Comment

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1 Introduction

In their contribution to the present volume, Davig and Leeper (henceforth, DL) study the implications of variations over time in policy rules. More specifically, they analyze the equilibrium effects of exogenous random switches in the coefficients of monetary and fiscal policy rules, embedded in an otherwise conventional dynamic optimizing model with staggered price setting. Their motivation for the exercise is an empirical one: They estimate a Markov switching model for the two policy rules and find evidence of recurring changes in those coefficients. Most interestingly, the estimated changes in the policy rules involve "qualitative" changes in the nature of the regime in place, i.e., they imply a shift from an "active" to a "passive" monetary policy (or vice versa), as well as analogous (but not necessarily synchronous) shifts in the fiscal policy rule. When DL embed their estimated monetary and fiscal Markov switching processes in a calibrated new Keynesian model and analyze the implied equilibrium properties, they uncover a number of interesting results, some of which are summarized below.

Before we turn to some specifics of their analysis, I think it is important to stress the central, more general message of the DL exercise: Once we accept the possibility of a change in the policy regime (and the recognition of that possibility by agents in the model as a logical implication of the rational expectations assumption), a conventional fixed-regime equilibrium analysis, i.e., one that treats the regime in place *as if* it were to persist forever, may be highly misleading. The fact that the fixed-regime assumption is common-place in the macroeconomics literature is somewhat paradoxical, since one of the main stated objectives for the development of current generation of microfounded DSGE models was *precisely* to analyze the implications of policy regime changes.

That general message of the DL paper is illustrated by some of their results. Here are, in my opinion, the most significant ones:

- An equilibrium may exist and be unique even under a "doubly passive" or a "doubly active" policy regime, i.e., regimes which would imply, respectively, an indeterminate equilibrium or the non-existence of a stationary equilibrium, when modeled "as if" they were permanent. Thus, for instance, the empirical violation of the Taylor principle in the pre-Volcker era detected by several authors (including DL in the present paper) does *not* necessarily imply that the equilibrium in that period was indeterminate or subject to potential sunspot fluctuations, even if fiscal policy was simultaneously passive.
- Fiscal deficits resulting from changes in lump-sum taxes may be nonneutral, *even under a passive fiscal policy regime.* In other words, and using the authors' language, the mechanisms underlying the fiscal theory of the price level may be effective (to a lesser or greater degree) at all times. Equivalently, Ricardian equivalence may not hold even if the conditions under which it has been shown to hold (in a fixed-regime world) are operating in any given period.
- The dynamic effects of any shock that occurs when a given regime is in place are *not* invariant to the characteristics (or the likelihood) of other possible future regimes. It follows that the use of estimated impulse responses for the purposes of calibration of "fixed regime models" may be unwarranted, even if those impulse responses are estimated using data from a "stable regime" period.

All of those findings share a common feature, which DL refer to as *cross-regime spillovers:* The equilibrium properties of an economy under any given regime are "contaminated" by the characteristics of the other possible regimes and by the probability distribution describing the shifts in regime. In other words, once we admit that policy regimes are subject to change, a description of the current policy regime is not sufficient to characterize the equilibrium dynamics of the economy under that regime. One needs to know all possible regimes and the probability distribution describing the shifts among regimes over time.

Given the forward-looking nature of the models involved, combined with the assumption of rational expectations, that result may not be that surprising after all. But the fact that such a result is not surprising does not mean that it is not important or useful. In some sense it takes the logic of the Lucas' critique to a higher level: The properties of the equilibrium are shown to be a function of the "meta-regime" in place. As far as I know, DL are the first to analyze this phenomenon explicitly in the context of a modern, quantitative macro model.

The rest of this comment raises two caveats on DL's paper. The first has to do with the approach followed in analyzing the uniqueness of the equilibrium. The second deals with the empirical relevance of the assumption of recurring regimes.

2 Determinacy Analysis

One of the most striking findings in DL's paper is the claimed coexistence of a unique stationary equilibrium with periods characterized by "doubly passive" and "doubly active" policies. Unfortunately, as the authors themselves acknowledge, no formal proof of that claim is provided in the paper. Instead, it is based on the convergence of a numerical algorithm that searches for decision rules consistent with equilibrium conditions. The postulated rules contain a minimum set of state variables as arguments, but that set does not allow for "redundant" state variables, including sunspots. It is thus not obvious that the mere convergence of the algorithm to a set of decision rules guarantees that those rules are the only ones consistent with equilibrium. The authors' finding of algorithm divergence when solving for the equilibrium under a *fixed* PM/PM regime known to imply indeterminacy offers some comfort, but is no definitive proof.

