Comment on Glenn Rudebusch's "Do Measures of Monetary Policy in a VAR Make Sense?"

by Christopher A. Sims

Department of Economics

Yale University

email: sims@econ.yale.edu

July 1996

Comment on Glenn Rudebusch's "Do Measures of Monetary Policy in a VAR Make Sense?" by Christopher A. Sims, July 1996

Generalities

The identified VAR literature has in the last few years begun producing consistent, somewhat controversial results. The results challenge conventional wisdom about the nature of monetary policy and its effects, and the methods rest on careful attention to simultaneity problems. The literature accordingly has begun attracting vigorous criticism, both from economists comfortable with one version or another of conventional wisdom and from economists comfortable with the currently fashionable view that macroeconomics, properly executed, never requires thinking about more than one regression equation at a time.

Before addressing the criticisms put forth in Glenn Rudebusch's paper, let me describe broadly what the literature has discovered.

1) Most variation in monetary policy instruments is accounted for by responses of policy to the state of the economy, not by random disturbances to policy behavior.

2) Responses of real variables to monetary policy shifts are estimated as modest or nil, depending on the specification.

3) Monetary policy has historically increased interest rates in response to non-policy shocks that create inflationary pressure by more than would it would have under a policy of fixing the monetary stock.

4) A reasonable picture of the effects of monetary policy shifts emerges only under identifying assumptions of delay in the reaction of certain "sluggish" private sector variables to monetary policy shifts.

Most papers in the literature have not emphasized these conclusions, probably because it has until recently been regarded as a substantial accomplishment just to display a model that is consistent with multivariate time series data and implies reasonable effects for monetary policy shifts. The small real effects have particularly not been emphasized, since it is apparently sometimes thought that finding substantial real effects for monetary policy changes helps make identified VAR analyses credible. That these conclusions are nonetheless borne out in the literature is argued in somewhat more detail in my paper with Tao Zha, "Does Monetary Policy Generate Recessions?" available at ftp://ftp.econ.yale.edu/pub/sims/mpolicy. They are confirmed for a number of European countries and Japan in work by Soyoung Kim.

There is a view, which Milton Friedman used to restate regularly some years ago, that erratic variation in monetary policy is the primary source of business cycle fluctuations, with each postwar US business cycle largely explainable via the pattern of monetary policy variations preceding it. Friedman used to defend this view via statistical analysis that took the time path of a monetary aggregate as a sufficient statistic for the time path of monetary policy. The recent VAR literature decisively undercuts this way at looking at history, and as far as I know there are no attacks on the VAR literature that explicitly put forth a quantitative model that contradicts the VAR literature on this score. This paper by Rudebusch does not, apparently, put forth

Friedman's or any other approach to quantitative analysis of the effects of monetary policy as a better alternative to identified VAR modeling. As far as I can see, this paper takes the gloomy view that the VAR literature is so deeply flawed as to be useless, but not because there is a better way to accomplish the same purposes.

Specifics

Many of the paper's complaints are what I would call "quibbles." They criticize modeling choices about aggregation, time aggregation, time variation, and linearity that are an essential part of any quantitative macroeconomic modeling. Of course there is some general distaste for quantitative macroeconomic modeling these days, precisely because such choices are necessary. I will address at least some of these quibbles below, but it would be interesting to know if the author believes that some other approach – calibrating stochastic equilibrium models? traditional simultaneous equations methods? – is less subject to this kind of criticism. To me at least, these criticisms, though in some cases they deserve discussion, are not very interesting, so I postpone them to the latter part of these notes.

There is one component of Rudebusch's scattershot attack on identified VAR modeling that raises legitimate and interesting research questions.

