Comment

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Introduction

This paper is an ambitious and engaging project. It aims to answer one of the basic questions in macroeconomics: How should a benevolent government conduct fiscal and monetary policy? More concretely, it investigates issues as important as: Should we lower taxes in a recession? Or should we keep the government budget balanced? Should we aggressively reduce inflation and achieve price stability? Or does inflation have positive effects on the economy?

Stephanie and Martín (SM hereon) build their paper around three key elements. First, SM specify a rich dynamic equilibrium model with those real and nominal rigidities that have been shown to be important in accounting for aggregate fluctuations in the U.S. economy. Second, they use a form of policy commitment recently proposed by Woodford (2003) known as the timeless perspective. Third, SM follow a quantitative approach: they calibrate and compute the model to generate concrete numbers to characterize the optimal policy. As bonus material, SM present simple monetary and fiscal rules that implement a competitive equilibrium close to the one induced by the optimal timeless perspective. These simple policy rules eliminate the problem of multiplicity of equilibria that may appear if we directly implement the optimal policy.

Among SM’s main findings, I would highlight the importance of price stability as the central goal of optimal monetary policy. Under an income tax regime, the optimal rate of inflation is 0.5 percent a year with a volatility of 1.1 percent. A second important finding is that governments should smooth taxation: the optimal tax on income is 30 percent with a 1.1 percent standard deviation. Finally, the paper shows
how, if we let labor and capital income be taxed at different rates, it is optimal to provide a large and volatile subsidy to capital.

I strongly agree with SM that we should use dynamic equilibrium models to conduct optimal policy exercises in the spirit and style of the exercises conducted in this paper. In addition, I find their results appealing and intuitive. I enjoyed thinking about the paper a lot. I hope a large audience of macroeconomists will read this work over the next few years. SM have offered a thorough review and consolidation of the literature on optimal policy and have pushed the frontier of macroeconomics several steps ahead.

Having said that, I do not have much to add to the content of the paper beyond a few minor points. First, I will talk about the strengths and weaknesses of the medium-scale model specified by SM. Second, I will discuss the formulation of the policy problem. Third, I will offer a few suggestions regarding the empirical strategy of the exercise. Finally, I will evaluate the results.

Model

SM build their model around the recent and influential paper of Christiano, Eichenbaum, and Evans (2005). I will refer to the authors of this paper as CEE from now on. CEE’s paper is an important contribution because it shows how we can formulate and estimate a model of the business cycle compatible with the evidence gathered from an identified vector autoregression (VAR).

CEE prove how dynamic equilibrium models of the business cycle have come a long way since the pioneering contribution of Kydland and Prescott (1982). Thanks to advances in economic theory and progress in numerical techniques over the last twenty-five years, macroeconomists know how to build rich and powerful models of the business cycle. Old challenges, such as the modeling of monopolistic competition, sticky prices and wages, credit market imperfections, and others are today much better understood than they were a few years ago.

These developments are important, not only because they allow us to formulate better models but also because these better models have a nontrivial role for fiscal and monetary policy. This more active role for policy requires that macroeconomists think carefully about the advice we provide, especially because policymakers are noticing the success of equilibrium theory. A growing number of policy institutions (the European Central Bank, the Federal Reserve Board, the Riksbank, the
International Monetary Fund, the Bank of Canada, the Bank of Spain, and the Bank of Italy, among others) are formulating and estimating dynamic equilibrium models of the business cycle similar to CEE’s for policy analysis.

But before these models play in the big leagues of conducting actual monetary and fiscal policy, we want to coach them to ensure that their debut is successful. To help in this task, I discuss some potential problems of CEE’s and SM’s models and suggest some potential avenues for improvement.

Potential Problems of the Model
CEE’s and SM’s models are an impressive achievement. With a moderate amount of real and nominal rigidities, CEE and SM account for a surprising amount of aggregate dynamics. However, it is wise to keep a critical eye on some shortcomings of the models to help us both improve upon them and assess how much confidence we should place in the policy recommendations they generate.

