[What Ends Recessions?]: Comment

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Comment

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1. Introduction

This paper advances a startling and intriguing proposition: Active, systematic monetary policy ended postwar recessions. It is the latest in a series of provocative papers in which Christina and David Romer have revived some of the methods and views of Friedman, and Friedman and Schwartz.

In evaluating this work, I am naturally drawn to Friedman's critics. In particular, Tobin's (1970) "Post Hoc Ergo Propter Hoc" and Kareken and Solow's (1963) "Lags in Monetary Policy" outlined the issues that, formalized by Sims (1972) and others, today define the standard methodology for evaluating monetary policy. They complained about *causal* inferences from Friedman's historical analysis and regressions. They demonstrated the central *identification* problems. In particular, Tobin showed how models with no structural or *policy-invariant* effects of money on output are consistent with Friedman's evidence. They complained that Friedman refused to write down any *models* or tell us what the identifying restrictions *are*.

These issues are at least 30 years old. Like the prisoners who have told jokes so often they refer to them by number, I should be able to say "Identification," "Exogeneity," and "Invariance" to provoke knowing laughter. But after so many years, perhaps we remember the numbers but forget the jokes.

2. Identifying "Policy Actions"

Much of this paper presents a history of "policy actions" in recessions. This work has many precedents. Among others, Kareken and Solow discussed the "inside lag" of monetary policy at length. Like Romer and Romer, Kareken and Solow found that the Fed typically perceives the onset of a recession quickly. They also found that the Fed often delays a response out of fear of still high inflation.

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Romer and Romer's history basically collects statements by Federal Reserve officials about the state of the economy, and what policies the officials thought appropriate. For example, here is what Romer and Romer say about *annus horribilis* 1980:

> At every meeting of the FOMC from July 1979 through the Summer of 1980, the Federal Reserve believed that a recession was either under way or was imminent. Concern about inflation and money growth, however, prevented policymakers from moving to lower interest rates until the spring of 1980. Beginning in April 1980, just after the actual peak in real GDP in the first quarter of 1980, the combination of weak money growth and unfavorable news about real output caused the FOMC to lower the federal funds rate sharply. The FOMC did not want to "exacerbate recessionary tendencies and the economy" and was concerned about "the risk that the contraction would prove to be deeper than widely expected."

The historian in me wants to question this history, e.g., by asking how a collection of quotes culled from the FOMC minutes document statements like "the combination of weak money growth and unfavorable news about real output *caused* the FOMC to lower the federal funds rate sharply" or how this history is consistent with the last Romer and Romer (1989) *Macro Annual* paper and with the conventional wisdom that the Fed *caused* rather than reacted to events in 1979–1980.

Instead, let's take the history at face value and ask, what can we learn from it? Well, I learned that Fed officials are about as well informed about the economy as the average number-watching economist and that they seem to advocate countercyclical policy. This is useful evidence. As we will see later, whether, how fast, and based on what information the Fed reacts to output and inflation is very important for understanding the time series. VARs yield fragile estimates of the Fed's reaction function, so corroborating historical evidence is helpful.

But what does this history tell us about the *ends of recession*? It documents the Fed's attempts at *systematic* policy, actions that the Fed takes *predictably* as a function of output and inflation. It's not clear that systematic policy has any real effect at all. If it does, it's not clear why we need to look for policy *actions*. My old undergraduate ISLM textbook trumpets "automatic stabilizers" as the great success of postwar policy, precisely because they *don't* require conscious recognition or action by policymakers. Monetary, nominal GNP, or interest rate targeting rules are often advocated to work in the same way. Finally, *predictable* actions are precisely those actions whose correlations with other events have dubious causal interpretations. As Sims (1992) asks, does the cock's crow cause the sunrise? For this reason, historical and

econometric analyses search for innovations or unpredictable movements.

Thus, "policy" could have "ended recessions" with no "policy actions," and "policy actions" could have occurred without helping to "end recessions." The pure history of policy actions can tell us that the *Fed* reacted to *output* and inflation, but doesn't tell us if *output* reacted to the *Fed*. Hence, it does not teach us much about what caused the ends of recessions.

3. *Measuring the Contribution of Policy*

Since the history is inconclusive, the heart of this paper is a set of *calculations* of how much postpeak declines in real rates increased subsequent output. The crucial ingredient of these calculations are *econometric estimates* of dynamic "policy multipliers."