Let's Look at the Policy Shock Time Series

In the middle of page 18, Rudebusch makes the most interesting point in his paper. His own VAR-style policy equation has residuals that have a correlation of only .12 with those of another VAR study by Bernanke and Mihov. This result may not be representative of the literature as a whole, but it deserves wider examination. Zha and I, for example, find that the monetary policy shocks for our two specifications (one using the Treasury Bill Rate and M2, the other replacing these variables with the Federal Funds Rate and Total Reserves) have a correlation of only .29. Examination of the policy shock time series in our two specifications show that they disagree substantially on when the biggest policy shifts occurred. I expect that when this issue is more widely examined it will be confirmed that despite their broad agreement on the nature of the response of the economy to monetary policy shocks, identified VAR studies disagree rather substantially about the history of policy disturbances.

Further study of these disagreements about policy shock time series will be important in resolving the disagreements about the size and existence of real effects of monetary policy shocks. But Rudebusch claims that "the VARs' estimated impulse responses will only be as good as the VARs' measures of exogenous shocks." If this were true, it would imply that the disagreement among these models concerning the policy shock time series discredits their broad agreement on the effects of policy shocks on the economy. However, it isn't true.

How Models Can Disagree on Policy Shocks, While Agreeing on their Effects

Multivariate models can be hard to grasp intuitively. An appealing but misleading way to think about identified VAR's is that they "identify" a policy shock time series, then find the effects of policy essentially by regressing everything else on the policy shock time series. This would imply that if two identified VAR's that find very different policy shock time series agree on the effects of policy shocks on other variables, this is only an accident (or the result of data mining). This interpretation of VAR's did apply to some of the earliest, rational expectations monetarist, approaches to estimating the effects of monetary policy. Those models implied that the innovation in a monetary aggregate was determined entirely by policy behavior. Some researchers, Robert Barro for instance, estimated models that literally constructed money innovations and regressed other variables on the money innovations to find the effects of policy.

But the modern identified VAR literature is not doing this. It works with multivariate models that can be understood only in a simultaneous equations framework. Suppose we were dealing with the simplest supply and demand simultaneous equations model. Everyone understands that if both equations in such a model are to be identified, then each equation must have a "shifter" variable that does not appear in the other equation – for example weather as a supply shifter and income as a demand shifter. The effect of a shock to supply (a change in its disturbance term, which moves the supply curve up and down or left and right, depending on the normalization) is a movement along the demand curve. It is accurately estimated if we have an accurate estimate of the demand curve. This requires that there be substantial variation in the weather, the supply shifter. On the other hand, the accuracy of the estimates of the supply equation shocks depends on the accuracy of the estimates of the coefficients in the supply equation. These depend on the amount of variation in income, the demand shifter. Thus in this simple example it is clearly not true that the estimates of the responses to supply shocks are "only as good as" the estimates of the supply shocks themselves. In fact, as the amount of variation in weather grows, while the amount of variation in income shrinks, the accuracy of the estimated supply shocks can get arbitrarily bad while the accuracy of the estimate of the response to supply shocks becomes arbitrarily accurate.

Tao Zha and I have checked our own model to see if this is what is happening. We find that this simple story is not adequate. We construct the posterior distribution of the correlation of the true shock series with our point estimate of the shock series. The distribution shows almost no probability on correlations below .8, and most probability concentrated on correlations above .9. Plots of typical random draws from the posterior distribution of shock time series show close agreement around the dates of the largest shocks. It is interesting to note, though, that the 88-94 period that covers most of Rudebusch's data set is a period of small shocks, and within this period correlations are substantially weaker.

Our conclusion is, then, that the .29 correlation between the policy shocks of our two specifications is much lower than can be accounted for by each model's own uncertainty about the policy shock series. It comes from a "statistically significant" difference in specifications.

Could it be then that two models have different specifications, implying quite different policy shock time series, yet both accurately estimate the same response to policy shocks? Consider again our simple supply and demand model. Suppose there are two supply shifters, weather and insect density. Suppose one model includes the weather variable, but omits, and thus relegates to the error term, insect density. The other model does the reverse. So long as both supply shifters are legitimate exogenous variables, uncorrelated with the disturbance term in the demand equation, both models can lead to accurate estimates of the demand equation, because each offers one legitimate instrumental variable for that equation. But of course, since each model includes the other's supply shifter in the "supply shock", there is no limit to how different their estimated supply shock time series might appear.

