in which *structural* is used here, though somewhat in conflict with the common usage pointed out in the text, is well founded in earlier treatments of these issues—for example, those by Hurwicz (1962) and Koopmans and Bausch (1959). Economists should standardize usage so that *structural* is always connected to a claim of usefulness in the prediction of the effects of actions. In contrast, *reduced form* is more widely used to refer to a transformation of some initial interpretable model into a form that is in some way more convenient but less easy to interpret. One can imagine a model useful for predicting the effects of policy that is a reduced form transformation of a model with a more elaborate set of behavioral interpretations. Such a model could be both structural and, in a sense, a reduced form. I see no harm in using *reduced form* in this way, though in this paper I will stick to the more stringent usage in which a reduced form model is taken never to be structural.

that results from the sample are relevant? Can the results be extrapolated to age or ethnic groups not represented in the sample at all? Should a veterinarian give any weight to this study of humans in deciding how to treat horses? This is the identification problem.

□ *Forecasting*

In economics, the use of a forecast implies that some decision is being made which may be influenced by beliefs about the future path of the economy. The reduced form model describes the data at each historical date t as drawn from a probability distribution conditioned on data up through $t-1$, usually with the form of that distribution the same for all dates t. In a pure forecasting situation, the structure asserts that the distribution of next period's data is unaffected by any action we may take, but that it is related to data up through the current period in the same way that data at t has been related to data up through $t-1$ at all historical dates t. But there is always a question as to whether or not this particular date is different from what we have seen historically: Will this quarter be the one in which our forecasting model finally breaks down? And there is also always a question as to whether or not the decision we are making does in fact have no impact on next period's data. If the decision is whether or not to take out a home mortgage, then there is probably no impact; but if it is whether or not to lay off 10,000 workers, the answer may not be so clear.

□ *Macroeconomic Policy*

When macroeconomic models are used to project the effects of policy, the most common procedure begins with a model in which policy variables appear explicitly and in which those variables are taken as predetermined. That is, the model is in a form where nonpolicy variables at a given date are determined by taking policy variables at that date as given. Policy options are then defined as time paths of the policy variables, and the equations of the model are used to solve for paths of other variables corresponding to the various policy options.

While the algebra of this procedure is straightforward, the interpretation of it as an identification of a reduced form is not. Suppose the model is linear and complete in that it can be solved for all variables it contains except the policy variables.[3] Letting the policy variables be X and the other variables be Y, a system of equations for all the variables has the form

$$(1) \qquad Y(t)A = X(t)B + Z(t-1)C + e(t)$$

$$(2) \qquad X(t) = Z(t-1)D + u(t)$$

where $Z(t-1)$ is the vector of all lagged values of X and Y that enter the system, where the first block of equations is the model, and where the second block of equations are prediction equations for the policy variables. We find paths for future Y corresponding to paths for X by fixing the X paths and solving for Y using the first block of equations. When we do this, we are setting the disturbances $e(t)$ in the first block of equations to zero while adjusting the residuals $u(t)$ in the second block to achieve the hypothetical paths for X which represent policy choices. Variation in policy is being represented entirely through variation in $u(t)$, with no variation in $e(t)$.

The equations of the model have been fitted to historical data. For it to be reasonable to project the effects of policy by setting e to zero and varying u, it must first be true that historically the $u(t)$'s and $e(t)$'s have been unrelated, and second be reasonable to suppose that prediction errors for policy variables can be identified with policy choices. It is not necessary that the only source of prediction error in policy variables be policy changes, but all sources of prediction error for policy variables must have the same sort of effect on the economy as policy changes. For example, in a period when the monetary authority was maintaining stable interest rates, money stock variations would—at least in the short run—directly reflect shifts in money demand. Treating money stock as a policy variable according to the conventional methodology of policy projection would then lead to serious mistakes, since it would imply a mistaken identification of the historical effects of money demand shifts with the effects of policy-generated shifts in money stock.

□ *An Excise Tax*

A statistical model need not contain policy variables in order to be useful in projecting the effects of policy. The simplest textbook example of econometric identification is a model for supply and demand, say

Demand

$$(3) \qquad p(t) = \delta + \alpha q(t) + \beta y(t) + u(t)$$

Supply

$$(4) \qquad p(t) = \phi + \gamma q(t) + \pi w(t) + v(t)$$

[3] If it is not complete, it can be completed by adding forecasting equations for any nonpolicy predetermined variables. In order to make projections with the model, this has to be done implicitly in any case, since all variables in the model will have to be forecast.

where p is price, q is quantity, y is income, and w is weather. Though this model does not contain an excise tax variable, it can be used to predict the effects of an excise tax. Let the excise tax rate at t be $f(t)$. Then if there was no tax in the past, we can still predict the effects of a tax paid by producers by introducing f into the supply equation (4), making it

$$(5) \quad [1 - f(t)]p(t) = \phi + \gamma q(t) + \pi w(t) + v(t).$$

Solving for p and q, we can obtain

$$(6) \quad p(t) = \{\gamma[\delta + \beta y(t) + u(t)] - \alpha[\phi + \pi w(t) + v(t)]\} \div \{\gamma - \alpha[1 - f(t)]\}$$

$$(7) \quad q(t) = \{[1 - f(t)][\delta + \beta y(t) + u(t)] + [\phi + \pi w(t) + v(t)]\} \div \{\gamma - \alpha[1 - f(t)]\}$$

which can be used to predict the effects of the tax rate $f(t)$ for given values of weather and income, which are treated as determined outside this market.