3.1 OLS ESTIMATES

Romer and Romer first run OLS regressions of output growth on the real federal funds rate,

$$\Delta y_t = \sum_{j=1}^{8} \beta_{yfj} ff_{t-j} + \sum_{j=1}^{8} \beta_{yyj} \Delta y_{t-j} + \epsilon_t, \tag{1}$$

and on the high-employment budget surplus. They use this equation to simulate output under different paths for the federal funds rate.

This kind of policy analysis also has a long history. Most notably, Anderson and Jordan (1968) ran similar regressions and calculated output paths under alternative policies. They obtained similar multipliers and reached similar monetarist conclusions. Their paper even has "Monetary and Fiscal *Actions*" in the title. This is known as the "St. Louis Fed" approach, in their memory. (See the discussion in Sargent's 1979 textbook, p. 287.) Anderson and Jordan used monetary aggregates rather than an estimate of the real fed funds rate,[1] and omitted lagged output, but these differences are irrelevant for what I have to say.

1. One can say both good and bad things about this choice. Here's a small sample. Good: The real federal funds rate can only change in response to a monetary tightening if money has some nonneutral effect. In the end of a hyperinflation, the real interest rate, properly measured, would not change, so no change in monetary policy would be registered. Bad: Of course we are now running one endogenous variable on another. The real funds rate is a complicated and imperfectly measured construct; it is undoubtedly determined by a complex lag of monetary policy and real events; and, thus, it is dubiously under the Fed's control. See Romer and Romer's plot 1: It is sometimes measured at *minus* 4%, which seems unlikely.

Kareken and Solow (1963) already criticized this method:

> Imagine an economy buffeted by all kinds of cyclical forces,... Suppose by heroic... variation in the money supply... the Federal Reserve manages deftly to counter all disturbing impulses and to stabilize the level of economic activity absolutely. Then, an observer... would see peaks and troughs in monetary change accompanied by a steady level of aggregate activity. He would presumably conclude that monetary policy has no effects at all, which would be precisely the opposite of the truth.

We tend to make this kind of point quantitatively today, by constructing models and seeing what aspects of those models are recovered by our empirical procedures.

3.1.1 Contemporaneous Shock Identification Suppose output is affected by the monetary policy variable m (a monetary aggregate, the real or nominal federal funds rate, or other indicator of policy) and other serially correlated disturbances, so

$$y_t = \sum_{j=0}^{\infty} a_{ymj} m_{t-j} + \sum_{j=1}^{\infty} a_{yyj} y_{t-j} + \epsilon_t. \tag{2}$$

Suppose the Fed reacts to output, as the Kareken and Solow's and Romer and Romer's historical evidence suggests,

$$m_t = \sum_{j=1}^{\infty} a_{mmj} m_{t-j} + \sum_{j=0}^{\infty} a_{myj} y_{t-j} + \delta_t. \tag{3}$$

Now we can solve Equation (3) for output, yielding

$$y_t = \frac{1}{a_{my0}} m_t - \sum_{j=1}^{\infty} \frac{a_{mmj}}{a_{my0}} m_{t-j} - \sum_{j=1}^{\infty} \frac{a_{myj}}{a_{my0}} y_{t-j} - \frac{1}{a_{my0}} \delta_t. \tag{4}$$

Note that Equations (2) and (4) have exactly the same list of right-hand variables! Will OLS recover Equation (2), the effect of money on output, or Equation (4), the Fed feedback rule? Well, OLS sets the residual orthogonal to the right-hand variables. But since contemporaneous m appears in the y equation and vice versa, *neither* ϵ nor δ is orthogonal to the right-hand variables.

This is a classic simultaneous equations system. To recover estimates of the structural parameters, we need an *identifying assumption*. Romer and Romer's assumption is that contemporaneous m does not affect y,

$a_{ym0} = 0$. Given this additional assumption, OLS *does* recover the structural Equation (1). But consider what happens if their identifying assumption is wrong. What if m *can* affect y within the quarter? Then, OLS recovers *mongrels*, combinations of the structural output effects and the Fed feedback rule. For example, if $\sigma_\delta^2 = 0$, then OLS estimates recover the feedback rule and have nothing to do with the effects of m on y!

