

## Commentary

Charles I. Plosser

It is indeed a pleasure to have the opportunity to be here today at the Thirty-Second Annual Economic Policy Conference of the Federal Reserve Bank of St. Louis. It is a conference I have attended a dozen times or more in various roles. In each and every case, it has proven to be a timely and thoughtful interchange among academic economists and policymakers.

I am particularly pleased to be participating this year, since this is the last of these conferences that will be held during Bill Poole's tenure as president of the Federal Reserve Bank of St. Louis. I have had the privilege of knowing Bill for at least 25 years, perhaps more. I am sure that many of you in the room can also make similar claims.

Over the years I have learned a great deal from Bill. His seminal contributions in the area of monetary theory and policy are widespread and span four decades. Whether it be his contributions on monetary policy under uncertainty, his early investigations of simple rules for setting the federal funds rate, or his analysis of rational expectations models of the term structure for monetary policy, his theoretical contributions provided fundamental insights and played an important role in developing what we now view as the core of modern monetary theory. He has continued his contributions to monetary policy as a member of the Federal Open Market Committee (FOMC), bringing the same sound, thoughtful, and consistent economic analysis to policy deliberations.

One of the themes of Bill's work was the importance of uncertainty for monetary policy. One dimension of uncertainty involves our uncertainty regarding the nature of the macroeconomic model that governs the economy. In Poole (1971), Bill investigated the performance of simple "rules of thumb" for setting the federal funds rate. He argued that these simple rules appeared to be "robust" across various model specifications.

This line of research has become increasingly active and has some important things to say about the conduct of monetary policy. I find the analysis of simple rules intriguing for a couple of reasons. First and foremost, they are rules. Second, in a framework where policy is decided by committee, simple rules that are robust across models provide a valuable focal point for discussion among people with different world views.

Bill's primary concern dealt with uncertainty in a given model, but he also discussed how optimal policy would vary when model parameters changed. Thus, his concern reflected a desire to analyze optimal policy under model uncertainty. As I indicated, this area of research has seen a resurgence in recent years, with various methodologies ranging from robust control to Bayesian model averaging being employed to analyze optimal policy under uncertainty. Perhaps even more interesting is the research that considers the robustness of simple rules across models that are non-nested and thus potentially very different.<sup>1</sup>

<sup>1</sup> McCallum (1988) was one of the first to investigate the robustness properties of simple rules.

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Before I discuss the contributions of the Orphanides and Wieland (2008) paper, which I found very interesting, I would like to backtrack and talk about why I believe explorations of rules of thumb are important and what we know about their performance.

As I stated at the outset, one of the greatest attractions of simple rules is that they are in fact rules. Since the pioneering work of Kydland and Prescott (1977), we have come to understand the theoretical foundations for the importance of commitment by policymakers.

One way that commitment manifests itself is that, in model economies, the optimal monetary policy typically takes the form of a rule. In these models the researcher looks for policies that deliver efficient allocations, that is, the allocations that would be selected by a Ramsey planner. In this context, optimal policy need not be simple, but it does need to be a rule.

However, the Fed does not pick allocations like the Ramsey planner—it picks an instrument and moves that instrument to influence economic outcomes. Thus, there are important issues regarding the implementation of policy that must be considered, and this is where I believe that simple policy rules have a role to play.

The question then is, why might we choose to adopt simple rules? If everyone had the same model of the economy, there would be no reason to do so. So the underlying attraction of investigating simple rules is twofold. First, everyone may not agree on a common model. Thus, optimal policy for one policymaker may not be optimal for another. Second, even if there is an agreed-upon model, the economy is likely to be more complicated than the model, so the optimal policy for that model may not be the optimal policy for the true underlying economy.

So it seems natural to ask if there are simple rules that capture the essence of optimal rules and that give good results in a variety of theoretical environments. In other words, how different are the allocations under simple rules from those obtained under optimal policy? How costly is a simple rule?

A number of researchers have analyzed the performance of simple rules. One interesting

approach is that by Schmitt-Grohé and Uribe (2006). The underlying model is quite rich, incorporating price stickiness, investment adjustment costs, habit persistence, variable capacity utilization, and monopolistic competition. The model considers three types of shocks: policy shocks, total factor productivity shocks, and investment-specific technology shocks.

They find that a simple Taylor-like rule that responds aggressively to inflation, wage growth, and very little to deviations of output growth from target comes very close to achieving the optimal allocations. In fact, a rule that responds solely to price inflation yields good results in this model. The basic message here is that, in a model with large non-neutralities, but primarily forward-looking agents, a simple Taylor-like rule comes fairly close to implementing Ramsey allocations. Perhaps surprisingly, the properties of the rule place significant weight on an aggressive response to deviations of inflation from target.

Although the model of Schmitt-Grohé and Uribe does a fairly good job of matching the data, it may not be the only model to do so. Other policymakers or researchers may wish to stress different model features. For example, I may not be as keen on models with the degree of price stickiness or as comfortable with the adjustment costs and habit formation built into the model, but I do place a lot of stock in models with forward-looking rational agents. So it is of some interest and importance to question whether these intriguing results are robust to perturbations in the model not considered by the authors. Basically, we want to know how robust these findings are to models that accommodate very different views of behavior, models that perhaps fit the data as well as the one Schmitt-Grohé and Uribe consider.

The question of robustness has been addressed in a number of ways. The most common strategy is to look at the performance of simple rules in a host of different, sometimes non-nested models. In these exercises, the most interesting questions in my mind are these: How similar are the optimal rules from different models? And are there simple rules that work well across models?