An alternative approach, pursued by the authors in a companion paper in the context of a simpler model (Davig and Leeper 2005), involves log-linearizing the equilibrium conditions and determining whether the resulting Markov-switching model satisfies the analytical conditions for stationarity established in the relevant literature (see, e.g., Francq and Zaqoïan 2001). Let me illustrate that analytical approach (as well as a potential caveat) using a simple univariate example.

Suppose that the condition describing the equilibrium behavior of variable x_t is given by the expectational difference equation

$$E_t\{x_{t+1}\} = \phi_t \, x_t \tag{1}$$

where coefficient ϕ_t is possibly time-varying and where, for simplicity, we ignore the presence of a fundamental driving force. A stationary solution to the above equation always exists, and is given by $x_t = 0$ for all t. The condition for uniqueness of that stationary solution for the case of a constant AR coefficient ($\phi_t = \phi$ for all t) is well known:

The above solution is the only one that remains in an arbitrarily small neighborhood of the steady state whenever $|\phi| \geq 1$. If instead we have $|\phi| < 1$ we have an additional set of stationary solutions of the form

$$x_{t+1} = \phi x_t + \xi_{t+1}$$

where $\{\xi_t\}$ is an arbitrary random process (a "sunspot") satisfying the martingale-difference property $E_t\{\xi_{t+1}\} = 0$ for all t.

If we assume instead a Markov process for the AR coefficient ϕ_t things change considerably. For the sake of concreteness, let us assume a two-state process $\phi_t \in \{\phi_L, \phi_H\}$ where $0 < \phi_L < 1 < \phi_H$ and where the transition matrix is given by

$$P \equiv \begin{bmatrix} p_L & 1-p_L \\ 1-p_H & p_H \end{bmatrix}.$$

Any potential sunspot solution to (1) takes the form

$$x_{t+1} = \phi_t x_t + \xi_{t+1} \tag{2}$$

where $E_t\{\xi_{t+1}\} = 0$. Furthermore, and under our assumptions, that solution is generally taken to be an admissible equilibrium if it is stationary. Francq and Zaqoïan (2001) derive necessary and sufficient conditions for stationarity of Markov-switching ARMA processes of which (2) is a particularly simple case. Their condition implies that (2) may be nonstationary even if $\phi_L < 1$ (i.e., even if solution (2) would be stationary in the case of a fixed regime with $\phi_t = \phi_L$ for all t). Roughly speaking, this will be the case whenever ϕ_H is sufficiently larger than one and when the system spends enough time under the ϕ_H regime. In that case, solution $x_t = 0$ for all t will be the only stationary solution even if ϕ_t recurrently takes a value less than one.

The previous result corresponds to DL's claim that their model's equilibrium may be locally unique even if, recurrently, a regime characterized by passive monetary policy and passive fiscal policy becomes effective. Their finding thus seems consistent with analytical results from the literature on Markov-switching processes. One would feel more confident about DL's uniqueness result if the latter was cross-checked using the analytical conditions derived in that literature.

That confidence may, however, be unwarranted in light of the findings of a recent paper by Farmer, Waggoner, and Zha (2006; FWZ, henceforth). FWZ show that a regime-switching expectational difference equation may have a multiplicity of solutions as long as one of the recurrent regimes implies such a multiplicity when considered in isolation, and as long as the economy operates under that regime a sufficiently large fraction of time. That result holds independently of the value taken by ϕ_H. For the particular case of the simple univariate model (1) above, the FWZ solution takes the form

$$x_t = 0 \quad \text{if} \quad \phi_t = \phi_H > 1 \tag{3}$$

$$x_t = \frac{\phi_L}{p_L} x_{t-1} + \gamma_t \quad \text{if} \quad \phi_t = \phi_L < 1$$

where $\{\gamma_t\}$ is an arbitrary *exogenous* martingale-difference process. Notice that $\{x_t\}$ reverts back to the steady state recurrently, with probability one as long as $p_H < 1$. Furthermore, as shown by FWZ the assumption $|\phi_L| < \sqrt{p_L}$ is sufficient to guarantee stationarity of the global solution. Hence multiplicity of stationary equilibria appears to arise for a broad range of parameter values, as long as a regime with $\phi_L < 1$ emerges recurrently. Whether a version of the FMZ result carries over (at least locally) to a non-linear model, like the one considered by DL in the present paper is not clear. If it did, one of the key findings of the DL paper, which currently relies exclusively on the convergence of a numerical algorithm, would unfortunately turn out to be wrong.

