Reactions to the Berkeley story

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Abstract

"You can get your information about the economy from admittedly fallible statistical relationships, or you can ask our uncle. I, for one, have never hesitated over this choice. But I fear there may be altogether too much uncle-asking in government circles in general, and in central banking circles in particular." *Central Banking in Theory and Practice*, Alan Blinder, p. 9.

1 A new rendition of the "Berkeley story"

The topic of this session is 'Changing Views about Stabilization Policy: a Historical Perspective.' The Romers contribute to this topic by narrowing it. Mostly they use a narrative approach to buttress, refine, and extend the "Berkeley story" about post World War II U.S. monetary policy.[1] The Berkeley story is that the monetary policy authorities knew an approximately correct model of the macroeconomy in the 1950s, forgot it in the late 60s and early 70s, made bad policy as a result, then relearned the correct model in the 80s and thereupon improved policy. The Romers say that by 1970 the Fed accepted the natural rate hypothesis[2] and had appropriately modified its preferences by aiming to sustain unemployment at its best estimate of the natural rate of unemployment.[3] But the natural rate is at

[1] I call it the Berkeley view because it was articulated by Brad DeLong (1997) and investigated statistically by Sargent (1999), a 1960s Berkeley undergraduate.

[2] A personal recollection: the natural rate hypothesis was certainly not widely accepted among academics in the early 1970s. Debates about the topic with my colleagues at Penn and Minnesota were heated, with the older policy types in the department doubting the hypothesis well into the decade.

[3] Thus, the Romers' interpretation is that from the 1970s on, the Fed had preferences of the type described by Blinder (1997, chapter 1), which penalize deviations of unemployment from the natural rate. Blinder uses this preference specification, rather than Kydland and Prescott's, to prevent time consistency from polluting the central bank's preferences. Perhaps, implicitly, this preference specification is why the Romers' story omits credibility issues. But it would be difficult to square Blinder's argument with the Romers' theme that the Fed's preferences did not change over the entire post WWII period, because they say that the Fed did not know the natural rate theory before the late 1960s.

best a slowly moving hidden variable obscured in noise. Adopting a theme of Athanasios Orphanides (2003, 2002), the Romers attribute the Fed's policy mistakes of the 1970s to its inaccurate estimates of the natural unemployment rate (or potential GDP). The Romers say that better estimates in the 80s and 90s facilitated better policy.

The Romers put changing ideas about the exploitability of the Phillips curve front and center. They assign Samuelson and Solow's 1960 paper an important role in creating the intellectual foundations for the policy mistakes that led to America's biggest peace time inflation:

> In the early 1960s, policy makers adopted the Samuelson–Solow (1960) view that held that very low unemployment was an attainable long–run goal and suggested that there was a permanent tradeoff between inflation and unemployment. (page 2)

The Romers' story is all about how policy makers temporarily went astray by forsaking a good model of the Phillips curve for a worse one, but eventually returned to the correct view:

> ... perhaps the most interesting characteristic of this evolution of beliefs is that core beliefs ended the century at much the same point that they began the postwar era. (page 3)

2 Omitted ideas

Interesting as the Romers' account is, it is particularly striking for how it neglects some of what I think were the most important and useful ideas that macroeconomists contributed to policy debates since WWII, including but not limited to these salient ones:[4] (1) rational expectations, (2) commitment and time consistency problems, (3) reputation as a substitute for commitment, (4) the disturbing multiplicity of reputational equilibria, (5) the development of systematic evidence that shock distributions widened then narrowed over the postwar period, (6) the subtle difficulties in empirically distinguishing time-invariant models from models with coefficient drift, especially with respect to low-frequency movements, (7) uncertainty about model specification. The absence of these ideas from the Romers' account contrasts with the analysis of the art of central banking by former Vice-Chairman Alan Blinder (1997). Blinder's book draws the reader into considered arguments about aspects of almost all of these issues and how they inform or constrain monetary policy decisions.

