Wrap-Up Discussion

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My summary remarks are organized around three basic questions: (1) Should the Bank target inflation or the price level? (2) What is the role of the monetary conditions index? (3) What is the optimal inflation rate?

1   Targeting Inflation or the Price Level

The first and the last papers, by Coulombe and by Black, Macklem, and Rose, respectively, address the relative merits of policies that define the price-stability objective in terms of either a specific inflation rate or a target for the price level. The former policy allows a one-time increase in the price level to remain, while in the latter, the central bank dampens demand by whatever it takes to pull the price level back down to its target. One-time shocks are treated as bygones under inflation-rate targeting, but their effects are eventually eliminated when the price level is the specified goal. This way of characterizing the choice suggests that short-run macroeconomic performance may be less favourable under price-level targeting. For example, with sticky prices, output may have to be pushed below potential output for a prolonged period to completely eliminate the effect of a one-time adverse supply shock.

One purpose of Coulombe's paper is to show that the economy's built-in stability features may be enhanced, not lessened, by a shift to price-level targeting. The debate can be highlighted by reference to the following highly simplified perfect-foresight model:

$$y = \alpha(m - p) + \beta\pi + \gamma g \tag{1}$$

$$\pi = \pi^* + \delta(y - y^*) \tag{2}$$

$$m = -\theta(\pi - \pi^*) \tag{3a}$$

$$m = -\lambda p. \tag{3b}$$

The variables *m, p, y, y*,* and *g* are the natural logarithms of the nominal money supply, the price level, real output, the natural rate of output, and autonomous real spending. The variables π and π^* are the levels of the inflation rate (the time derivative of *p*) and its target value. The remaining Greek letters denote positive slope parameters. The first equation defines aggregate demand, which depends negatively on the price level, and positively on the expected (that is, actual) inflation rate. Equation (2) describes aggregate supply considerations as summarized by an expectations-augmented Phillips curve. It is assumed that average inflation is what concerns agents in setting wages and prices.Two possible monetary policy reaction functions are specified in equations (3a) and (3b). In the first, the money supply departs from its long-run average value (which I take to be zero) whenever the inflation rate differs from its target value (which is zero as well). In the second reaction function, the money supply is above (below) its long-run average value whenever the price level is below (above) target.

As an example shock, consider fiscal retrenchment, in the form of an unexpected but permanent decrease in autonomous spending. With inflation targeting, the cumulative output loss that accompanies this event is given by the expression $\gamma/(\alpha\delta)$, and the system converges to its full equilibrium if $\beta\delta < (1 + \alpha\theta\delta)$. With price-level targeting, the cumulative output loss is $\gamma/(\alpha\delta(1 + \theta))$, and stability requires that $\beta\delta < 1$.

The fact that (in this model) it is harder for the stability condition to be satisfied in the second case is the analytical counterpart of the intuition that macroeconomic adjustment may be more protracted when the economy is pushed through what it takes to avoid letting bygones be bygones. But the fact that the same average inflation rate (zero) is achieved with a smaller output loss in this case is the analytical counterpart of the favourable expectations effect that Coulombe stresses in his paper. In the present model, price-level targeting means that the central bank responds more sluggishly to fiscal retrenchment than it does when the inflation rate is targeted. This property follows from the fact that the inflation rate responds immediately to variations in output, while the price level is predetermined at each point in time. As a result, with a price-level target the fiscal retrench-ment produces a bigger impact multiplier but a faster recovery, so fiscal retrenchment involves a deeper recession, but one that is eliminated more quickly relative to the case with an inflation target. The fact that the cumulative output loss is smaller with a price-level target indicates that we may prefer to switch to price-level targeting despite the trade-offs involved, as the results in Black, Macklem, and Rose suggest. We need more extensive

modelling of this question, which highlights (in particular) how results may depend on whether the central bank achieves its short-run target exactly, or just in expectational terms. The highly simplified but entirely standard model I present here is offered as a vehicle for appreciating some of the intuition behind this issue, and for arguing that the discussion following Coulombe's paper tended to underrate the costs involved in acquiring a favourable expectations effect.