In this example the historical responses of prices and quantities to movements in weather and income are being used to predict their responses to a previously untried excise tax. This is an intellectually appealing trick, so much so that economists sometimes forget that it would really be better to have some historical data from a period in which there was an actual excise tax. In that case the system (6)–(7) could be estimated directly. When we have to derive (6)–(7) from (3)–(4), we rely on some strong assumptions. Among these is the assumption that the dynamics which have been historically adequate to model market price fluctuations will also suffice to model responses to a more persistent change in price due to taxation. Another is the assumption that industry supply behavior is perfectly competitive.[4]

□ *Rational Expectations Policy Analysis*

The ideal policy evaluation procedure of the rational expectations school—what Sargent (1984) has in mind as an alternative to methods closely tied to VAR models—begins with a dynamic probability model for the economy. We can label this whole model Y. There is also a probability law for policy variables, which we can label X. Though the policy variables have varied historically, the rational expectations school makes a sharp distinction between changes in policy variables and changes in the probability law governing policy variables. Only the latter are taken to be "real" changes in policy. In the ideal framework, X has not changed at all historically. We contemplate making a single, permanent change in X. Getting historical data in which X has not changed to tell us something about what will happen to Y when we do change X is not impossible. It is qualitatively similar to the problem of predicting the effect of a never-before-tried excise tax, as discussed previously, though the rational expectations identification problem is even more interesting and intellectually challenging.

As Hansen and Sargent (1980) outlined it, the rational expectations prescription is that parameters of "objective functions of agents" be estimated. These parameters, often described as characterizing "tastes and technology," are taken to be invariant to the government decision rule X and also to be sufficient to determine the reaction of the economy to permanent, one-time changes in the decision rule. Both in claiming to be able to estimate all the relevant aspects of tastes and technology and in claiming to be able to use them to predict reactions to changes in X, we will have to make assumptions that are an order of magnitude more elaborate and questionable than those required in the excise tax example.

Just as in the excise tax example, we are really better off if the change in probability law for policy variables that we contemplate implementing has precedents. In that case both Y and X need time arguments, and we have to consider how to construct a dynamic model connecting $X(t)$ to $Y(t)$. In this model, the values of policy variables will be determined in two steps—first $X(t)$ is determined, then the values of policy variables are generated by $X(t)$. $X(t)$ itself is not then the probability law generating policy variables but a piece or stage of that probability law. Nonetheless, if $X(t)$ is what we are in the process of choosing, what we need is the distribution of the future path of $Y(t)$ given our deliberate choice of a path for $X(t)$. The fact that $Y(t)$ and $X(t)$ now represent probability laws, not values of real variables, does not change the basic nature of the identification problem.

By considering standard policy evaluation procedures applied in a situation where policy changes actually shift the linear structure of the economy by changing expectation formation rules, the rational expectations critique

[4]If industry supply behavior is not competitive, equations (6)–(7) will not be a correct description of industry behavior under the tax, even though the historical behavior of price, quantity, income, and weather data may be indistinguishable from that generated by competitive supply behavior.

points out one important dimension in which such procedures can fail. If policy shifts at random intervals on the order of ten years long between, say, inflationary expansion of demand and contractionary policies, then expectation formation by the public will differ between the two types of regime. It is likely that the economy will be best modeled nonlinearly, possibly shifting systematically between two different limiting linear structures as evidence of a policy regime shift accumulates. The ideal model will include a complete and accurate description of how expectation formation shifts with policy changes. Such a model will probably show nonlinear responses to changes in the paths of policy variables. A model that ignores this nonlinearity will be inaccurate, though possibly accurate for short-term prediction within regimes. But a model may correctly take account of the nonlinearity without explicitly modeling expectation formation. Such a model can even give accurate guidance to policymakers about the effects of a shift in regimes. In the sense we are giving the term, the model is structural, not a reduced form, even though some of its parameters will confound influences of demand with supply or influences of expectation formation with adjustment costs.

Even a model that does not ignore nonlinearity would be accurate only so long as the probability law historically governing the process of regime shifting remains constant. But it is not true that there is no "real" change in policy without a change in this probability law. The fact that the public has in mind some probability distribution over possible actions by a policymaker does not mean he faces no real choice. In fact, the rational public will always have such a distribution over all the policy choices which are truly possible. There is nonetheless a well-defined notion of a conditional probability distribution of the economy's future path given policy actions. This conditional distribution is what the policymaker needs to make his choice, and its existence does not conflict with the public's having in mind at the same time a probability distribution over his possible choices. It is also not true that the existence of a probability law governing policy implies that there can be no scientific advance in the making of policy. A rational public may expect occasional improvements in policymaking, though the timing of the improvements is likely to remain surprising. If this were true, the dynamic structure of the economy would drift through time as policymaking improved. A model that ignored this source of nonlinearity would be inaccurate, but again a model can allow for this nonlinearity without introducing detailed interpretations of all its parameters.