I will briefly take up these ideas that are missing from the Romers' narration, and then make some comments about the advantages and disadvantages of a narrative approach to evaluating policy.

[4]If space were less limited, I would add these too: (8) monetarism, the discussion of suitable monetary instruments and indicators, and the arguments for and against a k-percent rule, (9) the specification of wage-price dynamics in the literature started by Phelps and Taylor and Fischer that aimed to respond to the 'policy ineffectiveness' counterexample.

2.1 Rational expectations

Friedman's and Phelps's natural rate theory is incomplete without a theory about expectations. Friedman and Phelps both assumed adaptive expectations, an assumption that left open the possibility that there is considerable scope to exploit a Phillips curve. The natural rate hypothesis acquired its full power in limiting the scope of feasible counter policy only when Robert E. Lucas coupled it with the assumption of rational expectations.[5]

2.2 Time inconsistency and commitment

The assumption of rational expectations led to Kydland and Prescott's analysis of a time consistency problem that occurs when the natural rate hypothesis is true. They showed that when the public has rational expectations, a benevolent and fully informed monetary policy authority will choose a suboptimal policy if it cannot tie the hands of its successors. Choosing sequentially (i.e., i.e., holding Fed meetings recurrently and deciding meeting-by-meeting) worsens outcomes relative to what can be achieved if the Fed could precommit once-and-for-all to a plan. The problem is not that the Fed has the wrong model or the wrong objectives but that it has to choose sequentially.

2.3 Reputation

The time consistent equilibrium of Kydland and Prescott restricts the strategy of the monetary authority not to depend on histories of actions and outcomes. This rules out reputational effects that can operate when strategies depend on histories. In response to the time consistency problem, macroeconomists adapted ideas from game theory, and showed that it is possible for the Fed's time consistency problem to be overcome if the public comes to have a system of expectations about the Fed's actions, a Fed reputation, that the Fed wants to confirm, because failure to do so would cause the Fed to acquire a reputation associated with worse outcomes.[6] The theory carries some good news and some bad news. The good news is that self-sustaining reputations exist that give rise to optimal outcomes and thus solve the time consistency problem. The bad news is that the theory contains self-sustaining bad reputations, some of which actually give worse than time consistent outcomes. In these bad self-sustaining equilibria, the Fed has incentives to confirm expectations that it will choose bad policy. In these bad equilibria, the Fed is caught in what Chari, Christiano, and Eichenbaum (1998) call an 'expectations trap'. These three authors have strung together compelling quotations from Arthur Burns that convince them that Chairman Burns understood the natural rate hypothesis but thought of himself as caught in an expectations trap.

[5]For example, even with staggered wage setting and other forms of price stickiness, under rational expectations there exists a sequence of foreseen monetary policy actions that will lead to a cost-less stabilization of an initial on-going inflation. Under adaptive expectations, there does not.

[6]Key work was by Barro and Gordon, Chari and Kehoe, and Stokey.

2.4 Coefficient drift and changing distributions of shocks

Lucas's 1976 critique of econometric policy evaluation procedures adduced drift in macroeconometric specifications as evidence of misspecification, especially ways of modelling the public's expectations formation. After Lucas, an important split has developed about whether macroeconometric relations have actually drifted and continued to drift. Some important researchers[7] offer evidence for the hypothesis that although the variances of shocks have evolved over time, the coefficients in VARs and monetary policy decision rules seem to have been stable over the post WWII period.[8] Their evidence buttresses the view that it wasn't Fed behavior that changed between the late 60s–70s period and the Volcker-Greenspan era, but rather the distribution of shocks. The evidence says that shock variances for inflation increased markedly during the Burns period, then came back down.[9] (Figure 1 contains estimates of the drifting shock innovation variances and correlations for a three variable VAR for the unemployment rate, a short-term nominal interest rate, and CPI inflation. The source is Cogley and Sargent (2002).) However, other researchers who fit alternative models have provided evidence that the coefficients of VARSs and monetary policy decision rules have also drifted over the post WWII period.[10] I would summarize the current state of the debate between the no-drift in VARs versus the drift in VARs school as follows: while there is convincing evidence about the post WWII drift in shock variances, the evidence for or against coefficient drift is more tenuous and controversial because of the low statistical power that most tests of time invariance have against the kinds of drift that seems to be in post WWII VARs. To me this empirical literature seems very relevant to the questions about the Fed's learning, forgetting, and changing behavior that are taken up by the Romers.

