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Discussion

Comment on: Robust monetary policy with competing reference models[☆]

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1. Introduction

Andrew Levin and John Williams have written an excellent paper—a wonderful contribution to this conference celebrating the 10th anniversary of Taylor's (1993) previous contribution to the *Carnegie-Rochester* series. For Levin and Williams' paper illustrates quite nicely the tremendous progress that has been made in monetary economics over the past decade, as well as the important role that Taylor has played in facilitating this progress. Let me begin, therefore, by citing just two manifestations of these phenomena, as they appear in Levin and Williams' work.

2. Taylor: 10 years after

Back in 1993, when Taylor's now-famous Carnegie-Rochester article appeared in print, papers including Cooley and Hansen (1989) and Hairault and Portier (1993) represented the cutting edge of research on the monetary business cycle. In many respects, the models developed by Cooley and Hansen and Hairault and Portier closely resemble the new Keynesian benchmark (NKB) model used today by Levin and Williams and many others. All three of these models have detailed microfoundations, and Hairault and Portier's model even includes the same

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elements of nominal price rigidity that lead to the derivation of the New Keynesian Phillips curve specification displayed as Eq. (1) in Levin and Williams' paper. In fact, by explicitly considering the process of capital accumulation, the Cooley–Hansen and Hairault–Portier models actually go beyond the NKB model in one important respect: as explained by Casares and McCallum (2000) and Woodford (2002), New Keynesian IS curves like Levin and Williams' equation (2) are typically derived without explicit reference to decisions relating to investment.

In retrospect, therefore, nothing about the way in which the Cooley–Hansen and Hairault–Portier models describe the optimizing behavior of private households and firms seems at all oversimplified or out of date. Instead, what appears most primitive about the Cooley–Hansen and Hairault–Portier models is the highly stylized way in which monetary policy enters those specifications: both models simply assume that the money growth rate follows an exogenous, first-order autoregressive process. Not a particularly realistic description of actual Federal Reserve policy, then or now!

Levin and Williams' paper, by contrast, features what has become the state of the art along these lines: here, monetary policy is described by a rule for adjusting the short-term nominal interest rate in response to movements in output and inflation. Clearly, interest rate rules of this type provide a much more accurate picture of how the Federal Reserve and other central banks around the world actually operate. And, just as clearly, Taylor's (1993) Carnegie-Rochester paper was instrumental in refocusing monetary economists' attention on the workings of interest-rate rules for monetary policy—so much so that such rules have widely become known as "Taylor rules".

But this is just one manifestation of the enormously positive influence that Taylor's work has had on the profession. For a second example, consider that published accounts by Brayton et al. (1997) and McCallum (1999) only begin to hint at the paralyzing sense of discord and discontent that prevailed in monetary economics ca. 1993. Then, most central bank economists worked with models that differed almost completely from those preferred by academic economists; and even within academics, researchers of different intellectual persuasions used models of very different types. More significantly, in 1993, there seemed to be very little interest in communication among economists from these various groups: productive exchanges of ideas were extremely rare.

Levin and Williams' paper, however, provides heartening signs that there has been at least some convergence in ideas and attitudes over the past decade: users of each of the three basic models they study, for example, can be found both on the research staffs of various central banks and on the faculties of various universities around the world. But again more significantly, during the past decade, monetary economists have come up with new and creative ways of productively acknowledging the differences of opinion that do remain and pushing forward despite those differences. Here in Levin and Williams' paper, for instance, substantial progress takes place through a search for robustness, that is, through a search for results that hold true across a wide range of specifications, reflecting a wide range of views about the workings of the monetary transmission mechanism.

Of course, any discussion of robustness in monetary policy evaluation must begin by citing McCallum's (1988) work, also from the Carnegie-Rochester series, where the basic research strategy adopted by Levin and Williams was originally developed. But Taylor, too, has been a strong advocate of the search for robustness in monetary economics; this same research strategy, for example, underlies the design of Taylor's own conference on monetary policy rules, which led to the publication of his (Taylor, 1999) edited volume. Once again, therefore, we see in Levin and Williams' paper from today the enormous progress that has been made in monetary economics over the past decade, as well as the enormous influence that Taylor's work has had in pushing the research agenda forward.

3. The results: good news for central bankers

Having placed Levin and Williams' paper in historical context, let me turn next to the specifics of their results. Here, of course, Levin and Williams present and discuss a large number of results; but in my opinion, the most important are these three.

First, Levin and Williams show that monetary policy rules that are fine tuned so as to be fully optimal in the context of any given model tend to perform quite poorly when transported to competing models and by definition, therefore, are not robust. Second, Levin and Williams show that despite the non-robustness of fully optimal rules, alternative rules do exist that are robust, at least in cases where the central bank's objectives are evenly balanced across the two goals of output-gap and inflation stabilization: these robust policy rules work quite well across the whole range of models considered. Third and finally, Levin and Williams show that these robust monetary policy rules call for a modest amount of interest rate smoothing, together with a balanced interest rate response to movements in output and inflation.

Although Levin and Williams hedge a bit, as all good scientists tend to do, in drawing implications from these results, and strive to maintain their fair, balanced, and skeptical voice throughout, I see their results as providing unambiguously good news for central bankers. For if we take it as given that central bankers around the world do seem to attach considerable weight to output-gap stabilization as well as inflation stabilization in choosing their policy actions, then Levin and Williams' results indicate that policy rules do exist that work well in achieving their balanced goals under a wide range of assumptions about the workings of the economy. Perhaps future work will lead us to alternative specifications that reveal that Levin and Williams' preferred policy rules aren't so robust after all, or perhaps future work will help us identify alternative policy rules that work even better. At least for now, however, Levin and Williams appear to have succeeded admirably in achieving their goal of finding monetary policy rules that work well under a wide range of assumptions and under a broad range of circumstances. This kind of progress, I would say, is far more than we might have reasonably hoped for 10 years ago.