The most prominent examples of situations in which economists are attracted strongly to the rational expectations approach to policy analysis are sudden sharp changes in policy institutions—shifts from fixed to flexible exchange rates (or vice versa) or monetary and fiscal reforms that end hyperinflations. One could in principle attempt to predict the effects of such policy shifts using data from periods in which the shocks to the economy are less extreme and of a completely different nature. One could use data from a period in which inflation showed slight variation, monetary policy was stable, and economic fluctuations arose mainly from the effects of weather on agricultural yields to estimate utility and production functions for the economy. If this estimation were successful, it could, under the usual auxiliary assumptions of rational expectations, be used to predict the effects of a shift from fixed to flexible exchange rates or of fiscal reform to end an inflation. Most economists would, however, have little confidence in such an exercise. Even those who believe it is useful to think of tastes and technology as determining the economy's reaction to policy—even they understand that econometric estimates of utility and production functions are more or less crude approximations. And they understand that the validity of those approximations must be expected to deteriorate as the model is extrapolated to situations further and further outside the range of variation observed in the data.

Sargent (1982) has done some empirical analysis of such policy changes, taking a rational expectations perspective but using a methodology that weakens this objection. He studies a number of historical examples of economic policy shifts that ended strong inflations and suggests that they carry lessons for counterinflationary policy generally. This approach avoids the need to project the effects of such large policy changes purely from historical data in which there have been no such changes combined with a fragile structure of assumptions. However, as in any study which brings data to bear on policy choice, there are disputable identifying assumptions: Is the success of these reforms due to a policymaker having decided on the right series of actions? Or is it instead part of the dynamics of hyperinflations that at a certain stage such actions finally become possible? Can we deduce the effect of modest policy actions on minor inflations by scaling down the effects of drastic policy actions on great inflations? Or is the world more nonlinear than that?

There is no difference in principle between these questions about Sargent's historical analysis and the corresponding questions about the use of a VAR model to project the effects of tighter monetary policy next year when we identify tighter monetary policy with positive residuals in the model's interest rate equation. Of course, concretely there may be a difference. Some may feel the questions about Sargent's identifying assumptions in applying the hyperinflation studies are more or less telling than those about identifying prediction errors in interest rates with policy. The point is that in any empirical study there will be debatable questions about identification—questions that will leave us more or less uncomfortable about applying the conclusions. The rational expectations framework raises such issues from a different angle, but it cannot avoid them.

Rational Expectations Models Versus Policy Analysis With Forecasting Models

Obviously, in principle, a rational expectations equilibrium model can be as good a forecasting model as a VAR model. In fact, in a world in which people were rational and markets were competitive, one might expect that the best forecasting models would all be rational expectations equilibrium models.

Possibly not so obviously, there is no logical inconsistency between a world of competitive markets and rational people and a world in which the best forecasting models are VAR models and optimal policy can be made by using VAR models under the simplest of identifying assumptions. This latter point requires some extended mathematical argument and is developed fully in a recent paper of mine (Sims 1985).

Thus, in the abstract, a model could include linear VAR models and nonlinear rational expectations equilibrium models as special cases of a more general framework. But in actual problems of policy choice and forecasting, there are tradeoffs. Rational expectations models have not been used successfully for large-scale macroeconomic forecasting. Despite their apparent naivete, nonstochastic spreadsheet approaches to modeling are more commonly used than conventional econometric models, VAR models, or rational expectations models. Since computers and human minds are both limited, a model's complexity must also be limited.

On the one hand, if one believes that the dynamic mechanisms of rational expectations are important and that we know enough about their nature to recover them from the data, then it may be worth accepting the burden of unbelievable simplifying assumptions which are inevitable in a usable rational expectations model. One accepts that the resulting model will not be a good forecasting model because of a belief that having rational expectations mechanisms explicitly in the model may make it more accurate in some kinds of policy analysis. Since the model is worse as a forecasting model, it is obvious that decreased accuracy in some kinds of policy analysis is being traded off against a hope for increased accuracy in some others.

On the other hand, one might believe that we know too little about the nature of dynamic economic behavior for careful modeling of rational expectations dynamics to be very helpful. In that case, using a model which aims at finding a conditional distribution of economic outcomes given policy actions, without detailing all the dynamic optimization underlying that conditional distribution, may look more attractive.

VAR methods have one deep conceptual advantage over current implementations of rational expectations equilibrium models: they make the connection of the model to a reduced form forecasting model completely explicit. This means that one can validate the probability assumptions in the reduced form by giving it a dry run through the historical data, having it generate forecasts with data available at each date in the past. The model's own probability distribution for forecast errors can be compared to the observed sample distribution of forecast errors. In contrast, standard econometric models and, even more so, rational expectations equilibrium models, cannot do this. A major part of the uncertainty about forecasts from both types of models is uncertainty about the fragile identifying assumptions built into them. Though the models are probabilistic, the distributions they generate for their own forecast errors are of limited practical use, since they ignore error arising from uncertainties in their own specification. Economists generally tend to put forward policy conclusions and forecasts as if they were surer of them than they ought to be, based on any objective evidence. This tendency is reinforced by the use of apparently scientific mathematical models that ignore a prime source of uncertainty about their conclusions. But VAR methods, or any others that close the gap between model stochastic specification and the observed forecast error distribution, hold the promise of grounding policy discussion much more firmly on objective facts.