Romer and Romer treat these as minor issues and note the results are similar if contemporaneous m is included. But the issue is *central* in deciding what OLS has recovered in either case. If both m and y can affect each other within the quarter, regressions with and without current m will both be mongrels. And if the estimated multipliers look a lot like one's priors about the effects of m on y, they also look a lot like my priors about the negative of the Fed feedback rule!

Romer and Romer have in fact estimated the first row of a bivariate vector autoregression, assuming a recursive orthogonalization of the contemporaneous error covariance matrix with output first. This identification issue bedevils the VAR literature, and so much thought has gone into it. Most VARs using monetary aggregates make the opposite assumption—they presume that the Fed cannot see and act quickly enough to make m respond to y within the quarter rather than the other way around. Romer and Romer are primed to contribute constructively to this debate; their historical analysis can tell us a lot about whether the identification stories used in the VAR literature hold water. They should do so.

3.1.2 Omitted Variables Suppose now that other variables z_t are helpful in forecasting output and that the Fed watches them as well. To make matters simple, ignore contemporaneous correlation:

$$y_t = \sum_{j=1}^{\infty} a_{ymj} m_{t-j} + \sum_{j=1}^{\infty} a_{yyj} y_{t-j} + \sum_{j=1}^{\infty} a_{yzj} z_{t-j} + \eta_t \tag{5}$$

$$m_t = \sum_{j=1}^{\infty} a_{mmj} m_{t-j} + \sum_{j=1}^{\infty} a_{myj} y_{t-j} + \sum_{j=1}^{\infty} a_{mzj} z_{t-j} + v_t. \tag{6}$$

What does this system predict for the projection of y on lagged y and m, Equation (1)? The error term is $\epsilon_t = \sum_{j=1}^{\infty} a_{yzj} z_{t-j} + \eta_t$. By virtue of Equation (6), lags of m are correlated with this error term, so Equation (1) yields inconsistent estimates of the structural effects a_{ym} and a_{yy} in Equation (5).

Romer and Romer acknowledge but belittle the possibility of "downward bias" in the estimate of a_{ym}. However, the problem is really *identification*. The estimated lag polynomials in a regression of y on m are *mongrels*, combinations of all the lag polynomials in the system. One can obtain Romer and Romer's regressions from systems in which money has *no* effect on output; modern versions of Tobin's "Post Hoc Ergo Propter Hoc" example will deliver this result. Thus, this "bias" can be upward or downward or both (at different lags).

Are omitted variables quantitatively important? The question is simply whether one can improve output forecasts by using variables beyond lags of output growth and federal funds changes, and whether the Fed watches more than these variables in setting monetary policy. Again, the VAR literature has faced this problem. Many other variables *do* significantly help to predict output growth and the path of monetary policy variables. "Level" variables, including the consumption/output ratio, the term spread, and the default spread are prime examples (see Cochrane, 1994b). And analysis of the Fed's operating procedures and history, by Romer and Romer and others, convinces one that the Fed obsessively watches an enormous number of economic variables when setting policy. Thus, this is not an in-principle argument: A few easy regressions, the lessons of a large and well-known literature, and Romer and Romer's own historical analysis are convincing that left-out variables are a serious problem in Equation (1).

Finally, channels for mongrel coefficients beyond Fed feedback may be even more important. For example, lower output leads to lower money demand and, hence, lower interest rates. The standard real business cycle model predicts a dynamic relation between low output and low (real) interest rates in response to a low technology shock.

3.2 INSTRUMENTAL VARIABLES ESTIMATES AND ROMER-ROMER DATES

To mitigate the previous problems with Equation (1), Romer and Romer estimate it using the Romer–Romer (1989) dates and the Boschen–Mills index as instruments. The results are quite similar to the OLS results.

But the Boschen–Mills index is just another measure of the stance of monetary policy, so there is no reason it should be less correlated with the error term than the federal funds rate.

The Romer–Romer index is "a dummy variable equal to one on dates of apparent shifts by the Federal Reserve to policies designed to reduce inflation...." But it is hard to believe that the Fed ignores output in making such a decision. Romer and Romer's reading of the FOMC minutes is pretty persuasive to the contrary!