Tao Zha and I have made the point that there is a common characteristic across all the apparently various VAR studies that have found responses to monetary policy that appear reasonable, avoiding the price puzzle. Each of these studies, in one way or another, assumes that there are one or more "sluggish" private sector variables, usually including GDP, industrial production, or investment, that does not respond within the time unit to monetary policy variables. This variable or group of variables therefore qualifies as instruments for the money supply sector of the model, while lagged monetary policy variables – monetary aggregates and interest rates – become legitimate instruments for the non-policy equations describing the determination of the sluggish variables. Just as in the simple supply and demand model, the agreement among models in the exclusion of contemporaneous values of monetary policy instruments from the equations determining certain key private sector aggregates leads to agreement on the dynamics by which those variables are determined. The models disagree much more on which variables they include in the policy reaction function, and therefore also disagree more on the time series of policy shocks. Several of the models in the literature, for example, have a block recursive structure that entails that the group of sluggish variables coincide with the group of variables that enter contemporaneously in the reaction function. On the other hand Tao Zha and I, and Soyoung Kim, exclude contemporaneous GDP and GDP deflator from the reaction function, while still treating these variables as sluggish.

Fed Funds Futures

The paper demonstrates at some length that forecasts of the federal funds rate implicit in futures market prices are more accurate, and little correlated with, those obtained from a reduced form prediction equation for the fed funds rate similar to those in some VAR's. This point is of some interest, but it does not have the implications the paper claims for it.

It is a main point of the literature the paper criticizes that there is no reason in principle to assume that unforecastable changes in the federal funds rate are policy shocks. The literature uses a variety of identification schemes, and as Rudebusch points out, comes up with widely varying estimates of the history of policy disturbances. The "VAR policy shocks" that Rudebusch constructs for this paper are, as he points out, nearly identical with one-step-ahead prediction errors for the fed funds rate from a VAR. It is clear then that they are not nearly identical to the policy shocks in some other identified VAR's. But even if they were, the fact that futures market predictions produce better forecasts than the VAR variable list Rudebusch considers does not imply that the futures market prediction errors are good measures of policy shifts. The Fed can and does respond systematically within days or hours to current information on the private economy. Forecast errors for policy instruments therefore inevitably confound forecast errors for private sector variables that influence policy with forecast errors for policy itself. So if what futures markets can predict is policy, but not the private sector variables that systematically influence policy, the futures market forecast errors, though smaller, would be clearly worse as measures of policy shocks than the VAR errors.

In other words, much of the paper's discussion of the results for fed funds future starts from a false premise. That doesn't prevent it from being of some interest. Any variable that can substantially improve reduced form VAR forecasts is an interesting candidate for inclusion in an identified VAR analysis. Note, though, that if a model is being used to interpret behavior, not just for forecasting, it is not generally a good idea to include every variable that improves reduced form forecasts. There are a number of possible choices for a short interest rate, for example, and the models have generally chosen a single short rate, though the choice has ranged over the federal funds rate, commercial paper rates, treasury bill rates, and (usually in other countries) call rates. Short interest rates do not move perfectly together, but they are tightly related. Few people would be surprised to find out that over short horizons treasury bill rates help predict commercial paper rates or vice versa, yet this does not lead to many structural models containing both.

The reason for this is that in some sense, for structural modeling purposes, short interest rates are all measures of a single concept. If one includes multiple short rates while still hoping to obtain behavioral interpretations of the results, it is necessary carefully to model the frictions, institutional factors, etc. that lead to discrepancy among the rates, so as to distinguish the movement in the underlying short rate concept from the factors that distinguish the rates. Commonly such detailed modeling appears not to offer insight or accuracy commensurate with the effort it requires, so modelers settle on a single measure of short rates.