[7] See Sims (1980, 1999) and Bernanke and Mihov (1998a, 1998b).

[8] Sims (1980) suggests that the time invariance of VARs extends back to before WWII.

[9] This evidence is pertinent for drawing inferences from the Romers' evidence about forecasting errors. I would have introduced this evidence at the point in the argument where the Romers say

> A move to a more realistic model, all else equal, is likely to lead to smaller forecast errors. (page 24)

The empirical evidence about drifting volatilities suggests one of the important things that should qualify the 'all else equal' clause.

[10] See Clarida, Gertler, and Gali (2000) and Cogley and Sargent (2001, 2002).

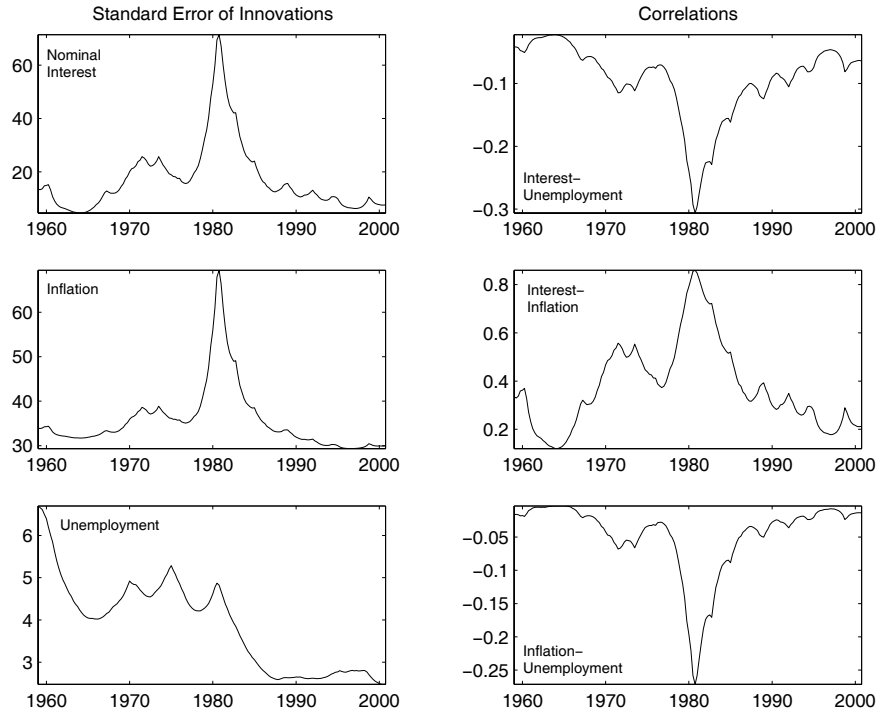

Figure 1: Drifting innovation variances and correlations.

2.5 Drifting persistence of inflation and inference about the natural unemployment rate hypothesis

Advocates of the drift-in-coefficients view have sought and found evidence that the *persistence* of inflation has drifted during the post WWII period, with inflation not having much persistence during the Bretton-Woods subperiod, acquiring considerable persistence during the 70s and early 80s, then becoming less persistent recently. This drift in persistence is important in light of some formulations of the natural unemployment rate hypothesis.[11] See John Taylor (1998) for a discussion of the role of how evolving persistence in inflation can interacts with an imperfect specification of the natural rate hypothesis to yield inferences about the natural rate hypothesis that unnecessarily depend on the persistence of inflation.