I like the examination in Black, Macklem, and Rose of the exploitable trade-off between price and output volatility. All aspects of specification and execution in their study are impressive. It is appropriate that the possible non-linearity of the short-run Phillips curve is stressed, although alternative ways of specifying this non-linearity are possible. Consider the following Phillips relationship:

$$\pi = \pi^* - \varphi(u - u^*), \tag{4}$$

where π and π^* are actual and expected inflation, and u and u^* are the actual and natural rates of unemployment. Consider also the following function for the slope of the short-run Phillips curve:

$$\varphi = \varphi^*\exp(-\kappa\bar{\pi})u^{-\eta},$$

where φ^* is a constant and $\bar{\pi}$ is the average inflation rate. The short-run Phillips curve is linear if $\kappa = \eta = 0$. If $\kappa = 0$ but η is positive, there is a convex Phillips curve, the average unemployment rate exceeds the deterministic natural rate (in a stochastic setting), and the magnitude of this excess is independent of the mean inflation rate. If both κ and η are positive, however, the excess of the average unemployment rate over the natural rate rises as the average inflation rate falls. Black, Macklem, and Rose's specification is equivalent to $\kappa = 0$. I encourage the authors to consider generalizing in this regard. I consider the thorough study by Dupasquier and Ricketts as supporting the suggestion that it would be prudent to experiment with $\kappa > 0$ in Black, Macklem, and Rose's future simulations.

I never like proposing more work for those who have already done so much (in both quality and quantity), but in future studies I hope these authors can explore Taylor rules—implemented in a forward-looking fashion—in both rate-of-change and level formats. Finally, I suggest that they include fixed exchange rates as one of the options they explore. A fixed exchange rate (in the form of currency union) is a credible policy option, and the standard presumption that a flexible exchange rate acts as a built-in stabilizer is not always supported when supply-side effects of exchange rates and exchange-rate overshooting are involved. It may be unappealing to adopt the U.S. inflation rate if we believe that the upward bias in measured

U.S. inflation is higher than the bias in the Canadian consumer price index. The paper by Crawford, Fillion, and Laflèche suggests that the bias in the U.S. measure (approximately 1 percentage point on an annual basis) may be double the measurement bias in Canada. Since this difference is not too large, however, the fixed-exchange-rate option should remain within the basic research studies conducted at the Bank.

2   The Monetary Conditions Index

The Bank's use of the monetary conditions index (MCI) drew a lot of attention during the round-table discussion involving Sherwin, Allen, and Heikensten, and also during Bank of Canada Governor Thiessen's remarks at dinner. A common theme was that monetary policy should be predictable and transparent. It was felt that credibility is enhanced as a result. Does using an MCI help in this regard?

As has been stressed, neither the level of nor the change in the MCI is a useful indicator of the stance of monetary policy. It is the gap between the actual and the "desired" levels of the MCI that may be useful. For example, if fiscal retrenchment were expected to make aggregate demand "too low" to maintain a constant price level, the "desired" MCI would be adjusted down. Then, if the actual MCI were to stay constant, a monetary policy that was initially appropriate would be reassessed as "too tight"—despite the fact that the MCI had not changed. If the Bank really wants to be more transparent, it has to publish both the actual and its "desired" values of the MCI.

The Bank may not wish to give more emphasis to the MCI in its reports because of the limited empirical support for the specifics of its calculation. As Eika, Ericsson, and Nymoen (1996) have shown, the confidence bound that we have for the three-to-one ratio of the coefficients on the interest rate and the exchange rate in the determination of aggregate demand is quite discouraging. Further, since Bank officials tend to stress direct supply-side effects for the exchange rate but not for the interest rate, it is not clear that the MCI—based as it is on just the relative strength of the demand-side effects of these variables—should be given too much focus.

One might claim that using the MCI allows the Bank to explain better what it is doing, and thus enhance its credibility. Since Johnson has estimated that the Bank of Canada now has credibility, however, the MCI may not be needed from this standpoint. In the end, it appears that Bank officials find the MCI useful as a short-term instrument, even if some of those who monitor the Bank do not find that reference to it has increased the transparency of the policy-making process.

3 The Optimal Inflation Rate

The remaining three papers investigate different aspects of the benefits and costs of price stability. On the benefit side, authors have cited the more efficient use of money and the decrease in the distortions that accompany the interaction of inflation and a non-indexed tax system. On the cost side of the ledger, attention has been focussed on output losses, and on what I like to call the "Linda McQuaig effect"—the impact of disinflation on the government debt ratio.

Ambler and Cardia use a calibrated general-equilibrium model to explain the unreliability of reduced-form regressions that purport to illustrate an exploitable relationship between the average inflation rate and the rate of economic growth. Devereux's scatter diagrams dramatically support this scepticism. Especially if we consider only relatively low-inflation countries, we find only very weak evidence in favour of a significant inverse correlation between inflation and real growth. It seems almost beside the point to consider such issues as whether data based on 25- to 30-year averages can approximate steady-state outcomes, and whether an inverse relationship between inflation and real growth is structural or not.