Federal funds rates futures are not short rates, but it turns out that the extra forecasting power they provide is a subset of that available by adding additional short rates to a VAR. Rudebusch kindly provided me with the data set he used for the paper, extended to include also futures prices from the middle of the month preceding the contract month as well as the end-of-month data used in the paper itself. It is important to keep track of the precise dating, here, because results are strongly affected by it. The series Rudebusch uses has a half-month timing advantage over the monthly average data used in most VAR's. It can therefore appear spuriously to have extra information just because of its timing advantage. Below are results from a regression of the federal funds rate on lags of itself, of the treasury bill rate, and of the discount rate, as well as on the futures price prediction as of 15 days before the start of the current month.

```
Dependent Variable FFR - Estimation by Least Squares
Monthly Data From 88:11 To 95:03
Usable Observations     77      Degrees of Freedom    69
Centered R**2     0.996339      R Bar **2   0.995967
Uncentered R**2   0.999481      T x R**2      76.960
Mean of Dependent Variable      5.7343662338
Std Error of Dependent Variable 2.3468022835
Standard Error of Estimate      0.1490275966
Sum of Squared Residuals        1.5324364930
Regression F(7,69)                 2682.5155
Significance Level of F           0.00000000
Durbin-Watson Statistic             2.033023
Q(19-0)                            15.789015
Significance Level of Q           0.67130548

   Variable                       Coeff       Std Error      T-  Stat      Signif
*******************************************************************************
1.  Constant                 -0.005249077  0.071294538     -0.07363  0.94152162
2.  FFF15{1}                  0.097391950  0.176886917      0.55059  0.58369385
3.  FFR{1}                    0.686652699  0.180151603      3.81153  0.00029683
4.  FFR{2}                   -0.038487564  0.137030961     -0.28087  0.77965240
5.  TB3MS{1}                  0.756340771  0.192798902      3.92295  0.00020403
6.  TB3MS{2}                 -0.252331336  0.155386818     -1.62389  0.10895900
7.  DISCRT{1}                -0.406015930  0.151887938     -2.67313  0.00937140
8.  DISCRT{2}                 0.149247075  0.144783362      1.03083  0.30622024
```

Clearly there is no forecasting power in the futures market forecasts that is not already available in lagged funds rate data, lagged treasury bill rate data, and lagged discount rate data.

The same regression run with the futures market forecasts from the end of the preceding month, instead of the middle of the preceding month, makes the futures market variable very significant, confirming that time aggregation issues are critical to interpreting results here. However even the end-of-preceding-month fed funds future variable leaves substantial predictive power for the lagged interest rates. Since this casts doubt on Rudebusch's strong claims of market efficiency, I display the results below, though they are not very relevant to the main points of these notes.

```
Dependent Variable FFR - Estimation by Least Squares
Monthly Data From 88:11 To 95:03
Usable Observations     77      Degrees of Freedom    69
Centered R**2     0.996785      R Bar **2   0.996459
Uncentered R**2   0.999544      T x R**2      76.965
Mean of Dependent Variable      5.7343662338
Std Error of Dependent Variable 2.3468022835
Standard Error of Estimate      0.1396545143
Sum of Squared Residuals        1.3457334526
Regression F(7,69)                 3056.0472
Significance Level of F           0.00000000
Durbin-Watson Statistic             1.990067
Q(19-0)                            15.272533
Significance Level of Q           0.70512557

   Variable                        Coeff       Std Error      T-   Stat      Signif
*******************************************************************************
1.  Constant                 -0.003869406  0.066606552     -0.05809  0.95384209
2.  FFF1{1}                   0.456011245  0.144797876      3.14930  0.00241910
3.  FFR{1}                    0.453104681  0.171897568      2.63590  0.01035617
4.  FFR{2}                   -0.069977581  0.127293211     -0.54974  0.58427593
5.  TB3MS{1}                  0.406371377  0.178547413      2.27599  0.02595467
6.  TB3MS{2}                 -0.078785963  0.153419346     -0.51353  0.60921877
7.  DISCRT{1}                -0.325033483  0.141919120     -2.29027  0.02506690
8.  DISCRT{2}                 0.151405023  0.135602940      1.11653  0.26806756
```

This is not to say that in modeling the behavior of the Federal Reserve, which is certainly aware of Tbill rates and the discount rate without delay, we should not use data on them. But clearly using them raises fresh modeling problems. The Fed may respond to market forces that push Tbill rates away from the funds rate, but the Tbill market may also anticipate Fed policy moves. No single-equation approach will unravel this.