The first concern is the high level of price and wage stickiness required by CEE to achieve their results. SM set the average duration of firms’ prices to be ten months. However, Bils and Klenow (2004) report that, according to Consumer Price Index (CPI) data, half of the prices for goods last less than 4.3 months. This excessively high degree of price stickiness is not unique to CEE or SM. Smets and Wouters (2003), whose paper is a close relative of CEE’s, need to fit the data that firms change their prices, on average, every twenty-seven months. Galı´ and Gertler (1999) and Eichenbaum and Fisher (2004) ask for firms changing prices only every eighteen months.

To answer this criticism, Altig et al. (2005) have modified CEE. In CEE, as in basic Real Business Cycle (RBC) models, capital is freely and instantaneously movable across firms. Altig et al. assume that the capital stock of each firm is predetermined in each given period. Thus, the marginal cost of the firm is rapidly increasing in its own output. Altig et al. note that, in this environment, firms change prices only by a small amount. Consider a firm that wishes to raise its price in response to a monetary shock. Since a rise in price will imply a drop in output, the firm will have a much lower marginal cost and, hence, a lower optimal price. Both effects, a higher price in response to a monetary shock and a lower price because of lower marginal cost, will nearly cancel each other. Moreover, Altig et al. show that the computation of this model is nearly as simple as the solution of CEE. The
theoretical results and the easiness of computation suggest that SM could extend their model to introduce firm-specific capital. Hence, SM could reduce the required level of price and wage stickiness and investigate optimal policy in this more flexible, yet empirically successful, environment.

A second related problem of CEE is its relatively weak built-in persistence. In CEE’s theoretical model, output response to a monetary shock is positive right after the policy innovation, and it peaks after four quarters. In the identified VAR estimated by CEE, output does not rise until two quarters after the shock, and it peaks at six quarters, with a much slower decay after the peak than in the theoretical model. This lack of persistence is worrisome because it casts some doubts on the internal propagation mechanism of the model and its adequacy to serve as a laboratory for policy and especially because CEE estimate their model to match the VAR impulse-response functions.

A third issue is SM’s assumption that labor decisions are made by a union that supplies labor monopolistically to firms. This assumption goes against the evidence on wage posting by firms (Mannig 2003) and likely against the observation that only 12.5 percent of American workers are union members. This criticism is relevant because in the type of models exemplified by CEE, wage rigidities are more important than price rigidities in accounting for the dynamics of the aggregate quantities. An alternative and more positive interpretation is that the wage rigidities in CEE are a reduced form for some underlying structure of the labor market that implies a high degree of rigidity on wages. However, we still do not understand that structure very well.

Finally, I would like to offer some comments about the microfoundations of CEE and SM. A basic thread of macroeconomics is the search for general equilibrium models soundly built around explicit assumptions about preferences and technology. How well do CEE’s and SM’s models perform on the microfoundations test?

The metric of what we consider assumptions about preferences and technology displays an uneasy degree of arbitrariness. For example, working with an aggregate production function or with putty-putty capital does not raise an eyebrow nowadays, but it was the object of furious discussion not so long ago in the nearly forgotten Cambridge Controversies (Cohen and Harcourt 2003). But even keeping these ambiguities in perspective, several aspects of CEE’s and SM’s models are troublesome.
Among those, I could discuss the mechanism behind Calvo pricing, the investment adjustment costs, or the union labor market structure. In the interest of concreteness, I will focus on the introduction of a demand for money through the joint assumption of a proportional transaction cost for households and a cash-in-advance constraint for firms. We could equivalently present this joint assumption as a statement about the productivity of real money balances.

Wallace (2001) has forcefully criticized those models in which real balances are assumed to be productive. He argues, first, that the models contain hidden inconsistencies that are difficult to reconcile with standard theory. Second, the relation between the fundamental models of money (those where the environment is such that money is essential) and the reduced-form structure embodied by productive real balances is often not clear. Moreover—and this point is especially important for my discussion—we do not know whether that relation is invariant to changes in monetary policy. The reader can recognize how this last concern is nothing more than the Lucas critique slightly disguised. SM are proposing a model for designing monetary policy, and a crucial component of that model, the money demand, is a black box whose response to changes in the systematic component of monetary policy is hidden from the researcher.