This is an important issue, and I wish Romer and Romer were clearer about what their dates mean. As I read it, they believe that the Fed follows feedback rules, which I can simplify for the purposes of this discussion to something like

$$m_t = a_i(L)y_t + b_i(L)\pi_t, \cdot\ i = \text{"growth" or "inflation."}$$

Sometimes, the Fed is more concerned with fighting recessions or maintaining output growth. This is more than just a time in which inflation is low, so that the contribution of the $b(L)\pi_t$ term is low; this is a regime in which $b_i(L)$ itself is small or zero, so that even high inflation would not spur the Fed into action. At other times, the Fed is more concerned with reducing inflation. Again, this is more than just a time in which output is high so that an $a(L)y_t$ term is small; it is a regime in which the $a_i(L)$ coefficients are small or zero. A Romer–Romer *date*, then, is a time in which the Fed *switched* from the large $a_i(L)$ to the large $b_i(L)$ regime.

Here is the fundamental problem. To use these dates as instruments, it does not matter whether the new *regime* places no emphasis on output—whether $a_i(L) = 0$ in the new regime—it matters whether the *change* in regime is made without regard to the current state of output, anticipated future output, or other variables correlated with output. *This* is what I find hard to believe. No disinflation event came in the depth of a depression! It is the crucial piece of evidence and it is not addressed by Romer and Romer's historical analysis.

3.3 IDENTIFICATION OF POLICY-INVARIANTS AND AN APPEAL FOR THEORY

Even if the regression is impeccably specified, a fundamental *identification* ambiguity remains and requires us to spell out our monetary *model* or compare data from different *regimes*.

Kareken and Solow knew of the problem: "...One cannot deduce conclusions about the effects of monetary policy or about their timing without making some hypothesis, explicit or implicit, about what the course of events would have been had the monetary authorities acted differently." Tobin showed us how a model in which money is totally passive can account for Friedman's reduced-form evidence. Sargent (1976) formalized the point more recently.

Here is a simple example. Suppose the structural relation between a monetary policy variable m_t and output y_t is given by

$$y_t = a_{yu}(L)(m_t - E_{t-1}m_t) + a_{ym}(L)m_t + a_{y\epsilon}(L)\epsilon_t,$$

and the feedback rule is given by

$$m_t = a_{my}(L)y_t + a_{m\delta}(L)\delta_t.$$

(To keep the algebra simple and to emphasize that orthogonalization is not the issue, suppose that $a_{my}(0) = 0$, $a_{y\epsilon}(0) = a_{m\delta}(0) = 1.$)

This model nests two interesting special cases: (1) If $a_{ym}(L) = 0$, then only unanticipated money affects output. The path of output is *completely* unaffected by the Fed's policy rule, $a_{my}(L)$ and $a_{m\delta}(L)$; alternative postpeak paths for the funds rate have *no* effects on output; and the *moving-average* representation (impulse-response function) is policy-invariant. (2) If $a_{yu}(L) = 0$, then there is no distinction between anticipated and unanticipated money; different feedback rules *can* stabilize or destabilize output; and the *autoregressive* representation is policy-invariant.

Unfortunately, the Appendix proves the following proposition: $a_{yu}(L)$ *and* $a_{ym}(L)$ *are not separately identified.*

No regression can distinguish whether the true "policy multiplier" is that estimated by Romer and Romer or *zero*. We *must*, impose some theory or "identifying restriction" to get an answer.

Romer and Romer implicitly assume that there is *no* distinction between anticipated and unanticipated money: $a_{yu}(L) = 0$. In this case (and with the orthogonalization assumption, and the absence of other variables) the regression of y on lagged m and y does yield the structural effects of money, $a_{ym}(L)$ (see the Appendix).

Is this assumption sensible? Many economists do seem to believe that anticipated or systematic policy can have real effects. However, some monetary policies have no effects: the ends of hyperinflations, currency revaluations, and policies in countries with high and variable inflation. Thus, we need a view of money that explains why monetary policy *does* have effects in some circumstances and *does not* in others. Most monetary models that can explain both sets of observations give no role to systematic policy (beyond inflation-tax effects). And there are few clean experiments to help us, aside from reforms and hyperinflations. The year 1979 is often trumpeted as an announced deflation, but consumers had been subjected to many announcements; only if everyone *believed* the announcement does it count. One needs to document the state of people's expectations, not the muddy, contradictory, and wolf-crying statements of Fed officials. For these reasons, I venture that few of us would go so far as to assume that there is *no* distinction between anticipated and unanticipated monetary policy.