In predicting the effects of policy actions, identifying assumptions will be required to make use of VAR models. These assumptions will be of uncertain validity. The point of VAR-based policy evaluation is to display explicitly the imposition of identifying assumptions on a carefully grounded reduced form forecasting model. This makes it easier to separate analysis of uncertainty about identification from analysis of uncertainty about the probability structure of the data.

Thus it remains true that there are two very different modern approaches to macroeconomic policy analysis: the first is a heavily empirical style, which ties analysis closely to forecasting models; the second is the rational expectations style, which is more complicated, less closely tied to data, more respectable in the eyes of most macroeconomists, and less used. From what has been said so far, perhaps the reader will agree that the issue is whether, in a particular application, one approach or the other requires implausible identifying assumptions. There is no general argument that one approach avoids difficulties which the other approach does not.

Identifying VAR Models

This section of the paper describes a class of convenient identifying restrictions on VAR models. Benefiting from it requires some previous exposure to econometric theory. (Less technically inclined readers can still perhaps gain some insight from the example presented in the next section without reading this one.)

The simplest way to achieve an identifying interpretation of a VAR model for policy choice is to assume that policy variation can be identified with the residual in one (or more) of the model's equations. In that case, policy projections can be made in the standard way—by dropping the policy equations from the model and treating the others as exact. This way of achieving identification is, as we pointed out, exactly what underlies the usual use of econometric models to project the effects of policy, whether they are explicitly VAR models or not. It is also exactly the identifying assumption underlying rational expectations monetarist models, in which unpredictable changes in money stock are postulated to be generated entirely by erratic policy choices.[5] It is, however, a restrictive approach to interpreting a model.

Recently a number of economists have explored ways to interpret VAR models which preserve a good deal of the simplicity and convenience of identifying policy with prediction errors in policy variables, without being quite so restrictive.[6] The setup is as follows. We suppose that there are behaviorally distinct sources of variation collected in a vector $e(t)$ and that these sources drive variation in the economy. Some elements of $e(t)$ are random fluctuations in policy. A model connects the observable vector of data, $Y(t)$, to current and past values of the driving disturbances according to

$$(8) \qquad \sum_{s=0}^{\infty} A(s)Y(t-s) = \sum_{s=0}^{\infty} B(s)e(t-s).$$

The VAR model for Y will have the form

$$(9) \qquad Y(t) = \sum_{s=1}^{\infty} C(s)Y(t-s) + u(t)$$

where $u(t)$ is the one-step-ahead prediction error in Y. Equation (9) can be solved to yield the impulse response matrixes for Y, $G(s)$, which satisfy

$$(10) \qquad Y(t) = \sum_{s=0}^{m} G(s)u(t-s) + H(m)Y(t-m)$$

where $H(m)$ depends on m but the G sequence does not. The ith row, jth column of $G(s)$, $G_{ij}(s)$ gives the response of $Y_i(t+s)$ to a unit disturbance in $u_j(t)$. A good part of the appeal of VAR models as descriptive devices is that the u vector, or at least some elements of it, is often under plausible identifying assumptions nearly the same as some elements of the e vector. In that case, part of the G matrix can be interpreted directly as responses to policy actions.

However, the general situation is that the behavioral disturbances of equation (8) are not the same as the u's. If the Y's are a rich enough list of data so that we could, if we knew the A and B matrixes, recover $e(t)$ at each t from knowledge of current and past values of $Y(t)$, and if the e process is serially independent, then (8) implies the following:[7]

$$(11) \qquad A(0)u(t) = B(0)e(t).$$

Then $u(t) = A(0)^{-1}B(0)e(t)$, and we can substitute in equation (10) to obtain responses of Y to e by replacing $G(s)$ with $G(s)A(0)^{-1}B(0)$, for all s.

The C and G coefficient sequences in equations (9) and (10) can always be recovered from the data by relatively simple estimation procedures. A and B in equation (8) cannot be recovered from the data without identifying assumptions that restrict their form. If the identifying assumptions restrict only $A(0)$, $B(0)$, and $\Omega = \text{var}(e(t))$, then they may imply restrictions on $\Sigma = \text{var}(u(t))$, but they will not imply any restrictions on C or G. Since efficient estimation of C is unaffected by restrictions on Σ, we can in this case proceed stepwise: first, estimate C by the usual simple VAR methods and form an unrestricted estimate of Σ from the estimated VAR residuals; second, use the restrictions to extract estimates of $A(0)$, $B(0)$, and Ω from the unrestricted Σ

[5]For examples of such rational expectations monetarist models, see Barro 1977 or Sargent and Wallace 1976.

[6]For example, Litterman (1984), Blanchard and Watson (1984), Blanchard (1985), Bernanke (forthcoming), and Mazon (1985).

[7]Sargent and Hansen (1984) have pointed out that the assumption that Y is a rich enough list of data is not innocuous. However, identifying assumptions are never innocuous, and this one will often be reasonable. It can fail if we fail to include in the model those variables which move directly in response to policy actions. It requires also that $B(0)$ be square (i.e., that the number of variables in the model match the number of behavioral disturbances in the e vector).

matrix. The responses of the system to policy disturbances then are available from the formula $G(s)A(0)^{-1}B(0)$, in which we can use the G from the original unrestricted VAR estimate together with the $A(0)$ and $B(0)$ estimates from the second step of the estimation.