[11]For example, the Romers' formulation of the natural rate hypothesis is $\pi_t = \pi_{t-1} - \theta(u_t - u^*)$. To rationalize this specification and an 'expectational Phillips curve' like Friedman's or Phelps's of the form $\pi_t = E_{t-1}\pi_t - \theta(u_t - u^*)$ requires that inflation be a random walk ($E_{t-1}\pi_t = \pi_{t-1}$). The Romers tell us that "This view that the change in inflation depends on the deviation of unemployment from the natural rate is the centerpiece of standard modern formulations of the natural-rate hypothesis." (page 12) and elsewhere they refer to this formulation as 'text book perfect'. I agree with John Taylor (1998) that this formulation is incomplete and misleading.

2.6 Difficulties in detecting trend breaks in productivity growth

Breaks and drifts in the trend rate of productivity growth are statistically difficult to detect and to disentangle from higher-frequency movements. For example, application of two-state Markov switching models that have aimed to detect and estimate breaks in trend productivity growth have instead detected higher frequency shifts between booms and recessions. This literature has much to say about evaluating estimates of the natural rate of unemployment and potential GDP using real time data together with one-sided filters.

2.7 Model uncertainty

Academic authorities from Milton Friedman to Alan Blinder have wrestled with how to respond to the fact that there is not a single macroeconomic model that they or other experts trust. Uncertainty about models, which is typically symptomized by wanting to bring *multiple* plausible models to bear in decision making, naturally creates a desire for decisions that are cautious or robust to model specification.[12] When there are multiple models in play, it is a subtle question about how to *learn* as new data become available. Economists' and statisticians' usual prescription for learning, which is to apply Bayes Law, presumes that a unique, but possibly very uncertain, model has been formulated.[13] One possible sense of Friedman's theme 'long and variable lags' refers to model uncertainty in the sense of multiple models, because lags typically do vary across models.

3 The Romers' narrative

The Romers' narrative is fascinating and I am very sympathetic to their story. But I think that a more nuanced and qualified view of the evidence would take into account some of the neglected ideas that I listed above. I have already mentioned Chari, Christiano, and Eichenbaum's (1998) work on expectations traps. In addition to the passages from Arthur Burns that these authors cite, distinguished monetary authorities like Blinder have written about the struggle about whether to confirm or disappoint what the market expects them to do. A credible government plan *is* an expectations trap, e.g., a system of expectations about the Fed's decision that the Fed has every incentive to confirm. By way of discussing credibility, Chapter 3 of Blinder (1997) contains a compelling discussion of whether the monetary authority should always confirm the market's expectations. Blinder is talking about expectations traps and about how to cope with them.[14]

A second reason for being cautious about the Romers' narrative interpretation that it is literary and not tight enough to subject to statistical verification. The Romers write about the process of the Fed's forgetting and learning, but they don't specify a model of learning.

[12] Bennett McCallum has urged and undertaken serious work on robustness of policies across models.

[13] Blinder (1997) wrestles with various aspects of this model specification and learning problem.

[14] See the passages in chapter 3 of Blinder (1997) about whether the Fed should always affirm the market's expectations about it.

When Chung (1990) and Sargent (1999) did write down and implement particular models of the Fed's learning process, they found that the Berkeley story has trouble fitting the facts because, by applying statistical procedures much like Samuelson and Solow's, the Fed would have learned to stop trying to exploit the Phillips curve at least by the early 1970s. That feature of the learning process diminishes the statistical fit of the story. One might come out of such statistical exercises with less confidence about the Berkeley story than the Romers' narrative conveys.

