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Discussion

Comment on Argia M. Sbordone "Inflation persistence: Alternative interpretations and policy implications"

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Let me begin with a summary of this interesting paper, before giving some comments.

The paper builds on Sbordone's earlier work with Tim Cogley (Cogley and Sbordone, 2005, 2006—referenced as CS hereafter). In that work, CS developed a macro-model with a time-varying inflation trend. As in a familiar version of Calvo pricing with indexing, CS assumed that firms that do not reoptimize partially index to the previous period's inflation rate. It is standard to log linearize around a zero steady-state inflation rate. This would lead to a linear equation with a lag of inflation in the Phillips curve (see, e.g., Woodford, 2003):

$$\pi_t = \gamma_b \pi_{t-1} + \zeta s_t + \gamma_f E_t \pi_{t+1} + u_t, \tag{2}$$

where s_t is the marginal cost; $\gamma_b = 0$ and $\gamma_f = \beta =$ household discount factor if there is no indexation; u_t is a cost-push shock. (Equation numbers match those in Sbordone's paper.)

CS depart from the standard setup by assuming that trend inflation follows a driftless random walk. It is no longer meaningful to linearize around a zero steady-state inflation rate. Instead, the linearization is done around the time-varying trend. The end product is a Phillips curve with time-varying coefficients and an additional term:

$$\hat{\pi}_t = \tilde{\rho}_t \hat{\pi}_{t-1} + \zeta_t \hat{s}_t + b_{1t} E_t \hat{\pi}_{t+1} + \text{another forward looking term} + u_t, \tag{3}$$

where $\hat{\pi}_t = \ln(\text{gross inflation/trend inflation})$ and similarly for $\hat{s}_t$. Note the t subscripts on $\tilde{\rho}_t$, ζ_t and b_{1t}. Empirically, the backwards term $\tilde{\rho}_t \hat{\pi}_{t-1}$ was found to be of minor

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consequence and hence is dropped in the analysis conducted in the present paper. This results in

$$\hat{\pi}_t = \zeta_t \hat{s}_t + \Phi_t E_t \hat{\pi}_{t+1} + \text{another forward looking term} + u_t. \quad (5)$$

Sbordone puts (5) to work in the analysis of (a) inflation persistence (Section 3 of her paper), and (b) welfare losses under misspecification (Section 4).

(a) *Inflation persistence*. Suppose the truth is the forward-looking Eq. (5), but we estimate (2). Then simulations indicate that we are quite likely to wrongly conclude that there is a backward-looking component to inflation (i.e., $\hat{\gamma}_b$ is quite likely to be positive). See Table 2 of Sbordone's paper. Hence, empirical results supportive of (2) are not inconsistent with (5).

(b) *Welfare losses under misspecification*. Suppose again that the truth is the CS Phillips curve (5), but the policy maker assumes the constant parameter hybrid Phillips curve (2)—or vice versa. How wrong will we go? This is analyzed by looking at the arithmetic increase in the following loss function: hold fixed the model used by the policymaker (either constant parameter or CS), and compare losses when the policymaker is wrong relative to when the policymaker is right. Under both ad hoc and welfare maximizing loss functions, this increase tends to be bigger when the policymaker assumes constant parameters than when the policymaker assumes CS. Hence, the CS model is interpreted as being a safe choice: it is less costly to mistakenly assume CS than to mistakenly assume hybrid.

So much for my summary. There were parts of the paper I quite liked, and some parts that I thought could have been better developed.

I applaud the goal of allowing nonzero trend or steady-state inflation. No matter how one defines trend inflation, it is hard to argue that the trend inflation has been zero over the past decades, even if one allows for a (say) 1% upward bias in the measured inflation rate. Ascari and Ropele (2006) consider the case of a constant but nonzero steady-state inflation rate, in a purely forward-looking model. They argue that allowing for small but reasonable values of the trend rate of inflation in an otherwise standard calibration has significant implications for stability and for policy evaluation. Hence, it appears to be important to allow for nonzero steady-state inflation.

I also applaud the goal of allowing for changes in steady-state inflation: the sustained rise and then sustained fall in inflation over the last 40 years certainly supports a view of the world in which perceptions of steady-state inflation change over time. Finally, I commend the paper for deriving behavioral equations that carefully respect time variation in trend inflation. The techniques used in this and related background papers are of general interest. The logic of these papers suggests, for example, that in the presence of a nonzero steady-state inflation rate, one needs to adjust the specification of wage setting equations, in models with wage stickiness.

It is not clear to me, however, that a random walk is a good model for trend inflation in the context of a monetary model such as Sbordone's. On the one hand, models with random walk parameters have been successfully used in some reduced form studies, including Cogley and Sargent (2005) and Stock and Watson (2007). As well, related studies have found improved fit when one allows for a near random walk in the Fed's inflation

target (e.g., Gürkaynak et al., 2005). On the other hand, a random walk model for trend inflation has technical and economic drawbacks as well. Let me comment on each in turn.

At a technical level, a random walk is often a simplifying assumption in a reduced form model with time-varying coefficients. But it makes life difficult in the present context. For example, the paper solves the forward-looking model assuming static (rather than model consistent) expectations. The random walk does not seem to me to be obviously simpler or more appealing than allowing for occasional changes in target/trend inflation (perhaps Markov switching, as in, e.g., Davig and Leeper, 2005, 2007).

And if there is an economic rationale for the random walk, that rationale is not spelled out in the present paper (though the paper does reference literature that ties the movements in trend inflation to the Fed's inflation target). The paper takes as given that central aspects of macro-data are driven by an exogenous, serially correlated variable (trend inflation). As such, the paper falls in the camp that relies on exogenous rather than intrinsic sources of inertia. In related literature, this means relying on a serially correlated shock to natural rate of interest rather than habit persistence, or serially correlated cost-push shocks rather than indexation or rule of thumb behavior.

Now, there is a large literature that says the two stories may be hard to distinguish in a given sample (e.g., English et al. (2003) on serial correlation in an estimated US Taylor rule). It is interesting that Sbordone finds that serially correlated trend inflation will likely be rationalized as backward-looking behavior, if one wrongly assumes backward-looking behavior. But I think the fundamental question is whether Sbordone's analysis argues for preferring the CS model to alternative models, such as models with backward looking components in the Phillips curve and/or ones with regime shifts. And in my view, a lot more econometric and economic evidence needs to be adduced before we can offer an answer to that question. For example, one might turn the exercise in Section 3 around: generate data under a model with a backward-looking component, or with one or two regime shifts, and see whether estimates of the CS model have high probability of spuriously finding a trend to inflation.

More fundamentally, one would want to evaluate the times series for trend inflation in the light of a story, or model, involving economic behavior. Sbordone quite reasonably states that trend inflation is under the control of policymakers. She also quite reasonably points out that endogenizing trend inflation will be difficult. Nevertheless, there are intermediate steps that can be taken while working towards formal modeling of the link between policymaker behavior and trend inflation. For example, is there any indirect or corroborating evidence that the movements in the constructed series predominantly reflect decisions of policymakers? And insofar as policymaker decisions do drive the series, is the variation due to changes in Taylor rule feedback coefficients, to changes in target inflation, or to other shifts?

These are the sorts of questions that I hope Sbordone will address in her future research.

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