The most straightforward example of identifying restrictions on $A(0)$, $B(0)$, and Ω is the Wold causal chain. According to this idea, Ω should be diagonal, $B(0) = I$, and $A(0)$ should be triangular and normalized to have ones down the main diagonal when variables are ordered according to causal priority. Using the fact that with $B(0) = I$, $\Sigma = A(0)\Omega A(0)'$, the triangularity of $A(0)$ implies that, once we have put the variables in the proper order, we can recover $A(0)$ and Ω from Σ as Σ's unique LDL decomposition. That is, it is known that there is a unique way to express a positive definite matrix Σ in the form LDL', where L is lower triangular with ones down its diagonal, and D is diagonal. Applying a standard LDL algorithm to Σ gives us $A(0)$ as L and Ω as D. This triangular orthogonalization has become a standard practice as part of the interpretation of econometric VAR models.

One can also apply the Wold chain idea only to two blocks of variables in the system, which requires $A(0)$ to be block triangular and Ω to be conformably block diagonal. This block Wold chain framework underlies all the standard econometric theory of simultaneous equations models, in that it is equivalent to dividing Y into two components, one endogenous and the other predetermined. If $A(0)$ is lower block triangular, then the upper part of the Y vector meets the usual criteria for predetermined variables, namely that their current and past values are uncorrelated with current values of the disturbances in the equations corresponding to the lower block. The block Wold chain structure does not by itself achieve identification—there remain more free coefficients in $A(0)$ and Ω than in the Σ matrix. In the standard approach to simultaneous equations, additional identifying restrictions are imposed on the A sequence. [The B sequence is taken as $B(0) = I$, $B(s) = 0$, for s nonzero.] If the additional restrictions involve $A(s)$ for positive s, then they will generally imply restrictions on C and invalidate the simple two-stage identification framework. But the standard framework does apply to the case where restrictions are imposed only on $A(0)$. The usual rank and order conditions for identification apply, and the usual array of simultaneous equations estimators can be applied as if equation (11) with $B(0) = I$ were the whole model and the estimate of Σ obtained from the unrestricted VAR model were the moment matrix of the data. The standard asymptotic distribution theory applies without modification to estimators constructed from (11) and the estimated Σ matrix.[8] (An example of empirical research exploiting this convenient framework appears in Mazon 1985.)

There is no need, though, to remain tied to the convention that restrictions on Ω are applied only in conjunction with block triangularity restrictions on $A(0)$. The reason it became conventional to impose such restrictions is that econometricians do accept the idea that often it will be reasonable to assume that behaviorally distinct sources of stochastic variation should be independent. This idea can be useful even when not coupled to a block triangularity assumption on $A(0)$. We might, for example, assume Ω to be diagonal and achieve identification by imposing additional restrictions on $A(0)$, thereby insisting that all behaviorally distinct sources of disturbance should be uncorrelated.

An Example of Identifying a VAR Model

We will examine a simple six-variable quarterly postwar VAR model of the U.S. economy over the period 1948:1–1979:3. The period is truncated at 1979:3 to avoid the likely need to allow for a shift in money supply behavior around 1979:4. The variables are real GNP (Y), real business fixed investment (I), GNP price deflator (P), the M1 measure of money (M), unemployment (U), and Treasury-bill rates (R). This list of variables is chosen to allow an interesting discussion while still being manageably short. The list is too short, however, for us to be sure that it will allow us to distinguish among behaviorally distinct sources of disturbance. In particular, since it does not include any monetary aggregate closely controlled by policy authorities, it forces us to lump together the money supply effects arising from the way the banking system uses reserves and effects arising directly from actions by the Federal Reserve System. Also, since the list includes no fiscal variables, changes in expectations about future tax and spending policies—which could be important— can show up in the model only indirectly.

A Model With Weak Restrictions

When a VAR model is fitted to these data, using four lags on each variable plus a constant term, and with a weak Bayesian prior with independent random walks as the

[8] The reason this works out so conveniently is that the likelihood for the data has a block diagonal information matrix. This is the condition Durbin (1970) pointed out as needed to guarantee that two-step estimation, treating first-step parameter estimates as if known exactly, yields the same asymptotic results as joint maximum likelihood.

prior mean, the resulting estimated system has the impulse responses plotted in Chart 1.[9] Each of the small graphs represents the response, over 32 subsequent quarters, of a variable in a given row to a one-standard-deviation innovation (prediction error) in a variable in a given column. In the notation of the previous section, the A matrix has been taken to be lower triangular, with the variables ordered as shown in the chart. The disturbances can be interpreted as behaviorally distinct if one believes that a behavioral version of the system would have the form of a Wold causal chain. The vertical scale is the same for all the plots in a given row, so the relative size of effects of various innovations on a given variable can be compared by looking at the relative sizes of plotted responses across that variable's row.