I don't want to rehash the old arguments over anticipated versus unanticipated monetary policy. Perhaps Romer and Romer *do* want to assume there is no distinction. The point is that the assumption identifying what is policy-invariant is *crucial*, so it needs to be explicit and linked to a monetary *theory* that can explain the wide variety of correlations between real and monetary variables that we observe. Do Romer and Romer want everyone who does not immediately buy their identifying assumption to dismiss their paper? Then they must *argue* for it.

Even in reading history, the example shows how we need to carry along some other variable, be it the way agents form expectations, the average duration of nominal contracts, or the costs of printing new menus, that differentiates the United States in 1979 from Germany in 1921 or Brazil in 1994. And we need a monetary *theory* (or even a *view* or a *story*) to tell us what that state variable is.

3.4 DO THE MULTIPLIERS MAKE SENSE?

Finally, look at the multipliers in the Romer's Figure 3. Can these be the structural effects of monetary policy?

The responses are *permanent* and *delayed*. No story for the effect of money on output that I know of produces such responses. If monetary policy does indeed have the plotted effects, we have *absolutely no idea* how it can do so!

The responses are *big*. A one percent decline in real interest rates causes up to a 3% rise in output. If one thinks like a Keynesian for a minute, monetary policy is alleged to affect output through its effect on investment. Since investment is about 10% of output, these estimates require a 30% rise in investment for each percentage point decline in interest rates! Even the 4–5% rise in investment required if one takes an expansive view, including housing and durables, is much larger than the investment literature suggests.

The VAR literature has a lot of experience with the federal funds–output system estimated by Romer and Romer. (Cochrane [1994b] presents a summary.) Two variable VARs yield large, permanent, and delayed impulse responses, much like Romer and Romer's multipliers. Fed funds shocks account for 50% and more of output variance. However, they also yield a "price puzzle"—prices *rise* following a tightening. This has been ascribed to the fact that the Fed also tightens when it gets news of future inflation. When more variables are added to the VAR, in particular commodity prices to control for the Fed's information about future inflation, the price puzzle disappears, but much

smaller and more *transitory* effects of a federal funds shock emerge. Federal funds shocks then account for 10% or less of output variation.

Finally, I don't think Romer and Romer take the multipliers that seriously. Why stop at constant rates versus the historical postpeak path? Why not set the real rate at minus 4% and *permanently* raise output by 20%? The real funds rate plot, the presumption that it is under the Fed's control, and the multipliers say this is possible! Well, obviously, there are constraints on what the Fed can do; perhaps such expansionary policy might eventually lose its effect on output and raise prices; perhaps the real interest rate really isn't under the Fed's control, i.e., maybe we don't really believe the multipliers.

In fact, Romer and Romer *tell* us not to take many aspects of their calculations seriously, such as the fact that the level of output is always higher under the constant interest rate rule. Well why not? If the method gives a bad estimate of the two-year response, why does it give a good estimate of the one-year response? I don't think you can have it both ways. Either this *is* or it *is not* the menu of options available to the Fed.

4. *Do Recessions Need "Ending?"*

The very title of this paper presupposes that "recessions" need "ending." Most of macroeconomics presumes that the economy reverts following a shock all by itself. For this reason, we usually focus on the shocks that *start* recessions and their propagation mechanisms, but almost never, until now, on policies and shocks that *end* recessions. In order to believe that policy actions "ended recessions," we need solid evidence that postwar recessions ended more quickly than a maintained economic model predicts. This requires an explicit statement of what the model is, and a little data analysis.

In the early 1960s, many macroeconomists thought about the world through a static ISLM model, in which "insufficient aggregate demand" could, in fact, persist indefinitely without policy action.[2] However, by the time Romer, Romer, and I were undergraduates, standard textbooks (Dornbush and Fischer) had taken the natural rate part of Friedman's 1968 address to heart, and added ad hoc dynamics by which the economy would revert to full employment.

Standard stochastic growth models in use today derive their dynamics endogenously and so make quantitative predictions about the speed with which the economy reverts following a shock. The standard model

2. The dynamics of Samuelson's multiplier-accelerator notwithstanding.

with a typical calibration[3] predicts a half-life of 9.1 quarters following a shock. This prediction is tied to parameters of the model, labor's share and depreciation in particular. If one allows for a higher than usual depreciation of 20% per year, the standard model predicts a 5.8 quarter half-life. One can, of course, advocate other models or parameterizations; one *has to* in order to think that recessions need "ending." But at least one standard model predicts that recessions end themselves, so I am not foolish in this presumption.