Levin, Wieland, and Williams (1999 and 2003) have explored the performance of various

simple rules in a number of model contexts. They characterize the optimal simple rule for each model and find that there are broad similarities that describe the best simple rule. In some cases they find the best simple rule that minimizes the average loss across models. In their 2003 article, this rule responds to smoothed inflation forecasts at most one-year ahead, current inflation, lagged interest rates, and, unlike the Schmitt-Grohé and Uribe analysis, in a significant way to the output gap. In part, this distinction is driven by differences in the loss functions. One feature of the robust rule is that it exhibits inertia in that the coefficient on the lagged interest rate is close to 1. This is in contrast to the results found in Schmitt-Grohé and Uribe, where inertia was not important.

Besides uncertainty resulting from stochastic factors or from not knowing the true model, policymakers and the public may be unaware of the true processes governing the stochastic elements of the model.

Orphanides and Williams (2002 and 2007), for example, examine the usefulness of simple rules when the processes for the natural rate of interest and employment are unobserved and not known. Further, agents form forecasts of relevant macroeconomic variables using a learning methodology. The learning mechanism along with persistent errors in estimating natural rates yields highly nonlinear behavior and implies significant departures from the rational expectations equilibrium. An important feature of this model is that there is the possibility that expectations of inflation can become unanchored. The main lesson I take away from their analysis is that, even in this environment, simple rules can work quite well, but those rules should be based on rates of change in output or employment. This avoids the notoriously difficult problems associated with estimating natural rates of output or unemployment.

These analyses indicate that in the presence of very different types of uncertainty—stochastic disturbances, uncertainty about the correct model, and uncertainty about the nature of true driving processes—simple rules for monetary policy are able to deliver good economic outcomes.

To many observers it comes as somewhat of a surprise that simple rules should do as well as

they do. The reason they do is not obvious. It is hard to know whether some form of Occam's razor is at work or something else. This is somewhat uncomfortable from a theoretical perspective, yet I find the analyses to date convincing and useful from the perspective and experience of a policymaker and economist.

Given that simple rules appear to have desirable properties in terms of delivering good outcomes in a variety of theoretical settings, I think they have other desirable characteristics that suggest they are of value to policymakers.

First, policy guided by simple rules is easy to monitor and to communicate. Transparency is an important attribute of good monetary policy. In a world where expectations of the future play a critical role in economic outcomes, the transparency and predictability/credibility of monetary policy can reduce expectational errors and contribute to a more stable economic environment. Using simple rules, which can be easily communicated to the public, as benchmarks or guidelines can enhance both transparency and credibility of policy.

I believe that there is another benefit from using simple rules as a guide for policy. Specifically, I believe that simple rules serve as a useful focal point in policy deliberations. The underlying models employed by various FOMC members can be quite different and, in some cases, may not even share the same set of state variables. Thus, deliberating the implications of various policy options or the workings of the economy can be quite complicated. Indeed, as I previously discussed, the optimal rule, even from a well-articulated model, can be quite complex and quite different across models. If the underlying model is not well articulated or completely specified, then we may not even know what the optimal or best rule might be. Thus, trying to reach a consensus on appropriate policy can be difficult.

By focusing on simple rules, deliberations can focus on a few key variables and our assumptions or forecasts that shape them. It also leads to a more focused discussion of the shape and parameters of the loss functions that may be applicable. I believe this would greatly improve policy deliberations by directing attention to the key factors

that matter for the policy choices. Of course, these benefits arise only if the simple rules have some good robustness properties associated with them. As I have indicated, my reading of the literature to date makes me optimistic that this is indeed the case.

Finally, let me turn my attention to the Orphanides and Wieland (2008) paper. How does their paper fit into this broader literature? First, the investigation is a positive analysis rather than a normative one. That is, they seek to study and uncover the characteristics of FOMC decisions over the past 20 years in the context of a simple rule.

The rules investigated are simple and, importantly, are based on the real-time information that policymakers actually possess at the time decisions are made. The rule that seems to explain Fed behavior the best is a forward-looking rule that responds aggressively to deviations of inflation from target. In this regard, Fed behavior seems in accord with the guidance of robust rules. If anything, the response seems more aggressive than is sometimes indicated by the more theoretical investigations.

The Fed also appears to respond to forecasts of unemployment and its deviations from some natural rate. The authors do not directly estimate a natural rate of unemployment; they allow it to be subsumed in the constant term. This strategy may or may not be a good one. Indeed, some of Orphanides's own work suggests that looking at growth rate rules might be a better practice, and it would have been interesting to see how they would have stacked up in this comparison. Of course, this might not make too much difference over this period if there was not much movement in the natural rate.

Another interesting feature of the results is that the degree of inertia is markedly less when the rules are assumed to be forward looking, that is, based on forecasts, than when they are based on outcomes. The robust rules prescribed in Levin, Wieland, and Williams (1999 and 2003) or Orphanides and Williams (2007) suggest that policy should be more inertial. What might be the reasons for this finding? One possibility is that the robustness results may be relying too

heavily on learning and the world may be more rational and forward looking than the models presume.

Probably one of the more interesting findings in the paper concerns the shifting focus of the Fed's preferred inflation measure. The puzzle is that, as the Fed shifted its emphasis from the consumer price index to the core personal consumption expenditures, it apparently did not change the parameters of the estimated rule. This is a puzzle because the personal consumption expenditures measure generally is about 50 basis points below the consumer price index, on average. Thus, changing the inflation measure allowed the Fed to maintain a lower funds rate for a given rate of inflation. I will not speculate on why this is the case. But it does suggest that policy was not as committed to a policy rule as the regression estimates seem to suggest.

Overall, I found the paper interesting and useful in furthering our understanding of simple rules. It points to some unresolved issues, particularly those that pertain to the ability to describe actual policy as rule based. That does not mean that simple rules are any less useful or valuable. Indeed, it may suggest that policy can be improved by being more transparent and committed to systematic behavior.

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