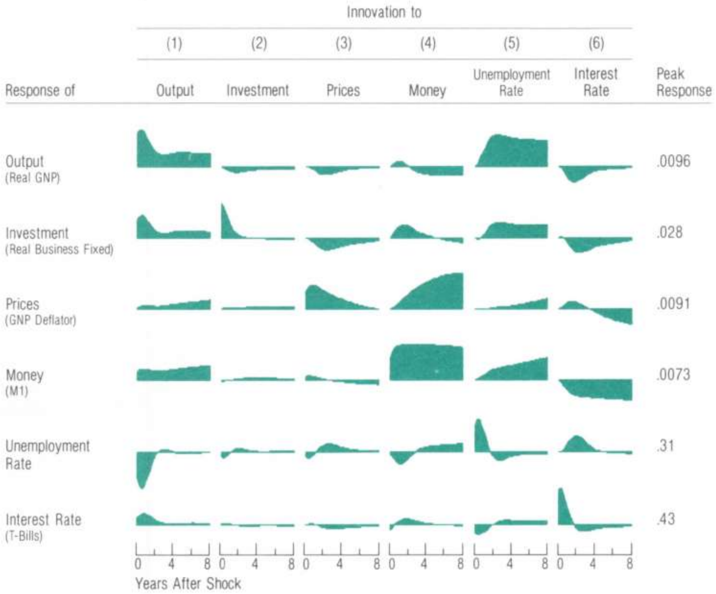
Many of the response patterns in Chart 1 do appear to have straightforward behavioral interpretations. The unemployment shock (column 5), for example, looks like a labor supply disturbance. Unemployment rises temporarily, returning to normal within two years or so, and output rises steadily over about two years, then remains at a higher level. Investment also follows the pattern of output. Interest rates decline slightly and temporarily. Money expands smoothly, without a correspondingly large expansion in the price level, indicating accommodation of real balances to a higher level of real activity. It is remarkable that the unemployment disturbance effect emerges as the largest single influence on the long-run behavior of output.

Though several other of the response patterns lend themselves to the telling of interpretive "stories" like the one we just developed for unemployment, let us focus now on a problem: Do we see money supply and demand disturbances in this chart? The rational expectations monetarist interpretation is that money innovations are

[9] The prior used here and its implementation are simplifications of the procedures used by Doan, Litterman, and Sims (1984).

Chart 1

Response Functions of Variables in a Weakly Restricted VAR Model*



*Each small graph is the response of the variable labeling its row to a one-standard-deviation disturbance of the type named at the top of its column. Each response is plotted over an 8-year (32-quarter) time span, and the responses in a given row are all measured in the same units. The larger filled-in areas in a given row correspond to the disturbances that are important in generating movements in that row's variable. All variables are measured in logarithms except for the unemployment rate and interest rate, which are taken as percents. The vertical scales are set so that the largest deviation from the horizontal axis is the same distance on the graph in each row, and the size of this peak response is noted at the right of each row. Chart 1 is the responses to triangularly orthogonalized innovations.

money supply disturbances. The money column (4) of Chart 1 shows that prices respond strongly and persistently, although with a delay, to money innovations, while real variables respond very weakly. The weakness of the real responses does not fit rational expectations monetarist theory well. Also, there is a negligible interest rate response. Since there is no immediate response in the level of prices or output, it is difficult to explain how, with a fixed money demand schedule, the public is led to accept a jump in the money stock.

Many economists would rather, in any case, treat short-term interest rates as the monetary policy variable. The interest rate column (6) of Chart 1 shows substantial, temporary negative effects on output of a rise in interest rates. It also shows a large, persistent negative effect of the interest rate rise on the money stock. However, there is little persistent effect of the monetary contraction on prices, and indeed an initial positive effect on prices. Furthermore, with a fixed money demand schedule, it is hard to see why the quickly dissipated rise in nominal interest rates leads to a very persistent decline in the money stock.

Two Different Identifications of the Model

Charts 2 and 3 represent two different identifications of this VAR system. In each case, identification is limited to separating demand and supply of money from each other and from a block of remaining equations. In each case, the behavioral disturbances *e* are taken to be mutually uncorrelated.

□ *The First Identification*

The model generating Chart 2 postulates that the money supply equation connects money innovations with interest rate innovations and that no other variables enter. This rests on the idea that the monetary authority and the banks can see interest rates and indicators of movements in monetary aggregates immediately, but can only react to the remaining variables in the economy after a delay because data on these variables are released later. The money demand equation in this model allows money innovations to depend on innovations in interest rates, output, and the price level. Investment demand is postulated to make investment innovations equivalent to investment demand innovations—that is, investment demand reacts to other variables only with a delay. The three other equations are postulated to determine output, price, and unemployment innovations from three more independent innovations and the innovations in investment and interest rates. These three equations are normalized to have a block triangular set of coefficients on price, output, and unemployment. Note that there is an important identifying restriction: the money stock innovation is not allowed into the equations other than money supply and demand; monetary sector variables feed back into the remainder of the model only via interest rates.