The *data* are also consistent with the view that recessions end themselves. The simplest example is just based on a nondurable and services consumption/private output ratio autoregression,

$$\ln \frac{C_t}{Y_t} = -0.04 + \underset{(0.036)}{0.872} \ln \frac{C_{t-1}}{Y_{t-1}} + \epsilon_t.$$

The half-life implied by the AR(1) coefficient is 5.07 quarters. More complex evidence from the VAR and forecasting literature yields similar results: Movements in output that are not matched by movements in consumption are expected to die off quickly (e.g., see Cochrane, 1994a).

Figure 1 graphs consumption and output through four recessions. The same message is apparent: Consumers *expected* the recessions to end promptly, which is why consumption is barely affected by the declines in output.

In the face of Figure 1, the only hope for the Romer–Romer story is that consumers expect recessions to end swiftly because they correctly anticipate that the Fed will step in and end them. However, to believe this, one must again believe that completely anticipated, systematic policy can have real effects: One must explain how consumers anticipate the monetary injection and its output effects, but how consumers and producers do not anticipate, expect, demand, or set higher prices.

Finally, this graph and the associated VAR evidence also shows that the "persistence" of recessions that Romer and Romer seek to explain

3. The model is

$$\max E \sum \beta^t \left(\ln(C_t) + \frac{(1 - N_t)^{1-\gamma}}{1 - \gamma} \right) s.t.$$

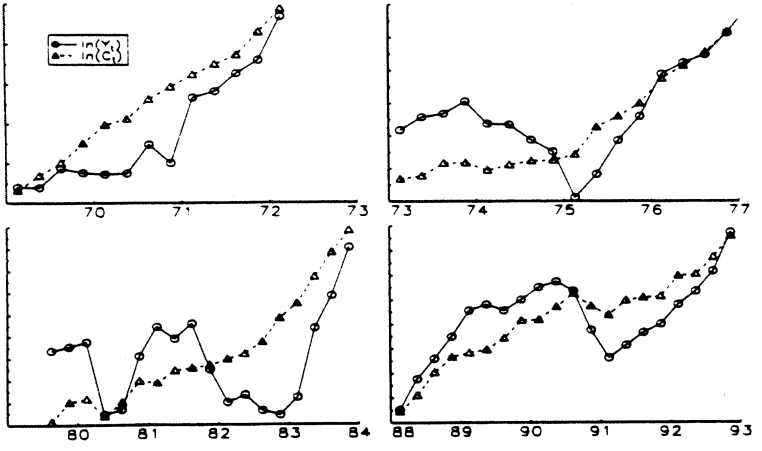
$$Y_t = (A_t N_t)^{\alpha} K_t^{1-\alpha} = C_t + I_t$$

$$K_{t+1} = (1 - \delta) K_t + I_t.$$

I calibrate to a steady-state return on capital of 6% per year, growth 2% per year, $\alpha = 2/3$, $\delta = 10\%$ per year, $\frac{2}{3}$ of a day steady-state leisure, and $\gamma = 1$.

Figure 1 LOG OUTPUT AND LOG CONSUMPTION IN FOUR RECESSIONS.

Output = GDP − government purchases. Consumption = nondurables + services. Log consumption is shifted up by mean ln(*Y*) − ln(*C*).



by persistent policy isn't there. It couldn't be. If it was, recessions wouldn't have "ended"! Also, Christina Romer's earlier work convinced me that business cycles ended just as fast in the United States before the Fed was there to step on the gas at the trough, and business cycles end just as fast in other countries with less lead-footed Feds than ours.

5. *Conclusions*

Here are some of the fundamental questions of macroeconomics:

- Can changes in the quantity of money or a swap of debt for lump-sum taxes affect output? If so, *how* and in *what circumstances*?
- Can *systematic* policy offset other shocks? If so, why are open-market operations different from currency reforms or the ends of hyperinflations?
- *Have* attempts at countercyclical policy in the postwar United States stabilized output? Or have ham-handed attempts at discretionary policy actually destabilized output?

To address these questions, the last 30 years have seen an outpouring of empirical work on the effects of monetary policy. In response to Tobin and Solow's concerns, a standard methodology has emerged.

