

COMMENT ON SARGENT AND COGLEY'S "EVOLVING US POSTWAR INFLATION DYNAMICS"

CHRISTOPHER A. SIMS

My comments fall under three main headings:

- (i) The later, "Taylor rule", part of the paper is a structural VAR analysis. It uses non-standard, and questionable, identifying assumptions without giving us a discussion of why it differs from most of the literature or what motivates the non-standard specification. It also fails to check its specification as thoroughly as is standard in the structural VAR literature.
- (ii) The evidence that monetary policy behavior has changed sharply between early and late postwar periods, or even between interwar and postwar periods, is less strong than might appear from this paper.
- (iii) The paper sets a new, and high, standard for descriptive analysis of macroeconomic data. I hope it will be widely copied, and therefore want to be sure to register objections to certain aspects of its technical procedures before it's too late. Some of the questionable aspects of its procedures may have affected its conclusions.

1. IDENTIFICATION

There are several related facts about policy rules and their relation to the data that reflect the identification problem that must be confronted in evaluating claims to estimate a rule.

- It is easy to generate "policy shocks" that produce strong price puzzles, particularly in pre-79 data, as we see from the Barth-Ramey paper in this volume.

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Identification schemes that produce price puzzles tend also to imply large real effects of monetary policy shocks and small responses of interest rates to lagged inflation — low “activism”.

- No matter what the actual policy rule, it will be possible to estimate a regression of interest rate on “fundamentals” (i.e. not P , M or other nominal variables; intrinsic state variables) that can play the role of a statistical “interest rate equation”. Yet, in most equilibrium models, if this regression were in fact the policy rule and fiscal policy took the conventionally assumed form, the model’s equilibrium would be indeterminate.
- Observations from a gold-standard or price-level-targeting policy regime will spuriously imply a “non-activist” policy rule unless quite sophisticated simultaneity is recognized in the estimation. This follows because in such regimes high inflation predicts low future inflation, which through the Fisher equation then implies low current nominal interest rates. Such a regime can be generated by a policy reaction function that makes r respond very strongly to the price level or inflation, but the policy reaction function is not recovered by OLS regression.

In other words, there is *always* an identification problem in determining whether policy is active. The identification problem can be resolved, but only by bringing in identifying assumptions that are not testable.

One of the identifying assumptions in this paper is that the residual in a VAR ex post real interest rate equation with unemployment and CPI on the right is the policy shock, which amounts to a recursive VAR identification scheme. While much of the identified VAR literature relies on this assumption, it can lead to problematic interpretations of the data. Most prominently, price puzzles (inflationary response to monetary tightening) are a common outcome (as e.g. in the Barth-Ramey paper in this volume) when purely recursive identification schemes are applied to pre-1980 US data. As Leeper and Zha (2001) show, policy rules are estimated as stable, and without price

puzzles, when the fact that policy behavior (at least before 1980) involved responses to the money stock is allowed for and the resulting simultaneity is recognized.

The paper also presents its policy reaction function as a “real interest rate rule”. The unusual timing of the paper’s data (r is not a quarterly average, but rather a monthly average from the first month in the quarter, while the other data are quarterly averages) makes this assertion difficult to interpret. In a continuous-time, or cleanly discrete-time, model, when prices are flexible and money is neutral, the monetary authority simply cannot set the real interest rate. A policy equation with the real rate on the left, even if it has lagged inflation on the right, contradicts the mapping from the economy’s real state to its real interest rate. With non-neutralities in the model, non-existence will no longer be a logical necessity, but there will be a range of models, with weak non-neutralities, for which such policy rules raise existence problems. It seems unwise to impose a policy rule of this form on the data as an a priori restriction.