How can one reconcile the FMZ finding with the *possibility,* under certain conditions, of a unique equilibrium, as implied by the Francq and Zaqoïan (2001) result discussed above? My conjecture is that the analysis in the latter paper (and in the related literature) requires that the error term in the regime-switching process (2) is truly exogenous (as assumed in conventional ARMA models). By contrast, the FMZ solution (3) implies

$$\xi_t = -\phi_{t-1} x_{t-1} \quad \text{if} \quad \phi_t = \phi_H > 1$$

$$\xi_t = \left(\frac{\phi_L}{p_L} - \phi_{t-1} \right) x_{t-1} + \gamma_t \quad \text{if} \quad \phi_t = \phi_L < 1$$

Notice that the previous $\{\xi_t\}$ process satisfies the martingale difference property $E_t\{\xi_{t+1}\} = 0$, but it is *not* exogenous, depending instead on lagged values of ϕ_t and x_t, as well as on the exogenous sunspot shock γ_t. Note that this kind of solution is not allowed for by DL's solution method, and it is also inconsistent with regime-switching models driven by exogenous shocks.

3 Empirical Relevance

In the introduction to their paper, DL point to the assumption of a fixed policy regime commonly made in modern analyses of fiscal and monetary policy as possibly being the least plausible among the many assumptions underlying that literature. In spite of that, there are many reasons for the prevalence of that assumption: It is convenient, it has a long tradition in economic theory (e.g., in the literature on the effects of capital income taxation), it allows for comparative dynamics exercises, and it facilitates the evaluation of a model's predictions. DL's analysis, however, emphasizes an important shortcoming of the fixed-regime fiction: The fact that it assumes away the possibility of cross-regime spillovers.

Of course, one may find DL's case for an explicit modeling of the possibility of regime changes fully persuasive without necessarily sympathizing with the specific model of regime changes postulated in the paper. i.e., one characterized by exogenous, *recurrent* switches between a finite number of policy regimes. Given that any two different policy regimes are likely to be rankable in terms of their desirability, it is hard to understand why policymakers would periodically switch to the least desirable of those regimes. Furthermore, the exogenous nature of those switches represents a renewed emphasis on *policy randomization*, away from the emphasis on the endogenous component of policy found in the recent literature.

While few economists would question the empirical relevance of regime change, I conjecture that most would view non-recurrent changes as more likely. Two examples of relevant non-recurrent regime changes come to mind:

- Anticipated "permanent" regime changes: including a stabilization program aimed at ending high inflation, or the abandonment of an unsustainable exchange rate peg
- A gradual variation in the policy regime, resulting either from learning (in an unchanged environment) or from adjustment of optimal responses to changes in the environment.

Any rational expectations model that incorporates the possibility of regime changes of that kind is likely to display the central property of DL's model, namely, the presence of cross-regime spillovers, without having to rely on the less plausible notion of recurrence.

4 Concluding Comments

DL's paper is ambitious and important. Taking it seriously leads to questioning some results previously thought of as well established (e.g., the need to satisfy the Taylor principle in order to guarantee a unique equilibrium). Unfortunately, one key result in the paper (the global uniqueness of the equilibrium in DL's calibrated model) has not yet been established in a rigorous way. That notwithstanding, the importance of cross-regime spillovers emphasized by the authors is somewhat orthogonal to the issue of indeterminacy and is likely to be relevant even in the context of switches among regimes which, when considered in isolation, are associated with a unique equilibrium. Similarly, the significance of those cross-regime spillovers does not hinge on the questionable Markov switching formalism adopted to characterize regime change in the present paper. In my opinion, much of the value added in DL's paper and the significance of their contribution lies in providing a useful illustrative model of the potential importance of cross-regime spillovers, rather than a model that one should take seriously as a description of post-war U.S. fluctuations and its sources.

Acknowledgments

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