There are two answers to this attack. First, the behavioral responses to a variation in the systematic component of monetary policy reflected in the reduced form of the money demand are likely to be small. Even if this may well be the case, defenders of productive real balances models of money do not provide a procedure to verify this claim. The second and more compelling answer is that fundamental models of money have not delivered an operational alternative to reduced-form models. We do not have a well-founded model of money that can be taken to the data and applied to policy analysis. Ricardo Lagos and Randy Wright have recently made important progress on this front (Lagos and Wright 2005). However, there is still a lot of space to cover before Lagos-Wright or a similar model can compete in terms of empirical fit and flexibility with CEE. In the meantime, models of money where real balances are productive, like SM’s, need to fill the void.

**Suggestions for Enhancing the Model**

We can use a model like SM’s for two different purposes. First, we may want to develop intuition about how the different real and nominal rigidities shape optimal policy. In this type of exercise, even if numbers
are important, they somehow play a secondary role behind the qualitative results. The goal is to further our understanding of economic theory and its implications. Second, we may want to use the model to provide policy recommendations or at least broad guidelines for action. My reading of the paper is that SM’s model aspires to fill this second role as an instrument for thinking about the real world. But if this is the objective, the hurdle we need to overcome is more challenging since we need to pick the most salient features of the data to provide sufficient trust in the results of the exercise.

The selection of these prominent components is a daunting task. Even if I do not have special insights with respect to this choice of modeling features, in the interest of the discussion, I will dare to offer three suggestions: first, to model long-run growth explicitly; second, to introduce investment-specific technological change; and third, to think about the open-economy implications of the model.

**Growth**  SM’s model lacks long-run growth. In the basic CEE model, this is not a big concern. We know that the cyclical properties of the basic RBC model and its descendents, like CEE, are not affected much by the presence of growth. We handle the issue of long-run growth either by ignoring or (since detrending is usually straightforward) by working with detrended variables.

However, long-run growth might not be so innocuous when we investigate optimal policy. SM recognize this point when they report that in the Chari et al. (1995) model, the standard deviation of capital income tax increases by 60 percent when we eliminate long-run growth. This result begets the question: Are there more cases where the absence of long-run growth will change the conclusions of the paper? Since the marginal cost of introducing long-run growth seems low, it may be a worthwhile addition to the model.

**Investment-Specific Technological Shocks**  The work of Greenwood, Hercowitz, and Krusell (1997, 2000) and Fisher (2003) has emphasized the role of investment-specific technological change as a main driving force behind economic growth and business-cycle fluctuations. Fisher (1999) documents two observations to support this view. First, the relative price of business equipment in terms of consumption goods has fallen in nearly every year since the 1950s. Second, the fall in the relative price of capital is faster during expansions than during recessions.
The role of investment-specific technological change appears clearly in models similar to SM’s. For example, Boivin and Giannoni (2005) estimate a model by Smets and Wouters (2003) using a rich data set and a factor structure. They report that investment-specific technological change accounts for 42 percent of the variance of output at an eight-quarter horizon. If we compare this finding with the role of government expenditure (8 percent of the variance) or with monetary policy (21 percent), the case for modeling investment-specific technological change appears strong.

An attractive characteristic of investment-specific technological change is how easily we can introduce it in our models. The law of motion of capital for SM would become:

\[ k_{t+1} = (1 - \delta)k_t + \xi_t \left[ 1 - \frac{\kappa}{2} \left( \frac{i_t}{i_{t-1}} - 1 \right)^2 \right] \]

where \( \xi_t \) is the investment-specific technological shock. We can assume, as SM do for the other shocks in the model, that the logarithm of \( \xi_t \) follows a first-order autoregressive process of the form:

\[ \log \xi_t = \rho_t \log \xi_{t-1} + \xi_t^i, \quad \xi_t^i \sim N(0, \sigma^2) \]

where \( \rho_t \leq 1 \).

Beyond a better empirical fit, investment-specific technological change has the potential for qualifying some of SM’s results. An investment shock has a different impact on aggregate quantities than a regular productivity shock. The fall in the relative price of investment induces an increase in hours worked and a relative fall in consumption and labor productivity in the short run (in opposition to the standard model, where hours and productivity increase at impact). These dynamics hint at the possibility of different behavior, for example, of the tax on capital over the cycle. The main argument of Judd (2002) about justifying the subsidies to capital relies on equating the social rate of return of capital and the private rate of return. Those rates of return are directly affected by investment-specific technological change. Thus, the properties of the tax on capital, in particular its correlation with the business cycle, may change in this richer environment.