I prefer to ignore the estimated regressions linking inflation and growth, and to use calibrated models (such as that provided by Ambler and Cardia) to illustrate the orders of magnitude involved. They find that a reduction of inflation by 1 percentage point raises the real growth rate by just less than one-fifth of a percentage point. This is much larger than some other estimates. For example, Dotsey and Ireland (1996) report one-200th of a percentage point, and one might expect that their study involves particularly large supply-side effects. After all, in another paper, Ireland (1994) uses a calibrated endogenous growth model to suggest that the revenue-maximizing tax rate is 15 per cent—not the 60 per cent that others have reported in similar studies. This debate reminds me of Solow's quip that "there is nothing wrong with supply-side economics that dividing by 10 could not cure" (Solow 1996, 298). The above illustrations suggest that a correction factor even more dramatic than Solow's may be appropriate concerning the role of inflation.

Howitt's (1997) reaction to this controversy is compelling. He takes a small estimated effect of lower inflation on the real growth rate, lets that effect cumulate for 30 years, and then assumes that the uncertainty surrounding this issue forces us to ignore any further effect on the growth rate. Thus, he ends by assuming that the level of output per head (not its growth rate) is higher by one-half of 1 per cent as a result of a 1 percentage point reduction in inflation. This is a prudent way of proceeding.

I applaud the survey on the benefits and costs of disinflation provided by Black, Coletti, and Monnier. As they argue, their ability to use such a survey to provide a reasonably small range of estimates (in which we can have confidence) requires that a few "outlying" studies be excluded. I agree, but I am concerned that a few rather central studies have been excluded as well. For example, Cooley and Hansen (1991), Braun (1994), and Black, Macklem, and Poloz (1994) all show that the optimal inflation rate can be positive. The basic idea is that, with distorting taxes, inflation raises more revenue than just pure seigniorage. With lower inflation, a significant amount of revenue is lost, so that disinflation must be accompanied by increases in some other (distortionary) taxes. If inflation is pushed below the positive level that minimizes overall distortion losses, then the "correction factor" that is needed in the benefit-cost tabulation is not just Solow's fraction—it may even be a negative number. Of course, important biases could run in the other direction. For example, a cash-in-advance constraint may be an incomplete vehicle for capturing the costs imposed throughout the economy because our accounting system is made imperfect by inflation. My general message is that a little more prudence could be built into the summary tables in this survey.

Regarding the costs of inflation, I am pleased that these authors are taking into consideration the lasting effect on international indebtedness of the temporary recession that accompanies disinflation. Black, Coletti, and Monnier estimate that 20 per cent of the benefits of disinflation are eliminated by this monetary-policy-induced increase in the ratio of debt to gross domestic product. The Bank's credibility is greatly enhanced when its researchers respond constructively to criticisms.

The other costs of disinflation follow from the unemployment that results—that is, both the temporary rise in unemployment that occurs if the standard version of the natural rate model is appropriate, and the permanent increase in average unemployment that follows if any of the hysteresis, non-linearity, or nominal-wage-floor hypotheses apply. Black, Coletti, and Monnier make a generous allowance for hysteresis effects; in future work I hope they can add illustrations of the potential size of the other two effects (and explicitly compare their overall results with the set of calculations provided by Fortin 1997).

Perhaps further consideration of the non-linearity and wage-floor issues will force us to acknowledge more uncertainty regarding the optimal inflation question. It is daunting, after all, to try to deduce subtle differences in curvature from a set of time-series data. Furthermore, while Dupasquier and Ricketts' paper on non-linear aggregate supply relationships is well done, the authors acknowledge that more work is needed to test whether their conclusions are sensitive to such things as direct exchange rate effects

in the aggregate supply function. Also, alternative specifications concerning curvature of the demand-side relationships can be just as important for generating permanent effects on average unemployment (see Scarth 1996, 50-54).

While the applicability of the wage-floor hypothesis is difficult to pin down, the contribution of Crawford and Harrison is a major step forward. Of the increased bunching of wage increases at zero that has occurred recently, they have estimated how much could have been expected simply because the mean of the estimated wage distribution is now lower. They conclude that there is some "excess" bunching at zero, so there is some (but limited) evidence in favour of the proposition that inflation may "grease the wheels" of relative price adjustment. Extensions of this work are worthwhile and are proceeding. It is important, for one thing, to identify separately the impact of variations in productivity growth. Second, it is appropriate to focus on the entire lower tail of the wage-change distribution, not just the zero point. Finally, one can explore options for adapting the method of estimating wage distributions, so that data sets that involve a higher incidence of negative wage changes can be studied.

Conclusions

Part of a discussant's task is to highlight suggestions for further work that have been stressed during the conference. But it is also appropriate to acknowledge what has been accomplished. While we still do not fully understand either the benefits and costs associated with both moving to and maintaining any particular target for the inflation rate or the price level, or any particular short-run targeting strategy we might use, there is no doubt that the high quality of the presentations of all contributors to this conference have narrowed these gaps in our knowledge.

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