To understand this problem, consider the simple model

$$\text{(Fisher relation)} \quad r_t = E_{t-1}\pi_t + \bar{r}_t$$

$$\text{(policy rule)} \quad r_t = E_{t-1}\pi_t + \alpha\pi_{t-1} + \gamma u_{t-1} + \varepsilon_t.$$

It is easy to understand that this pair of equations leads to nonexistence of a stable rational expectations equilibrium, because taking the difference of the two equations would force innovations in the real rate to be exact functions of innovations in the policy equation. If we replaced $E_{t-1}\pi_t$ in the first equation with $E_t\pi_{t+1}$, as would be appropriate if the model’s data had conventional timing, the system would be well behaved. But of course if the data had conventional timing this specification would no longer represent policy setting the real rate. Replacing $E_{t-1}\pi_t$ in the second equation with π_t itself is no help, however, as the resulting system still has no solution. It would have been better for the paper to stick with a nominal rate rule, as does the rest of the structural VAR literature. As it is, the interpretation of all the parts of the paper that depend on this identification is problematic.

I agree with the authors that it is reasonable to assert as an identifying assumption that policy responds only to lagged information. This view could have been incorporated into their structure simply by omitting current π_t from the reaction function.

Papers in the structural VAR literature almost universally check identification by examining impulse responses, trying to ensure that the estimated system does not have unreasonable properties. It is easy for apparently reasonable identifying restrictions to lead to estimated systems that are implausible, so this type of check is important. This paper does no such checking. Thus we do not know whether the periods of implied low “activism” also are accompanied by a price puzzle, whether implied responses of monetary authorities to private shocks are reasonable, or whether the response of the economy to the policy shocks are reasonable.

Probably the majority view among macroeconomists (and especially within the Fed system?) is that monetary policy has changed drastically for the better over the last 30 or 40 years — Alan Greenspan is completely different from Arthur Burns. But the most careful statistical assessments of this idea are at best inconclusive, and for the most part suggest on the contrary that changes in the systematic component of policy in this period are modest. Examples of work that comes to this conclusion, using widely different methodologies, are papers by Orphanides (2001), Leeper and Zha (2001), Hanson (2001), and myself (1999). My own paper argues that the most important changes between periods can be accounted for as shifts in the variances of the structural disturbances. Time-varying variances are hard to distinguish from “parameter” variation. Attempts to show shifts in policy behavior should recognize this, in order to come into contact with the literature supporting the opposite view.

2. TIME VARYING DESCRIPTIVE STATISTICS

The paper implements a novel strategy to summarize the variation in the economy’s characteristics over time. It uses descriptive statistics computed from simulated future time paths drawn from the posterior predictive density at each date, displaying how

they change over time. The results are thought-provoking and deserve further study. I found particularly interesting the concentration of the posterior on the “activism” coefficient during the 70’s, followed by widening uncertainty thereafter. Even though the paper’s interpretation of its activism coefficient may be dubious, this pattern of increased, then decreased, certainty about important components of inflation dynamics is suggestive. Phenomena like this might have played a role in the inertia of policy at the time and in the subsequent popularity of Monday morning quarterbacking of it.

The paper sticks entirely to forward-looking data summaries. For many purposes this is appropriate, but such “filtered”, as opposed to “smoothed” estimates of the stochastic properties of the model contain a component of variation that is learning, rather than actual time variation in the behavior of the economy. Commonly graphs like, say, 3.11 or 3.12, show quite different time paths when computed on the basis of smoothed estimates. The difference lets us distinguish between best ex post estimates of what was actually happening and best current estimates at the time of what was happening. It would be interesting to see the work extended in that direction.

3. THE “LEARNING THE NRH” STORY

The paper’s Figure 4.1 confirms a point that Albert Ando has made for a long time: It is hard to blame the inflation of the 70’s on econometric modelers serving up a long-run inflation tradeoff. It is an important result of both the Chung thesis this paper cites and the Sargent book that the story that naive econometric Phillips curve estimation led to the inflation of the 70’s cannot be sustained.