The equations for this system and their estimated coefficients are listed below:

Money Supply

$$(12)\quad r = \underset{(46.0)}{71.2}m + e_1$$

Money Demand

$$(13)\quad m = \underset{(.103)}{.283}y + \underset{(.128)}{.224}p - \underset{(.0043)}{.0081}r + e_2$$

Output

$$(14)\quad y = -\underset{(.00308)}{.00135}r + \underset{(.024)}{.132}i + e_4$$

Price

$$(15)\quad p = -\underset{(.0012)}{.0010}r + \underset{(.054)}{.045}y + \underset{(.015)}{.00364}i + e_5$$

Unemployment

$$(16)\quad u = -\underset{(.045)}{.116}r - \underset{(2.5)}{20.1}y - \underset{(.72)}{1.48}i - \underset{(4.1)}{8.98}p + e_6$$

Investment Demand

$$(17)\quad i = e_3 .$$

The data on all variables are logged except for *U* and *R*, which are measured in percent. Approximate standard errors are given in parentheses below the coefficients.[10]

The money demand and supply equations are perhaps surprisingly reasonable, given that they are identified only by exclusion restrictions on innovations. The response to a money supply shock shown in Chart 2 is quite different from either the money shock or interest shock response of Chart 1, being close to a difference of those two response

[10]The standard errors are based on the approximate second derivative matrix accumulated during the search for the likelihood maximum by a BFGS-update optimization algorithm. (See Gill, Murray, and Wright 1981, p.119.) Such an approximate Hessian need not be very accurate, even when the search algorithm has worked well in finding the likelihood maximum attained.

Charts 2 and 3

Response Functions of VAR Model Variables for Two Different Identifications*

Chart 2 Responses for the First Identification

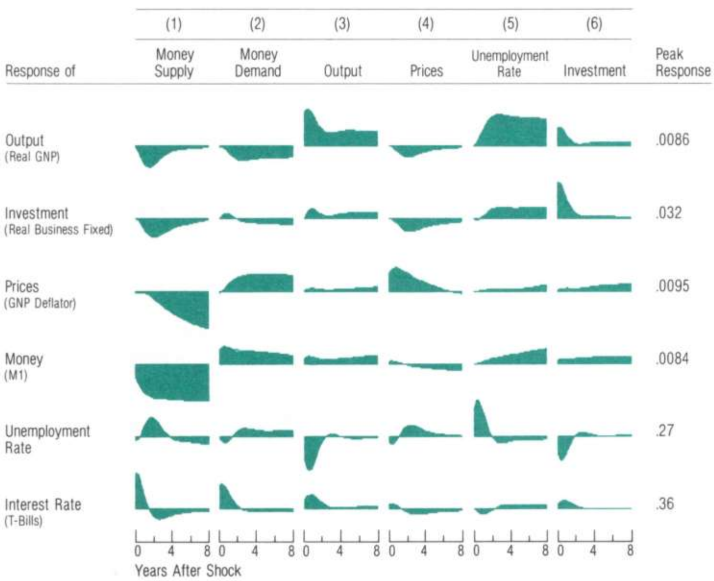
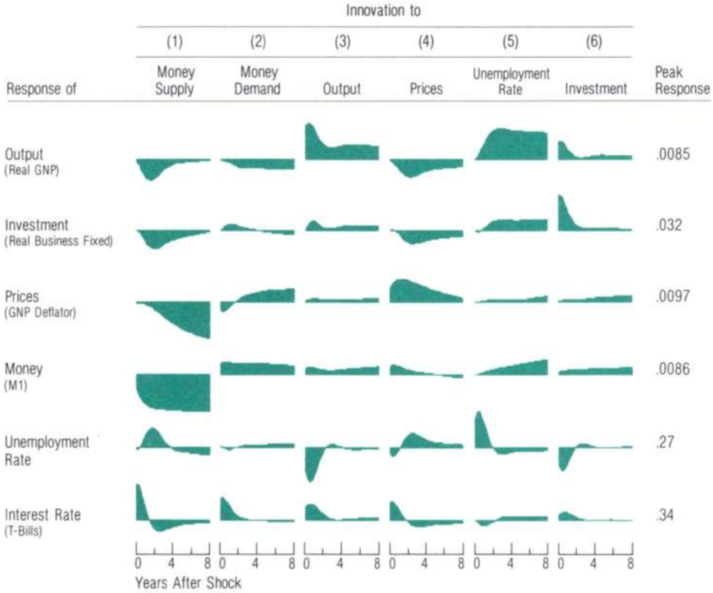


Chart 3 Responses for the Second Identification



*See note for Chart 1. The identifying restrictions used here are designed to get better estimates of responses to money supply shocks. Each set of identifying assumptions is described in more detail in the text.

patterns. Now a money supply shock (Chart 2, column 1) consists of simultaneous jumps upward in interest rates and downward in money stock. The money stock movement increases over the next few quarters and remains very persistent. The interest rate movement remains temporary, but now the delayed yet persistent movement in prices shrinks real balances, so that the persistence in the nominal money stock, despite the evanescence of the interest rate rise, is not puzzling. Output, investment, and unemployment all take plausible business-cycle-like excursions in response to the money supply shock.

The estimated response to a money demand disturbance (Chart 2, column 2) is harder to rationalize. That a money demand shock should raise interest rates and the money stock initially and result in a persistent decline in output is not unreasonable. But that it should have no immediate effect on prices and, in the long run, should raise prices, is not so reasonable. There are several possible explanations in terms of weaknesses in identifying assumptions. One is the problem that by putting M1 in the money supply equation, we forego separating bank behavior from Federal Reserve behavior and thereby possibly also forego properly separating demand from supply behavior. Another explanation is that without fiscal variables, a change in the current debt or in expected future real federal surpluses might generate what is identified here as a money demand shock. A rise in the debt through a current deficit, unmatched by a rise in expected future real surpluses, would generate the following effects: an increase in desired money holdings (through an increase in the scale of the public's nominal investment portfolio), a rise in the interest rate, and upward pressure on prices. Finally, the model does not contain exchange rate or commodity price variables. A fall in the value of the dollar or a rise in commodity prices could create increased transactions demand for money, anticipated GNP-deflator inflation, a rise in interest rates, and a decline in output.