One adds other variables to the output equation; one adds other equations to control for Fed reaction, effects of output on interest rates and so forth. One can find exogenous stochastic processes in the error terms, and plot responses. This is a VAR, of course. A small taste of this literature, selected because the references happen to be on my hard disk, includes Bernanke and Blinder (1988), Christiano and Eichenbaum (1991), Cochrane (1994b), Gordon and Leeper (1993), King and Watson (1992), Sims (1992), and Strongin (1992). This literature is making some progress: Many different identification schemes are converging on similar answers, which are, as I mentioned, quite different from Romer and Romer's multipliers.

The last 30 years have also seen an outpouring of theoretical work on monetary economics, including the development of rational-expectations, cash-in-advance, overlapping generations, and sticky-price and limited-participation theories of money and its potential nonneutralities. Public finance has produced a similarly enormous body of work evaluating the potential for a fiscal nonneutrality. This material is the heart of macroeconomic training in every Ph.D. program and standard textbooks.

Finally, a generation of monetary economists following Friedman, including Kareken and Solow, Poole, McCallum, Meltzer, and many others, has explored the lags of monetary policy, how the Fed makes decisions, and what variables are under its control.

Romer and Romer completely ignore all of this literature. There is not a mumble of an apology in the direction of Tobin and Solow's methodological concerns, much less their formal statements by Sims and others. Despite its fundamental importance for identification, there is not a hint of a reference to monetary theory, even David Romer's thesis or the collection of papers in his book with Greg Mankiw (1991). The empirical findings of the huge VAR literature go unmentioned (with one lonely exception). The paper reads as if Romer and Romer are the first to ever examine recognition, decision, and action lags at the Federal Reserve. The underlying economics, like the empirical methods, is straight from the 1960s: The paper does not ask whether the economy returns to a natural rate without policy intervention; the 1970s challenge that systematic policy might have no real effects is not even dismissed, to say nothing of the 1980s challenge from stochastic growth models that not even the beginnings of recessions need policy shocks.

The omission is so glaring it must be intentional. Here is my—quite sympathetic—interpretation. The last 30 years of macroeconomics are difficult, and the period hasn't provided firm answers to the earlier questions. VARs address Tobin and Solow's criticisms, but lots of prob-

lems remain. One has to identify shocks from the residuals, consider the potential effects of omitted variables, and worry about whether the AR representation, MA representation, or some combination is policy-invariant. Identification isn't easy. The empirical results are sensitive to specification; the standard errors are big, and one ends up with the impression that the data really don't say much about the effects of monetary policy—which may in fact be true. Theoretical models seem equally sensitive to assumptions and do not connect easily with empirical work.

We've been at this over *30 years*, and look how little progress we have made toward answering such simple questions! Can understanding monetary policy really be so difficult? Why don't we just throw all the formal methodology overboard and go read the history of obvious episodes and *see what happened*? If, like me, you have struggled with even the smallest VAR, this approach is enormously attractive.

Perhaps this is Romer and Romer's motivation. But if so, I think that Romer and Romer are falling into the same trap that ensnared the rest of us. Perhaps they started with a desire to just look at the facts. But then they wanted to make *quantitative* statements. How *much* would output have changed if the Fed followed a different policy? To do so, they reinvented the St. Louis Fed approach—an econometric technique. Despite the desire to "do something simple" (David Romer, during the discussion), they in fact evaluated policy from the autoregressive representation of an output—fed funds VAR. Now they face Tobin and Solow's classic causal and identification problems, which cannot be addressed by quotes from FOMC meetings.

Adam and Eve in the garden of Friedman, they have taken one bite of the forbidden econometric fruit. But the serpent (me) is still there, whispering "go ahead, just add a few more variables;" "you can fix that, just put in a Fed reaction function;" "Why don't you write down a few structural models and verify what your regressions are picking up?" I don't see how they can resist taking bite after bite, until they are cast out of the garden, explicitly running VARs, and working hard for identification with the rest of us.

I don't mean to disparage history. Perhaps we can read history with Solow and Tobin's criticisms in mind and try to address them with historical analysis. Historical analysis should be able to help us figure out *how* monetary policy has nonneutral effects. History contains many different *regimes*; by finding relations between money and output that are invariant across these regimes, we can help identify which relations are invariant to different *policies*. For example, Sargent's (1986) analysis of the ends of hyperinflations brings home the potential neutrality of

some large monetary events, the government's intertemporal budget constraint, and the fact that inflation is often and in many places a *fiscal* phenomenon, in a way that mountains of formal papers do not. Finally, and most importantly, Romer and Romer's analysis of FOMC minutes may be very helpful in sorting out how the *Fed* reacts to the *economy*.