4 Hindsight and the unavoidable subjectivity of a narrative approach

Morris Zapp's dictum that 'every decoding is another encoding' warns us that a narrative approach is treacherous.[15] The narrator must filter the historical record, and any sensible filter is based on a model, so that the narrative has to reflect the narrator's own model. To illustrate some of the inherent difficulties of the narrative approach, I offer my own commentary on a string of quotations from a pair of prominent macroeconomists and policy advisors who wrote during the early 1960s. Before asking you to guess who they are, I will refer to these authorities as Professors X and Y.

My commentary unfolds.

4.1 Ahead of their time: warnings about instability of the Phillips curve

Professors X and Y convey a keen awareness that macroeconomics had not settled upon a commonly accepted model for interpreting output-inflation dynamics:

> Rather than pronounce on the terribly difficult question as to exactly which is the best model to use in explaining the recent past and predicting the likely future, we shall try to emphasize the types of evidence which can help decide between the conflicting theories. (p. 177)

As was typical at that time, Professors X and Y started from a benchmark model that asserted that monetary expansions would have no affect on real variables if they were engineered in a way to make them purely equivalent to changes in units of account. However, Professors X and Y emphasize that empirically most changes in money don't satisfy those neutrality conditions:

> But as Hume had early recognized, the periods of rising prices seemed to give rise to at least transient stimulus to the economy as active profit seekers gained

[15] Morris Zapp is a fictional character in several novels by David Lodge. Zapp is an English Professor at Euphoria State (Lodge's alias for my alma mater Berkeley, I think).

> an advantage at the expense of the more inert fixed-income, creditor, and wage sectors. (p. 178)

They thus caution that

> ... This illustrates the danger of going from the innocent hypothesis, that a balanced change in all prices in the long run be consistent with no substantive changes in real relations, to an overly simple interpretation of a complicated change that is actually taking place in historical reality. (p. 179)

Professors X and Y are cautious about using data to make inferences about competing views of inflation:

> What appear at first to be subtle and reliable ways of distinguishing cost-induced from demand-induced inflation turn out to be far from airtight. In fact we are driven to the belief that aggregate data, recording the *ex post* details of completed transactions, may in most circumstances be quite insufficient. It may be necessary to disaggregate. (p. 182)

Professors X and Y understand that expectations about the future shape current decisions, which means that inflation, output cross-correlations have a dynamic structure that must be interpreted carefully:

> ... in a closely interdependent economy, effects can precede causes. Prices may begin to ease up because wage rates are expected to. (p. 183)

> ... a period of high demand and rising prices molds attitudes, expectations, even institutions in such a way to bias the future in favor of future inflation. (p. 185)

Professors X and Y were ahead of their time in being skeptical about the permanence of an observed trade-off between inflation and unemployment:

> ... there is a suggestion that in this country it might take 8 to 10 percent unemployment to stabilize money wages. But would it take 8 to 10 percent unemployment forever to stabilize the money wage? Is not this kind of relationship one which depends heavily on remembered experience? We suspect that this is another way in which a past characterized by rising prices, high employment, and mild, short recessions is likely to breed an inflationary bias ... (p. 187)

Furthermore, Professors X and Y point out that long enough time series of U.S. data don't reveal much of a trade-off anyway:

> A first look at the scatter [of U.S. unemployment vs. the increase in the money wage] is discouraging; there are points all over the place. (p. 188)

They go on to note that it is only by focusing on short enough subsamples that they can spot a trade-off between inflation and unemployment, and they emphasize the past instability of those short-period trade-offs:

> What is most interesting is the strong suggestion that the relation [between unemployment and money wage increases], such as it is, has shifted upward slightly but noticeably in the forties and fifties. (p. 189)

Professors X and Y go on to emphasize the *prospective* instability of such a trade-off should policy makers attempt to exploit it:

> All of our discussion has been phrased in short-run terms, dealing with what might happen in the next few years. It would be wrong, though, to think that our Figure 2 menu that relates obtainable price and unemployment behavior will maintain its shape in the longer run. What we do in a policy way during the next few years might cause it to shift in a definite way. ... it might be that the low pressure demand would so act upon wages and other expectations as to shift the curve downward in the longer run – so that over a decade, the economy might enjoy higher employment than our present day estimate would indicate.[16] (p. 193)

Although this passage was written a number of years before Edmund Phelps and Milton Friedman formulated the natural unemployment rate hypothesis, it clearly conveys the sense that there is a family of Phillips curves indexed by the public's expected rate of inflation. There is also a firm suggestion of the Lucas critique, in particular that the location of the Phillips curve depends on monetary policy choices, through their effects on peoples' expectations of inflation.

This ends my commentary, which is sincere. I reveal the authors on the next page.

[16] But the same authors immediately qualify the preceding statement by adding

> But also the opposite is conceivable. A low-pressure economy might build up within itself over the years larger and larger amounts of structural unemployment ... The result would be an upward shift of our menu of choice, with more and more unemployment being needed just to keep prices stable.

4.2 Professors X and Y revealed

Professors X and Y are Paul Samuelson and Robert Solow and the quotations are all from their much maligned May 1960 American Economic Review article "Analytical Aspects of Anti-Inflation Policy". Romer and Romer join a host of previous writers who put black hats on Samuleson and Solow in the passage cited on page 1 of this note.[17] While the Romers' summary judgement about the ill effects of Samuelson and Solow's paper might be just, a subtler reading of Samuelson and Solow's paper and its subsequent influence and ramifications are also possible. For example, about Samuelson and Solow's paper Stanley Fischer (in Capie et. al (1994)) wrote:[18]

> The Phillips curve was brought to the United States by Paul Samuelson and Robert Solow (1960), who after presenting the menu view of the curve, warned that their discussion dealt only with the short run, and that it would be wrong to think that the same trade-off would be maintained in the longer run.(footnote: Samuelson and Solow gave two examples of how the curve would shift: first, that low inflation might shift the curve down because of its impact on expectations; second, that structural unemployment might rise as a result of higher unemployment, so that the curve would shift up. Both these possibilities have been central to subsequent discussions, the first as the expectations-augmented Phillips curve, the second as the phenomenon of hysteresis.)

Which reading of Samuelson and Solow is correct, Fischer's or the Romers'? Maybe both.[19]

5 Concluding remarks

The Romers' reading of the story of post WWII Fed behavior in terms of knowing, forgetting, and relearning is appealing and even comforting because in the end the Fed has gotten things straight, raising the prospects for a future characterized by excellent U.S. monetary policy. I would add some words of caution to this rosy outlook. As in any good story, the Romers have simplified things to make their point. They are optimistic that the Fed has converged on a correct specification of the dynamics of the system they control.[20] They ignore the role that fixed exchange rates probably played in disciplining Fed policy in the 50s and 60s. They downplay the possibility that the late 60s and 70s were times of especially large shocks, and

[17] Brad DeLong (1997) and Sargent (1999, chapter 1) also make Samuelson and Solow's paper into a villain. However, my interpretation is qualified significantly and is kinder to Samuelson and Solow than is the Romers' (for example, see the footnote on p. 2 of Sargent (1999)).

[18] I thank Athanasios Orphanides for pointing me to this quotation.

[19] Perhaps because Samuelson and Solow's paper contained a lower ratio of equations to words, it is subject to more ambiguity than were the many celebrated formal contributions of both of them.

[20] My 1999 book, which tells a story with a beginning similar to the Romers but diverges from them and ends on a sour note. I raised the possibility that the Fed did the right thing for the wrong reason, and that the fact that it might have accepted a faulty version of the natural rate hypothesis exposes it to the hazard of recidivism on the issue of exploiting the Phillips curve.

that in the last two decades shocks have been drawn from distributions that make the Fed's job easier. But let's hope that the Romers are correct and that the Fed has learned the most important lessons to be learned.

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