□ The Second Identification

Before giving up on identifying money demand, however, two or three more experiments with modified sets of identifying restrictions were tried. In one, the money innovation was allowed to enter the price equation directly. This specification made the reaction to an estimated money demand shock no more reasonable, but it made the price equation disturbance start to behave like a money demand shock. Listed below are the innovation equations for this interpretation of the system, renormalized so that the price equation is normalized on money, and vice versa:

Money Supply

$$(18) \quad \underset{}{r} = \underset{(13.0)}{82.0m} + e_1$$

Money Demand

$$(19) \quad m = \underset{(.08)}{.29y} - \underset{(.0164)}{.0088i} + \underset{(.22)}{.90p} - \underset{(.0022)}{.0082r} + e_2$$

Output

$$(20) \quad y = \underset{(.0021)}{-.0021r} + \underset{(.02)}{.13i} + e_3$$

Price

$$(21) \quad p = \underset{(.0025)}{-.0058r} + \underset{(.06)}{.22y} - \underset{(.11)}{.50m} + e_5$$

Unemployment

$$(22) \quad u = \underset{(.04)}{-.12r} - \underset{(1.1)}{20.1y} - \underset{(.36)}{1.49i} - \underset{(1.17)}{8.97p} + e_6$$

Investment Demand

$$(23) \quad i = e_4 .$$

Chart 3 itself now provides a more believable response to the money demand shock (column 2). Interest rates rise, money stock rises, and prices fall at first. Output falls and stays low. The money stock remains high, and prices eventually rise to eliminate the higher real balances. It is not easy to understand why output should remain depressed for so long after interest rates and real balances return to normal, but the response is small enough that it may not be statistically significant. Note that the effects of the money demand shock on other variables are all relatively weak, but that the money demand shock produces an initial impact on money stock of the same order of magnitude as the initial impact of the money supply shock. This means that, though money supply shocks in this model are a powerful influence on both prices and real business cycle behavior, the quarter-to-quarter movements in the money stock contain a large component of money demand shifts. The model suggests that stable money supply behavior would help stabilize the economy, but it also suggests that stable money supply behavior is quite different from stable money stock in a quarterly time frame.

While Chart 3 is appealing, equations (19) and (21) are less so. Why should money demand be more responsive to price than to output in the short run? What is the interpretation of the negative response of price to money in the nonmonetary block of the model? The conclusion must be that this interpretation, like the previous one, is subject to serious question. One ought to have a model including exchange rates, commodity prices, fiscal variables, and a more directly controlled monetary aggregate before drawing any firm implications for policy.

What the Exercise Suggests

Nonetheless, this exercise suggests that a relatively simple expansion on the Wold causal chain identification scheme, which has been regularly applied in VAR models, can yield important insights. Here it appears that money supply effects are more important for inflation and output—and that they tie short-run output effects to long-run price effects more tightly—than one would think if limited to a framework in which behavioral disturbances coincide with variable innovations. If this interpretation were correct, it would imply that conventional econometric macromodeling which ignores endogeneity of monetary policy instruments is likely to be quite misleading. It also suggests that the more naive versions of both rational expectations monetarism and conventional monetarism are mistaken. The effects of money supply shocks do not operate through contemporaneous price surprises, as simple rational expectations monetarism would suggest, nor are money stock innovations nearly entirely made up of money supply shocks. Policy prescriptions for eliminating fluctuations of the money stock over short periods are not supported. Although the model attributes most money stock variation over long horizons to money supply shifts, much of the unpredictable quarter-to-quarter variation in money stock comes from demand and other influences.

Were the patterns of responses to money supply shocks found here to be substantiated in a more detailed VAR model, it would be reasonable to use them in determining optimal policy reactions to disturbances in money demand, exchange rates, labor supply, or the like. Of course, if use of the model in this way led to drastic shifts in policy, there would be questions about identification. If it suggested unprecedentedly large movements in policy instruments, it would be doubtful that the VAR model's linear structure would remain fixed. If it suggested dramatic changes in the trend or degree of smoothness in prices, the rational expectations critique suggests a reason for concern about a different kind of nonlinearity.

But policy analysis based on a rational expectations equilibrium model is suspect for the same class of reasons. Such a model will almost inevitably have a weaker, more heuristic connection to the data than a VAR model, so that all its implications are suspect from the start. It will rely on approximate assumptions about functional forms for tastes and technology in the economy—assumptions that will be accurate only over some reasonable range of variation in the economy's behavior. It will make assumptions about market structure and individual rationality which are sure to be incorrect to some degree. Since the limitations of such a model are different from the limitations of a VAR model, even though conceptually similar, it will often be useful to think about the consequences of policy changes in the context of such rational expectations models as well as in the context of VAR models. There is a tradeoff between types of models for policy analysis, not a hierarchy of them.

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