Quibbles

Time-Invariant Linear Structure

It is true that the identified VAR literature has maintained linearity assumptions for the most part. My own work [1993] has shown that in reduced form analysis certain nonlinear features and stochastic time variation in parameters can improve fit substantially. However the absolute sizes of the gains in fit from such features are not large, and the types of nonlinearity and time variation that proved useful in reduced form analysis would be difficult to integrate into an identified VAR analysis. Finding ways to introduce such features into identified VAR models is indeed likely to be worthwhile, as the models become more widely accepted and used. In the meantime, most of the existing studies have devoted some attention to robustness of results to sample splitting or omission of episodes where policy is widely thought to have been unusual.

Rudebusch apparently finds these sample split checks , or at least the way the VAR literature has used them, unconvincing. He does his own sample-split tests on the regression equations he has fit, which do not correspond precisely to any fit in the VAR literature, and finds statistically significant parameter shifts.

In the VAR literature, as in all macroeconomic time series modeling, one can find a range of approaches to handling parameter variation. At one end of this range are modelers who act as if they believe that macroeconomic models with completely time-invariant parameters exist, and that once statistically significant parameter variation has been demonstrated, a model can be dismissed as false. Rudebusch writes as if he is in this camp. Within the VAR literature, a similar point of view leads Gordon and Leeper [1995], for example, to use a shorter sample period than most of the rest of the literature. At the other end are modelers like Zha and myself, who view linearity and time-invariance as always more or less good approximations. We also construct tests for parameter-invariance, but we use the Schwarz criterion rather than F-tests, and find little evidence of important parameter variation.

The use of the Schwarz criterion rather than simple F-tests is important, even for researchers who believe in time-invariant macroeconomic models. Deciding whether there is time variation in parameters by conducting Chow tests with a standard significance level is an inconsistent decision procedure – when there is in fact no time variation, the procedure does not lead to correct decisions with arbitrarily high probability in large samples. Use of the Schwarz criterion is a consistent decision procedure. It amounts to using Chow tests in which the significance level is steadily tightened as sample size increases.

I do not mean to argue here that there is no room for improving models by allowing for time variation in parameters. There almost certainly is room for improvement along these lines. However the amount of time variation apparent in the data is weak, not strong, so as with nonlinearity, it makes sense at this stage to examine models that do not allow for time variation.

Rudebusch observes that some researchers have found quite different impulse responses in different parts of samples. Since this literature has avoided the complications of using Bayesian priors, it has regularly skated close to the edge of degrees of freedom problems. VAR's fit to short time series can produce extremely erratic impulse responses. It is therefore not news that sample splits can produce quantitatively important differences in estimated impulse responses. But the differences tend not to be large statistically, when checked by a consistent criterion like the Schwarz criterion.

The paper cites the difficulty that single-equation "reaction function" modelers have had in getting good, stable fits with regression functions attempting to explain monetary policy behavior. But reaction function modelers set out to "explain" monetary policy behavior, often with a focus on month-to-month or quarter-to-quarter variation. It is difficult to obtain a substantial R-squared in a regression of, say, changes in the monthly average federal funds rate, on anything. There is a temptation to overfit, therefore, finding specifications that work, but only for short periods. The identified VAR literature finds that most of the predictable part of monetary policy variables is low-frequency in nature, with short-run changes quite erratic. A high R-squared in predicting short-run changes in the federal funds rate is not the objective in identified VAR modeling. In such modeling the objective is accurate characterization of the probability distribution of policy shifts. This may result in a model that is fairly stable over time, attributing quite a bit of high-frequency variation to the disturbance in the reaction function. If the monetary authorities' short-run behavior follows rules that they perceive as systematic, but that change frequently and erratically, then a probability model that treats these rules simply as sources of random fluctuation, without trying to track them in detail, may be exactly correct. The fact that these fluctuations are the outcome of deliberate decisions, taken after a process of debate and discussion, does not make it illegitimate to treat them as random.