**Open Economy** Even a cursory following of the media reveals that open-economy considerations are an important component of economic policy. Discussions regarding the exchange rate of the dollar or
the U.S. trade deficit fill the pages of the *New York Times* or *The Economist* and preoccupy both the Treasury and the Federal Reserve System.

SM chose to skip all these open-economy considerations in their paper. Their choice is the correct one. SM already advance our understanding of optimal policy models with real and nominal rigidities by several important steps. Attempting to deal with open-economy considerations in the same paper risks lack of focus and clarity.

However, I am eager to see this research agenda develop in the next few years to model open economies. Just to sketch how such a model can be built, I will offer a summary of Adolfson et al. (2004). These authors extend CEE, the model of reference chosen by SM, by adding open-economy components. The model requires modification of only three aspects.

First, the model has three categories of firms: domestic firms, importing firms, and exporting firms. The intermediate domestic-good firms behave as in SM, producing an intermediate good that is aggregated into a final good. The importing firms transform a generic imported good bought in the world market into a differentiated imported good, which is sold to domestic households. The exporting firm buys the final good and differentiates it by brand name.

Second, the consumption of the household \( c_t \) can be defined as:

\[
\begin{align*}
    c_t &= \left(1 - \omega \right)^{1/\eta_c}(c^d_t)^{1/\eta_c} + \omega^{1/\eta_c}(c^m_t)^{1/\eta_c} \\
    &+ \eta_c \omega(c^m_t)^{1/\eta_c} \\
    &= \left(1 - \omega \right)^{1/\eta_c}(c^d_t)^{1/\eta_c} + \omega^{1/\eta_c}(c^m_t)^{1/\eta_c} \\
    &+ \eta_c \omega(c^m_t)^{1/\eta_c}
\end{align*}
\]

where \( c^d_t \) is a composite of all the domestic differentiated goods, \( c^m_t \) is a composite of all the goods produced by importing firms from the generic imported good bought in the world market, \( \omega \) is the share of imports in consumption, and \( \eta_c \) is the elasticity of substitution across consumption goods.

Finally, Adolfson et al. follow Beningno (2001) and close their model with a premium on foreign bond holdings, which depends on the real aggregate net foreign asset position of the domestic economy. This premium induces stationarity in the model by making it more costly to borrow when the economy has a lower net foreign asset position.

Adolfson et al. estimate their model using Euro area data and Bayesian techniques. They show how the model can capture the volatility and persistence of the real exchange rate and the observed large home bias in trade. These results seem to be a promising line of research, and I invite SM to extend their results to a model with open-economy components. Since SM are leaders in the field of both open macro and opti-
mal policy, they may have a great comparative advantage in that undertaking.

**Fiscal Policy Instruments** The tradition in Ramsey taxation problems has focused on the question: How should we tax in order to raise the resources to finance an exogenous stream of government expenditure? SM follow this tradition. In practice, however, governments often employ public expenditure as a powerful policy instrument. This intervention may be explicitly targeted toward stabilizing the economy, following the Keynesian tradition, or implicitly targeted, as in the military buildups of the early 1980s and after 9/11 (I do not want to enter into the discussion of whether the military buildups were exogenous; suffice it to say that they were in part freely chosen by the different U.S. administrations). Burnside, Eichenbaum, and Fisher (2003) document how a persistent increase in government purchases leads to a persistent increase in aggregate hours and a decline in real wages.

The intuitive answer to this suggestion—we do not really use public expenditure any longer to stabilize the economy—can be turned around: maybe we should! A suitably modified version of SM could address questions such as: Does the use of government expenditure increase welfare? Is it a better or a worse alternative than tax policy?

I should disclose here that my prior is that playing with government expenditure is unlikely to be a sound policy recommendation. However, I would like to have a model to back up my belief. A version of SM may play that role.