But a successful reading of history can't *ignore* Tobin and Solow's concerns, and a fundamentally econometric paper like this one can do so even less. VAR methods did not evolve as recreational mathematics. They evolved as the best response a generation of talented economists could come up with to genuine and serious concerns, expressed 30 years ago by Tobin and Solow, with the Friedman and Schwartz methodology that Romer and Romer are attempting to revive. I hope that Romer and Romer can find a way to address these concerns with careful historical analysis rather than reinventing the VAR wheel. But if economic history simply ignores the history of economics, it is doomed to repeat it.

Appendix: Identifying Policy-Invariants

The structural system is

$$y_t = a_{yu}(L)\delta_t + a_{ym}(L)m_t + a_{y\epsilon}(L)\epsilon_t$$

$$m_t = a_{my}(L)y_t + a_{m\delta}(L)\delta_t.$$

In addition to the assumptions mentioned in the text, I assume that all the structural lag polynomials are invertible.

Deleting the (L) to simplify notation, the moving average representation is

$$\begin{bmatrix} y_t \\ m_t \end{bmatrix} = \begin{bmatrix} \dfrac{a_{y\epsilon}}{1 - a_{my}a_{ym}} & \dfrac{a_{yu} + a_{ym}a_{m\delta}}{1 - a_{my}a_{ym}} \\ \dfrac{a_{my}a_{y\epsilon}}{1 - a_{my}a_{ym}} & \dfrac{a_{yu}a_{my} + a_{m\delta}}{1 - a_{my}a_{ym}} \end{bmatrix} \begin{bmatrix} \epsilon_t \\ \delta_t \end{bmatrix},$$

which we can compare to that of an unrestricted VAR,

$$\begin{bmatrix} y_t \\ m_t \end{bmatrix} = \begin{bmatrix} \theta_{y\epsilon} & \theta_{y\delta}VV \\ \theta_{m\epsilon} & \theta_{m\delta}W4W \end{bmatrix} \begin{bmatrix} \epsilon_t \\ \delta_t \end{bmatrix}.$$

If $a_{ym} = 0$, then

$$a_{yu} = \theta_{y\delta}.$$

If only unanticipated money matters the impulse response-function recovers the structural response of output to money innovations. This is the usual assumption.

The autoregressive representation is

$$\begin{bmatrix} \frac{1}{a_{y\epsilon}}\left(1 + \frac{a_{yu}a_{my}}{a_{m\delta}}\right) & -\frac{1}{a_{y\epsilon}}\left(a_{ym} + \frac{a_{yu}}{a_{m\delta}}\right) \\ -\frac{a_{my}}{a_{m\delta}} & \frac{1}{a_{m\delta}} \end{bmatrix} \begin{bmatrix} y_t \\ m_t \end{bmatrix} = \begin{bmatrix} \epsilon_t \\ \delta_t \end{bmatrix}$$

Compare the autoregressive representation to an arbitrary VAR

$$\begin{bmatrix} \beta_{yy} & \beta_{ym} \\ \beta_{my} & \beta_{mm} \end{bmatrix} \begin{bmatrix} y_t \\ m_t \end{bmatrix} = \begin{bmatrix} \epsilon_t \\ \delta_t \end{bmatrix}.$$

The second row identifies the money reaction function parameters a_{my} and $a_{m\delta}$. The first row implies

$$\frac{1}{a_{y\epsilon}}\left(1 + a_{yu}\frac{a_{my}}{a_{m\delta}}\right) = \beta_{yy}; \quad -\frac{1}{a_{y\epsilon}}\left(a_{ym} + a_{yu}\frac{1}{a_{m\delta}}\right) = \beta_{ym}.$$

We can eliminate $a_{y\epsilon}$ by dividing the two equations, but then we have one equation in the two unknowns a_{ym}, and a_{yu}. This proves the proposition in the text. a_{ym} and a_{yu} are not separately identified.

If $a_{yu} = 0$, there is *no* distinction between anticpated and unanticipated money, and Romer and Romer's multiplier recovers the structural effects of m on y,

$$a_{ym} = -\beta_{yy}^{-1}\beta_{ym}.$$

Under this identification assumption, the *autoregressive* representation is policy invariant.

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Comment

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This paper is broad in scope and well written. The discussion of the episodes is a good, quick review of the events, and it is excellent