This paper proposes a new, incompletely articulated, theory. It seems to me more a narrative than a time-invariant theory that could be tested. The theory used in the Chung thesis, in Sargent’s book, and in my (1988) paper specifies both the (incorrect) model the policy-makers use and the correct (natural rate) model relating unemployment and inflation. It works out the consequences of these assumptions. My paper and Chung’s thesis show that such a setup can easily lead to very long (at least millenia),

possibly permanent, periods of near-Ramsey behavior, with interest rates and inflation low on average. Sargent's book and Chung's paper show that this setup does poorly at explaining US postwar inflation and unemployment data, because it implies that policy authorities quickly realized the Phillips Curve is nearly vertical.

It is hard to understand why the paper gives such a prominent role to the t -test for the hypothesis " $\beta_1(1) = 1$ ". Figure 4.1 shows that the test strongly rejected the null starting in 1973. Not until more than 6 years later, in late 1979, did the "Volcker regime" begin. If the t -test showing neutrality was crucial to producing the Volcker policies, the connection was certainly not a simple one. It seems likely that the connection of this t -test to future changes in policy will be at least as tenuous.

My own view, which agrees in many respects with that of Orphanides (2001), is that unemployment rose and inflation rose because of real disturbances that lowered growth. Faced with the simultaneous rise in these two variables, and believing that unemployment affected inflation with a lag, policy-makers had to decide whether the rise in unemployment that had already occurred was enough to exert adequate deflationary pressure. Since such "stagflation" had not occurred before on such a scale, they faced a difficult inference problem, which it took them some years to unravel. Note that in this story it is not $\beta_1(1)$ that is crucial, but the relation between β_0 and $\beta_2(1)$, i.e. the Phillips curve "natural rate". I think it likely that careful statistical work using the Phillips curve would have demonstrated much earlier than 1979 that the current levels of unemployment were not exerting much downward pressure on inflation. But policy models at the time were estimating "gap" variables by focusing entirely on real factors — production functions and trend rates of growth. Policy makers realized their mistake only slowly because of excessive reliance on a theory that claimed the "gap" was a function of the level of output and the current level of technology. If they had paid more attention to a wider range of data they would have seen their mistake earlier.

The notion that monetary policy acts on the price level by first affecting unemployment, or a "gap", which then via a Phillips Curve affects inflation, is in my view

mistaken. But if it had been the basis of a flexibly parameterized dynamic econometric model analyzing inflation, interest rates and real growth jointly, it probably would not have led to such an acceleration of inflation as actually occurred.

4. PRIORS

The paper uses a prior that makes no attempt to push the parameter estimates toward the unit root boundary, centers the prior at an OLS estimate (that will tend to be more stationary than the truth when the truth is near the unit root boundary), and truncates the parameter space to rule out even mildly unstable roots. This is in the name of being “less informative” than, e.g., Doan, Litterman and Sims. It is always true that there is no unique way to produce an “uninformative” prior, and this is especially true in VAR’s. A prior like that proposed here, in a model that conditions on initial observations, implies a lot of weight on stationary models, which in turn generally imply a great deal of sample history is explained by large initial “transients”. How this happens is elaborated in some earlier work of mine (Sims, 2000). Such a prior is not uninformative, and may easily lead to strange results.

In the latter part of the paper simulations are used to give us an idea of how long it is likely to be before t -tests of $\beta_1(1) = 1$ are likely again to accept the null hypothesis. But the prior’s concentration on stable models, and the time-variation model’s insistence on making the model bounce away from the non-stationary boundary, could be strongly influencing the results of these simulations.

5. CONCLUSION

This paper breaks new ground in interpreting data with a structural VAR and time varying parameters. Many of the methodological ideas in it are new and worth pursuing. Its choices of prior and identifying assumptions, however, are deviations from standard practice in the structural VAR literature that should not, in my view, be

imitated. These aspects of the modeling and interpretation are crucial enough to the paper's substantive conclusions that those conclusions remain doubtful.

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PRINCETON UNIVERSITY

E-mail address: sims@princeton.edu