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Comment on Benjamin M. Friedman, "Does Monetary Policy Affect Real Economic Activity? Why Do We Still Ask This Question?"

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Benjamin Friedman's paper is a thoughtful contribution to the literature on the impact of monetary policy on the economy. It provides an up-to-date review of recent theoretical and empirical work on the subject. It weaves the review around a provocative theme, making for much more entertaining reading than the typical review article. I enjoyed the paper and I am sure that the readers of this conference volume will enjoy it, too. In fact, I agree with most of the paper and have only a few quibbles over emphasis and interpretation.

Why are academic specialists in monetary economics still researching whether monetary policy affects the economy, when practical policymakers (that is, central bankers and their economic advisers) seemingly take the effect for granted? Ben Friedman's answer to the question focuses on the increasingly demanding statistical standards of what it means to prove something in economics. In other words, statistical tests, which previously seemed to prove that monetary policy affected the economy, are continually being criticized and revised in ways which raise questions about their validity.

It is true that advances in time-series econometrics have invalidated finding after finding as faults with existing statistical methods were discovered. For example, the GDP gap—the percentage deviation of real GDP from potential GDP—was once commonly used as a measure of economic activity for the purposes of examining the impact of monetary policy on the real economy. But this measure of the gap was shown to be defective if potential GDP was measured using deterministic trends. Later, but in similar fashion, David Hendry and his colleagues criticized Milton Friedman and Anna Schwartz for not using the latest advances in econometric methodology when showing the impact of monetary policy on the economy. (Though Milton Friedman and Anna Schwartz showed in detail why the old ways were just fine, thank you!) And more recently, the VAR literature raised deep questions about the identification of monetary policy disturbances.

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While greater statistical refinement is certainly a valid point, I do not think it provides a fully convincing explanation for the wide difference between academic researchers and practical policymakers, which is the key part of Ben Friedman's question. If tests are being challenged in serious ways, then why would these challenges not affect the views of policymakers as much as academics?

Thus, the refinement of statistical tests does not fully resolve the puzzle that, at least from the vantage point of outside observers, there is much more debate in academic circles than in monetary policy circles over the basic fact about whether monetary policy affects the economy. I think that part of the explanation of the puzzle is in the very nature of the *probing* process common to all scientific research; to borrow from what I tell beginning students (Taylor 1995:1062–1063):

> Macroeconomic theory is by no means stagnant. Indeed like many of our theories of the physical world—from explanations of how genes in our bodies work to theories of why black holes exist in outer space—macroeconomic theories are in a constant state of flux. If you look at the details of what macroeconomic researchers have done in the past or what they do now, you will see numerous explanations as to why the theory does not work, why the theory no longer applies, and what changes in the theory are needed. The differing viewpoints—or schools of thought—cause passionate debates between the proponents of the various explanations. Eventually after much research and debate the prevailing theory is modified.

In other words research is a probing process: good researchers look unmercifully for defects and problems with existing models. Each probe causes huge debate and disagreement, whether it is successful in finding a defect or not. If no defect is found, or if the defect is minor, then it will have little or no impact on practical policymaking. Only in the relatively rare case will a probe be so devastating that the whole model is found to be incorrect and harmful as a policy aid, then the policymakers' framework must change, too. But usually, and even as the probing is going on, the practitioners continue to use the existing models. In academia, we see vociferous debate while in practice things look relatively calm. I think essentially the same thing goes on in other sciences.

There is also the related question raised by Ben Friedman about why the focus of the probes is frequently on the *either-or, yes-or-no* question of whether monetary policy has any effect, rather than on the size of the effect. Of course, a model has to be able to deliver a non-zero impact if it is to deliver any quantitative impact. And yes-or-no probes are usually easier to conduct and explain.

I also feel that theoretical research and debate about the existence of an impact can improve our quantitative knowledge of the size and timing of the impact. For example, the assumption of rational expectations alters both the size and the timing of the impact of monetary policy. But early studies which focused on the existence of the impact with rational expectations helped pave the way for those interested in finding the size of the impact, a much more difficult question.

In this connection, I would have emphasized the theoretical research on price stickiness more than Ben Friedman does in his paper. True, once prices are sticky we know that monetary policy may have an effect. But depending on the form of this stickiness the impacts of policy can differ greatly—by more than is due to differing econometric estimates of the size of the coefficients.

For example, the new Fuhrer and Moore model of sticky prices, to which Ben Friedman refers in his paper, has much different *theoretical* implications for the costs of disinflation than my original model of staggered contracts. But let me dwell on another example using even more recent work on sticky prices. As Ben Friedman shows in his paper, sticky prices are at the heart of question of whether monetary policy has an effect on the economy or not. Recent work by Caballero and Engle (1993) and King and Wolman (1995) that the probability that an individual price (or contract) will adjust depends on both the size and the sign of the deviation of the price from some desired price. This is a significant generalization of earlier work on sticky prices such as Calvo (1983), Chadha (1987), and Levin (1991). The latter three researchers develop staggered contract models in which contract prices are interpreted as having a probability of changing each period, but in which that probability is *exogenous*. The probability distribution is exponential in the case of Calvo (1983) and Chadha (1987) and more general and time-varying in the case of Levin (1991).

Just as in the earlier staggered contract models only a fraction of prices are adjusted each period in the Caballero–Engle and the King–Wolman models. However, unlike the earlier models, the distribution of the number of prices that undergo an adjustment is *endogenous*.

This endogeneity has a number of important monetary policy implications. But in order to explore those implications some basic empirical questions need to be asked. Are the aggregate price dynamics—and therefore the impact of policy—actually being explained by the model? It is also necessary to compare the cross-sectional implications of the Caballero–Engle and King–Wolman findings with those that have been estimated with the earlier staggered price setting models. Levin (1991) reports estimates of a cross-sectional distribution of wage changes in the United States which can be used for such a comparison.

In my view, one of the most important reasons to endogenize the price change or contract termination is to better understand monetary policy issues.

The exogeneity of the price change distributions in the staggered contracts models is one of their most criticized assumptions. Most of the criticisms are from those skeptical of their policy implications—including the finding that monetary policy can be effective.

Cabellero–Engle and King–Wolman have models which can potentially answer such criticism. Doing so, of course, will require embedding the price adjustment models into an aggregate demand model. My guess is that the policy implications will not be too different from the earlier staggered contract models. For example, if the distribution of micro price adjustments does not change much over time, then the existing staggered price of wage setting models will be good approximations for policy. But this guess remains to be proven and in the meantime the probe is going on. In any case, there are a host of important policy questions—from the question of fixed versus flexible exchange rates to the question of the optimal feedback rule for policy—that the Cabellero–Engle or King–Wolman frameworks can help answer.

I raise these newer sticky price models as examples to illustrate the importance of doing research which may implicitly involve the question as to whether monetary policy has any impact at all. Even though we think we know the answer to that question, the process of developing models of monetary policy requires that we ask that questions of new models as they are created. And a "no" answer may indicate a problem with the new model as much as, or more frequently than, it indicates a problem with what we "know."

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