**A Caveat** At this moment, it is important to offer a caveat regarding the complexity of the model. This warning will, up to some point, contradict my own words in the last pages. When do we know enough is enough? When should we stop the process of enriching the models we want to use for policy analysis?

As I mentioned before, this problem is more complicated in an environment like SM’s than in purely theoretical papers since we want to use the model for real policy evaluation. On one hand, we want enough detail to capture the dynamics of the data. On the other hand, too much detail may imply the loss of intuition and too many confounding effects. For example, in SM’s model, the policymaker faces a tradeoff between minimizing relative product price dispersion and minimizing relative wage dispersion. This tradeoff is solved quantitatively in favor of minimizing price dispersion. However, this result is
sensitive to the shocks driving the business cycle. As SM point out, Chugh (2005) reports how, with government expenditure shocks but without productivity shocks, volatile inflation may increase welfare even with nominal rigidities. How many of these results reversals are hidden in a complicated model? The point of the example is to illustrate how the interaction between shocks and rigidities may get too complicated to provide a thorough understanding of the behavior of our model.

I do not want to construct this example as a criticism of rich models. I want to present it as a warning. After having absorbed the shock of *The General Theory*, macroeconomists in the 1950s built and estimated models such as Klein-Goldberger’s, which showed a fantastic fit for the standards of the time and great promise for future development. Unfortunately, the models from the 1950s did not scale well in the 1960s. Models such as the Brookings model or the FRB-MIT-PENN model became too cumbersome. They were difficult to handle and nearly impossible to understand, and their forecasting power was very poor (Nelson 1972).

We may now be in a situation similar to the 1950s. We are creating and formulating models that show a fantastic fit for the standards of our time and great promise for future development. We do not want to end up saddled with models too complicated and too cumbersome to understand.

**Timeless Perspective**

SM follow a form of policy commitment recently proposed by Woodford (2003) known as the timeless perspective. This perspective differs from the standard Ramsey solution in that the timeless form imposes time invariance on the optimal policy of the government. In that way, the behavior of models such as Chari et al. (1994), where the government levies a large tax on capital in the first period, is not allowed.

I like the application of the timeless perspective in the paper. Timeless perspective has an intuitive appeal that the standard Ramsey solution concept lacks because of its first-period peculiar behavior. I will comment only on two issues.

First, the difference between standard Ramsey and timeless perspective may be important in some cases. Most of the welfare gains in Chari et al. (1994) come from the first period taxation of capital. The reason is that taxing capital at a high rate in the first period is a non-
distortionary procedure to build public assets. The income from these public assets will allow future reductions in taxes. In the timeless perspective, all those welfare gains disappear, and since the government needs to tax more in each period, the economy will converge to a different stochastic steady state than under Ramsey. Even if we prefer the timeless perspective to standard Ramsey, it would be interesting to have a more thorough understanding of the differences between the two approaches.

Second, we must keep in mind that the results from the timeless perspective can be very different from the results from a sustainable equilibrium. The assumption of commitment has nontrivial implications, for example, with respect to the taxation of capital. This observation is well known. I will, however, cite my paper, Fernández-Villaverde and Tsyvinski (2002), just because I understand it better. Aleh and I show that the best equilibrium in a stochastic business-cycle model with taxation implies a tax on capital that fluctuates around 25 percent. This is quite different from the result in Chari et al. (1994), where capital income tax fluctuates around 0 percent. This shows how important the quantitative effects of lack of commitment are.

However, the problem of handling lack of commitment is not trivial since we need to resort to the Abreu-Pierce-Stacchetti toolbox (Abreu et al. 1990), which imposes a considerable computational burden. Nothing like my exercise with Aleh can be attempted at the moment for a model like SM’s. Thus, I want to emphasize only that we should remember the importance of the assumption of commitment behind SM’s results.

**Empirical Strategy**

SM’s model is profligately parameterized. Table 6.1 in the paper lists twenty-eight parameters. Because the models are so close, SM borrow most of their parameters from the empirical work of CEE and Altig et al. (2005). In addition, SM have several explicit calibration targets for the remaining parameters. This mixed strategy is a reasonable compromise between a thorough empirical implementation of the model and complexity.