Information Sets

The paper cites, appealing to various sources, a list of variables that have not generally been used in published VAR studies but that have been considered possibly important influences on Fed behavior. These include the foreign trade deficit, the "stance of fiscal policy", "measures of political pressure", the value of the dollar, stock prices, "general financial liquidity", labor costs, housing starts, inventory-to-sales ratios, durable goods orders, and "reports on regional shocks". Some of these variables have appeared in some VAR studies. This is true of the labor cost variable and the value of the dollar – Zha and I use a wage variable in our system, Christiano, Eichenbaum and Evans have used exchange rates, and Soyoung Kim uses exchange rates. Indeed use of exchange rate variables is standard in the identified VAR literature for non-US economies. Zha and I use bankruptcies, one measure of "general financial liquidity". Bernanke and Gertler have used measures of residential construction. Others of these variables have not appeared in published VAR's, but have been tried at early stages of research at least by me, and no doubt by others as well. Stock prices and inventories are in this category. And finally some are variables that are both difficult to quantify and excellent candidates for relegation to the disturbance term in an identified VAR study – stance of fiscal policy and measures of political pressure seem to fit here.

There is a reason that, despite having for the most part been tried to some extent by VAR researchers, these variables have not become standard in the literature. They do not have major qualitative effects on conclusions and their behavior does not emerge as a key factor in interpreting results. Thus modelers, who are constantly pressing against constraints on the size of model that can be managed, have regarded these variables as expendable.

Occasionally a variable is suggested that does change conclusions or interpretations, and it then becomes part of the standard specification. Commodity prices is in this category. Eight years ago it was not part of the standard specification. It played a part in resolving the "price puzzle", and is now part of the standard specification.

Use of Final Revised Data

Since my paper [1986] that unwittingly resolved the "price puzzle" before its time, I have always used specifications of policy behavior that assume that the authorities react immediately to the variables they can observe without delay – commodity prices, monetary aggregates, financial variables – and only with a delay to variables that they can observe only with delay – e.g. GDP and the GDP deflator. I think it is true that specifications pretending that authorities can react instantly to such data can lead to important distortions. I am therefore somewhat sympathetic to this complaint of Rudebusch's.

On the other hand, it is not clear that this criticism is quantitatively important. Partly perhaps because it quickly leads to a need for simultaneous equations methods, few other researchers have followed my practice in this regard, and qualitative conclusions about the effects of monetary policy have not been affected by these differences in practice. If the policy authorities make efficient use flawed but immediately observable, measures of final revised data, and if the resultant measurement errors do not affect the behavior of other components of the economy, there will be no problem with specifications that assume the authorities react to final revised data. The measurement errors simply contribute welcome exogenous variation in monetary policy.[1]

Long Distributed Lags

Rudebusch takes the fact that VAR modelers find significant coefficents on variables with long lags to imply that "the Fed reacts systematically to old information". This is just a mistake. In the first place, Fed policy variables are not subject to arbitrage arguments that imply that we know that they display no inertia. But even if they were, variables that display no inertia do not necessarily show no long lags in regressions on other variables. For example, we expect stock prices are close to random walks. This means that a regression of the change in stock price on, say, industrial production, should show stock price changes depending only on the innovation in industrial production. But industrial production itself does show inertia. The best univariate predictor of it based on its own past is likely to show significant effects of lags of several months. Thus a regression of stock price on its own past and on current and past industrial production could well show significant coefficients on long lags of industrial production. This would not mean that stock prices "react systematically to old information."