4. Experimental design: a new type of cross-equation restriction

While, as suggested above, I find Levin and Williams' results quite interesting, useful, and relevant, I do want to highlight one aspect of their analysis that deserves more attention—a potentially important issue regarding experimental design.

Throughout their analysis, Levin and Williams assume that the central bank's objectives are described by a loss function of the form

$$L = \mathrm{var}(\pi) + \lambda\ \mathrm{var}(y) + \phi\ \mathrm{var}(\Delta i),$$

which penalizes variability in inflation π, the output gap y, and the first difference of the short-term nominal interest rate Δi. Each of their individual experiments then holds the weights λ and ϕ in this loss function fixed, while changing the form of the IS and Phillips curves describing the optimizing behavior of households and firms in the economy.

The potential problem arises here because in microfounded models at least, where the loss function L can be thought of as a measure of a representative household's welfare, the parameters λ and ϕ in the loss function depend on the underlying details of the model—the same details that ultimately determine the form of the IS and Phillips curve equations. The leading example of this general result is one that, in fairness, Levin and Williams themselves cite in their paper: in the NKB model, the parameter λ turns out to be quite small if price setting is assumed to follow Calvo's (1983) staggering specification, but much larger if price setting follows Taylor's (1980) specification instead. But other examples of this general result, presented by Giannoni and Woodford (2002), are even more dramatic: they show that as one moves from completely forward-looking specifications like Levin and Williams' NKB model to partially backward-looking specifications like their Fuhrer habit persistence (FHP) model, it is not just the parameters of a welfare-theoretic loss function that change, it is the basic form of the function itself. Apparently, a new type of cross-equation restriction has been discovered, linking the form of the central bank's loss function to the form of the IS and Phillips curves in a manner that is similar to the way in which the cross-equation restrictions originally brought to light by Lucas (1976) link the equations of a reduced-form macroeconometric model to the equations describing the monetary and fiscal policy regimes.

Unfortunately, these observations do raise questions about the logical consistency of Levin and Williams' exercises, in which the central bank's loss function is held fixed even as the IS and Phillips curves are varied. How important, quantitatively, are these newly discovered cross-equation restrictions? And will additional exercises that account more fully for these restrictions overturn Levin and Williams' results on robustness? Clearly, these are important questions for future research.

5. Taylor rules and Friedman rules

Now, let me close by offering up one final suggestion for future research—a suggestion that brings us back once more to Taylor (1993) and its enduring influence on the profession. As suggested at the outset, I think that it would be very difficult to overstate the importance of John Taylor's earlier contribution to the Carnegie-Rochester series. But given the almost exclusive focus on interest-rate rules in the recent literature on monetary policy, I wonder if it might be worthwhile to take a small step back and ask whether money-supply rules might also have a role to play in the kind of robustness exercise that Levin and Williams take us through here.

After all, before the Taylor rule, we had another famous monetary policy rule: Friedman's (1959) constant-money-growth-rate rule. And, perhaps not coincidently, Friedman argued in favor of his rule using the same kind of appeal to robustness that lies at the heart of Levin and Williams' work. According to Friedman, we know far too little about the workings of the monetary transmission mechanism to do any fine tuning; we should therefore focus instead of finding a policy rule that works at least tolerably well under a wide range of assumptions and circumstances. In Friedman's view, a policy of constant monetary growth has that desirable robustness property, and so I think it would be very interesting to see whether the basic elements of his argument could be restated more formally with the help of the same state-of-the-art research methods employed here by Levin and Williams.

Of course, money-supply rules of the type advocated by Friedman are often criticized, drawing on results that go back to a widely cited paper by Poole (1970). According to this criticism, interest-rate rules are preferable because money-supply rules fail to insulate the economy from the effects of shocks to money demand and because there can be no doubt that the profound technological and regulatory changes that have taken place over the past few decades in the United States economy have left private demands for the monetary base and the broader monetary aggregates highly unstable.

In response to this criticism, I would merely point out that Friedman's constant-money-growth-rate rule is but one type of money-supply rule. It is certainly possible to conceive of money-supply rules that allow the money growth rate to respond actively to changes in the economy, just as the Taylor (1993) rule calls for the interest rate to adjust in response to movements in output and inflation. One might consider, for example, a policy rule that manages the money growth rate in an effort to target nominal income. As Tobin (1983) and others have observed, the equation of exchange $MV = PY$ indicates that a policy that is directed at stabilizing nominal income PY also works to stabilize MV, which can be regarded as a shift-adjusted measure that corrects the money supply M for shocks to money demand or velocity V. This line of reasoning suggests that a money-supply rule that targets nominal income might provide the same sort of mechanical accommodation of money demand shocks that occurs under an interest rate rule. Alternatively, a money-supply rule might insulate the economy from the effects of money demand shocks by responding directly to innovations in velocity, as suggested by Meltzer (1987) and McCallum (1988).

How relevant are Friedman's (1959) robustness arguments, supporting a constant-money-growth-rate rule? And what advantages, if any, do more general money-supply rules offer in the search for robust monetary policies? These, too, are important questions for future research.

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