However, I would like to draw attention to two different potential problems. First, structural parameters do not have a life of their own. There is no or out there waiting to be discovered. Parameters are only meaningful within the context of a model. Even if CEE and Altig
et al. (2005) are models very similar to SM’s, they are not the same model. Consequently, we do not have any reassurance that the parameters are structural with respect to the change of models.

A more consistent approach will attempt to estimate the parameters of the model using U.S. data. There are many procedures to do this: different versions of method of moments, indirect inference (as in CEE, when they match impulse responses of an identified VAR), maximum likelihood, etc., but my favorite empirical strategy is Bayes. The Bayesian approach is a powerful procedure to take dynamic economic models to the data. Fernández-Villaverde and Rubio-Ramírez (2004a, 2004b) show that Bayes has good asymptotic and small sample properties, even if the model is misspecified and even though it can handle nonlinear models much better than classical methods. Moreover, it offers a formal mechanism to incorporate prior information, such as the estimates of CEE and Altig et al. (2005). Thanks to all these attractive properties, over the last few years, the Bayesian paradigm has been applied to numerous dynamic equilibrium models. Two recent examples are in this macroeconomics annual, where Levin et al. (2005) use Bayes to study monetary policy under uncertainty and Lubik and Schorfheide (2005) implement the Bayes approach to estimate open-economy models.

There are several additional advantages of an explicit estimation procedure. First, it is easier to sell to policymakers since they appreciate formal approaches that are reproducible. Second, it allows us to assess the fit of the model and compare it with alternatives. Third, it gauges uncertainty regarding parameters. Finally, estimation allows the evaluation of the robustness of policy recommendations toward changes in parameter values within empirically relevant ranges.1

A second potential problem is to evaluate how “structural” the structural parameters are. In particular, I am worried about the Calvo parameters determining the fraction of firms not setting prices optimally and labor markets not setting wages optimally each quarter. The reason why we model price and wage stickiness with a time-dependent model like Calvo’s (or alternative Taylor pricing) is tractability. Exogenous and staggered timing allows for simple aggregation of individual pricing policies. This aggregation facilitates the computation of equilibria with standard methods. However, time-dependent models lack microfoundations. Why are firms allowed to change prices only in a fraction \( \alpha \) of quarters?
The typical answer to this lack of microfoundations is to defend time-dependent models as a simpler version of a more complicated, state-dependent pricing model, where firms change prices endogenously subject to a menu cost. The hope is that, in a low-inflation economy like the United States, the difference between time-dependent and state-dependent models will be small. This is, for example, the finding of Klenow and Kryvstov (2005), who estimate that the intensive margin of price changes (the average size of the change) accounts for 95 percent of the variance of monthly inflation, while the extensive margin (the fraction of items changing price) accounts only for 5 percent. Since the time-dependent models can account for the intensive margin, Klenow and Kryvstov (2005) conclude that these models do a fair job of capturing inflation dynamics.

However, Klenow and Kryvstov study 1988–2003 data, when inflation was low and stable in the United States. In that environment, there is bound to be too little variation in the data to tell time-dependent and state-dependent models apart. If we want to evaluate alternative monetary policies, we would need to know that both types of models are still going to be close under the different policy regimes.

Unfortunately, the answer may be negative. We know that state-dependent models often have radically different implications than time-dependent models. Caplin and Spulber (1987) present the most radical example. Under certain assumptions about the money process and price distribution, Caplin and Spulber find that money is neutral. More recently, Dotsey et al. (1999) have presented a dynamic equilibrium model that shows how the patterns of price adjustment in a state-dependent model change dramatically when systematic policy changes. Based on Dotsey et al.’s results, I find it difficult to accept that the Calvo parameters of SM are really “structural.” It is more plausible to assume that firms will change their prices more often if we implement a policy regime with a higher variance of inflation.

This observation has implications for policy recommendations. Chari et al. (1995) show that, in the context of a flexible-price model, the optimal rate of inflation volatility is extremely high, above 10 percent a year. The reason is that unexpected variations in inflation make nominal assets state-contingent in practice. In SM, this desire for inflation volatility is overcome by the cost of price dispersion induced by nominal stickiness. But if the government implemented a high volatility inflation policy, firms would adjust their prices much more often,
and we could get a reversal of SM’s results back to Chari et al.’s results. I emphasize the word could. Maybe price stability would still be a prime goal of monetary policy in a state-dependent pricing model. But I would like to know how many of SM’s results survive in a state-dependent pricing environment.