Conclusion: Constructive Criticism

The Rudebusch paper does not offer much constructive criticism. It is more of an attack than a critique. So I close by offering constructive criticism of it, by trying to point it in the direction of leaving aside the more obvious and weaker points it has embraced and expanding its treatment of the points that are more penetrating. I also offer a brief summary of my own views of where the identified VAR literature is deficient and where it should be heading.

The issues of time invariance, linearity, and variable selection are universal in macroeconomic modeling. VAR modelers have inevitably thought about them already, as have their critics. To make criticism along these lines telling, the paper would have to show that a particular way of introducing nonlinearity, allowing for time variation, or changing the usual variable set, not yet considered in the literature, results in sharp changes in results. The paper does not begin this task, so its criticisms on these lines do not bite.

That financial market variables, and in particular interest rates other than the funds rate, have predictive value not captured in the usual list of VAR variables is a point worth making. It does not prove that existing VAR studies are worthless, but it suggests that there might be returns to more detailed modeling of monetary policy and money supply. The one-short-interest-rate convention may have outlived its usefulness. Rudebusch has already in this paper estimated one-sixth (one equation) of a VAR. I urge him to take the plunge and estimate a full scale dynamic model, in which policy shocks are identified and the interaction of funds and discount rate setting with private-sector based disturbances to market rates is modeled explicitly. It appears from this paper as if he would not like to think of himself as a VAR modeler. In that case I urge him to present us with another approach to dynamic modeling that can progress in the same direction.

As for my own views of what the literature lacks, they are partly implicit in a current joint project of Eric Leeper, Tao Zha and me, to distinguish Federal Reserve from private banking system behavior. This involves modeling the joint behavior of at least two monetary aggregates (one

---

[1] Ben Bernanke has articulated this point in discussions of this aspect of specification.

“controllable” like total reserves or non-borrowed reserves, one “transactions” like M1 or M2) and at least two, perhaps three or four, interest rates (from among, e.g. the funds rate, the discount rate, treasury bill rates, rates on bank deposits, bank loan rates). Such a model has to be bigger than existing identified VAR models, and hence requires some new numerical and statistical approaches, but our preliminary results are more encouraging than discouraging.

The restriction of identified VAR modeling to handling only either just-identified models or over-identified models that restrict only contemporaneous coefficients is artificial. It is time for some move in the direction of relaxing this computationally based constraint.

Ultimately the identified VAR literature and the dynamic stochastic general equilibrium model (DSGE) literature should converge. I see no satisfactory way to handle parameter drift and nonlinearity in identified dynamic models that does not require more complete behavioral interpretations than are used in the identified VAR literature. Leeper and Sims [1994] and Jinill Kim [1995] represent steps in that direction. They use models that are nonlinear in parameters and that model explicitly time variation in interpretable parameters.

Bernanke, Ben and Ilian Mihov [1995]. "Measuring Monetary Policy," presented at the CEPR/FederalRserve Bank of San Francisco Monetary Policy Conference, March 1995.

Gordon, David and Eric Leeper [1995]. "The Dynamic Impacts of Monetary Policy: An Exercise in Tentative Identification," *Journal of Political Economy 102*(6) 1228-47.

Kim, Jinill [1995]. "Effects of Monetary Policy in a Stochastic Equilibrium Model with Real and Nominal Rigidities," processed, Yale University.

Kim, Soyoung [1995]. "Does Monetary Policy Matter in the G-6 Countries? Using Common Identifying Assumptions about Monetary Policy across Countries," processed, Yale University.

Leeper, Eric and C. Sims [1994]. "Toward a Modern Macro Model Usable for Policy Analysis," (with Eric Leeper), *NBER Macroeconomics Annual*, 81-117.

Sims, C. [1993]. "A 9 Variable Probabilistic Macroeconomic Forecasting Model," in *Business Cycles*, *Indicators*, *and Forecasting*, James H. Stock and Mark W. Watson, editors, NBER Studies in Business Cycles Volume 28, 179-214.

_______ [1986]. "Are Forecasting Models Usable for Policy Analysis?", Minneapolis Federal Reserve Bank *Quarterly Review*, Winter, pp. 2-16.