Evaluating the Results

SM do a fantastic job of presenting the results of the paper. Thus, I will not repeat them here. As I mentioned in the introduction, I am very happy with the main results: low and stable inflation, a large subsidy on capital, smooth taxation, etc. But the fact that I am so sympathetic to the results worries me a little. Either I have a special sense for policy recommendations—something I doubt—or we may have committed the sin of “specification searches” (Leamer 1978).

I have always feared that CEE may have already incurred that problem when they estimated their VAR: the results are sometimes too close to what our educated intuition tells us they should be. But how do we know that our educated intuition was right to begin with? Similarly, the different ingredients added by SM deliver answers that beautifully fit with what we thought was a sensible policy. Of course, CEE and SM could argue that their assumptions are explicit and that we can always check their robustness. Since CEE and SM are probably right, I do not want to further elaborate on the problem of specification searches but rather leave it behind us as a phantom menace.

The two results I want to discuss in more detail are the low inflation and the large subsidies to capital. SM defend a very low rate of inflation (0.2–0.5 percent). However, most central banks target, explicitly or implicitly, higher levels (1–3 percent). What is the source of this divergence? There are different alternatives. One is that SM have missed an important margin. For example, central bankers are awfully worried about the possibility of financial meltdowns. They will even claim that avoiding those meltdowns is their foremost job. SM’s model does not capture any of these systemic problems and, consequently, it may miss the channel for this higher inflation. However, it is not clear why a little bit higher inflation may avoid financial meltdowns. Because it allows policymakers to engineer negative real interest rates? A second possibility is that central banks are too liberal. However, accusing central banks of leniency seems a bit dramatic (although it has been done; see, for example, Cukierman 2002). Finally, we can argue that the
objects of theory and measurement are different possibly because of
the bias in our indexes of inflation induced by quality improvements
(see Bils and Klenow 2001).

The second result I want to comment on is that SM find that large
subsidies to capital are optimal. We do not observe these large sub-
sidies in the real world and proposing such a scheme would probably
seem too radical to have a realistic chance of being accepted by our
current political system. Again, we have different possibilities. First,
SM could have missed an important margin. The lack of commitment
of governments I discussed above is the usual suspect, but there may
be others. For example, Aiyagari (1995) links a positive tax on capital
income and incomplete markets. In this environment, a positive tax on
capital income reduces the overaccumulation of capital created by bor-
rowing constraints. Second, the perceptions of the public and politi-
cians could be very far away from sound policy recommendations.
The attitudes of the general population toward international trade are
probably a good example of how there can exist dissonances between
the general opinion of economists and the intuition of the representa-
tive agent. Finally, maybe there is a good political-economic reason
precluding a move toward subsidies for capital (Heathcote 2005).

Concluding Remarks

I conclude by emphasizing once more the importance of this paper. SM
have offered us a synthesis of what was known plus many new results
on optimal policy design. At the moment, this paper represents the
frontier of the field of policy analysis in equilibrium models of the busi-
ness cycle. I have discussed several aspects of the paper where I see
room for improvement, but none of my comments should be construed
as diminishing the importance and beauty of SM’s work, only as
pointers for follow-up articles.

There is one more great thing about this paper. If the reader goes to
the companion web page of the paper at http://www.econ.duke.edu/
~uribe/research.html, he or she can find an expanded version and the
MATLAB code required to run the experiments in the paper. Even if
SM do not want to brag too much about it, a final contribution of the
paper is a set of numerical tools to compute Ramsey policy problems
in a general class of dynamic equilibrium economies. I would encour-
age all interested readers to visit the web page and experiment with
the code themselves.
Endnote

1. There is, however, one difficulty hidden in the closet of estimation of dynamic models. How do we estimate government policy? Do we estimate its whole strategy? Or do we specify simple rules like the ones proposed by SM? But are these rules similar to anything the government follows? Can we use them for counterfactuals? I will not discuss this issue further since it will take me away from the main theme of my discussion.

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