Discussion

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In October 1993, the Bank of Canada held a research conference on the economic implications of low inflation. In his introduction to the resulting conference volume, Stephen Poloz (then head of the Bank's Research Department) noted that at least two topics had been consciously omitted from the conference agenda: (1) measurement bias in the consumer price index, on which an extensive technical report had just been published (Crawford 1993); and (2) the empirical link between inflation and economic performance. On the second point, Poloz (1994, ii) wrote:

> There already exists a great deal of published work on this issue, and more recent contributions to this literature have shown that previous estimates of this link based on cross-country evidence are very fragile.

The conference volume was subsequently reviewed in the *Canadian Journal of Economics*, where Johnson (1995, 724) describes this second omission as

> what I feel is the biggest disappointment of the volume…. [T]his topic is much too important to be omitted; in the volume … Howitt (1990) [is quoted] as saying this topic "may be the most important issue of all." I agree.

Noting that other papers in the volume conclude that the welfare benefits of low inflation are potentially large because they *assume* that lower inflation raises labour productivity growth, Johnson (ibid.) goes on to say:

The link between inflation and productivity growth is too important not to be addressed directly in the volume; if the link is so fragile, its fragility should have been reported clearly, so that readers would have the information to assess its fragility.

He then concludes (ibid., 725-26):

If the current regime is permanent, we shall experience a long period of low inflation and discover if the benefits of low inflation are significant. Another conference could then investigate the issue and produce another set of papers.

How could anyone resist such an invitation? It should therefore come as no surprise that in this 1997 conference the Bank has consciously decided to examine this question, both in the Ambler and Cardia paper, which addresses the empirical literature, and in the Black, Coletti, and Monnier paper, which asks how large the long-run benefits of low inflation must be to justify the short-run costs of lowering inflation. Black, Coletti, and Monnier also survey some of the literature.

The intervening years have shown that this topic is far from exhausted, to judge by the number of publications on it. One year alone saw interesting papers on the subject by Sarel (1996), Barro (1996), Judson and Orphanides (1996), Hess and Morris (1996), Dotsey and Ireland (1996), and Cameron, Hum, and Simpson (1996), among others. If anything, publication rates on this topic may not yet have crested.

In approaching this still rapidly evolving literature, Ambler and Cardia chose not to give us a synthesis or a snapshot of the empirical evidence. Their paper is more ambitious; it tries to shape our thinking about the relationship between this empirical literature and the policy question at hand. What I think of as the most central of their many contributions can be summarized as follows:

- Estimates of the correlation between inflation and output growth will be a combination of a non-monetary effect and the effect (if any) due to a change in monetary policy. The relative weights of these two effects depend on the relative importance of monetary and non-monetary shocks in our data. However, only the effect due to monetary policy is relevant when central bankers are considering whether to pursue low-inflation policies. Put algebraically, when we regress output growth, $\dot{y}$, on inflation, π, we get

$$\dot{y} = \hat{\alpha} + \pi \cdot \hat{\beta} + \varepsilon, \text{ and} \tag{1}$$

$$\hat{\beta} = \gamma \cdot \beta_{\text{monetary}} + (1 - \gamma) \cdot \beta_{\text{non-monetary}} . \tag{2}$$

The challenge for those seeking to guide policy is to disentangle these two effects.

- In a world with exogenous money growth and stable velocity, non-monetary shocks create a strong negative correlation between inflation and real growth, so $\beta_{\text{non-monetary}} = -1$. To see this, recall the identity

$$MV \equiv PY, \text{ so } \dot{m} + \dot{v} \equiv \pi + \dot{y}, \tag{3}$$

where $\dot{m}$ and $\dot{v}$ are, respectively, the rates of change of the money stock, and velocity.

$$\text{If } \frac{d(\dot{m} + \dot{v})}{d\dot{y}} = 0, \text{ then } \frac{d\dot{y}}{d\pi} = -1. \tag{4}$$

In effect, this implies that policy is equivalent to a nominal income-targeting rule; since the growth rate of nominal income is fixed, it follows that higher real growth is associated with a one-for-one decrease in inflation.

- Ambler and Cardia's model generates a very slightly negative value for $d\dot{y}/d\pi$ when only monetary shocks are present.

- For the standard mix of shocks, it generates a negative relationship that is much stronger than that found in the data (-0.22 in the data versus -0.8 in the model).

To understand the relevance of their model for policy, I think we need to understand why it falls so far short of capturing what we see in the data. Put another way, the contentious issue may not be whether the correlation in the data is different from zero, but why it is not closer to -1. There are four possibilities to consider:

1. $\hat{\beta}$ is mismeasured;
2. γ is much larger than in their model (that is, monetary shocks are much more important);
3. β_{monetary} is a big positive number; and
4. $\beta_{\text{non-monetary}}$ is closer to zero.

With respect to point (1), Ambler and Cardia suggest that the Phillips curve could be an important source of the mismeasurement. Specifically, a short-run positive relationship between inflation and growth would tend to raise their estimated covariance of growth and inflation, since their estimate is based on unfiltered data.

Ambler and Cardia explicitly consider the possibility of point (2) as case 4 in their Table 2, where they set new parameters for the model with monetary shocks that are 10 times more important than in their base case.

Even under these extreme assumptions, the simulated β is, in absolute magnitude, roughly double what we get from the data. It therefore seems that changes in γ alone cannot explain the difference.

To digress somewhat, a potentially interesting empirical puzzle arises from their assertion that increasing the importance of monetary shocks should raise (that is, make closer to zero) our estimate of β. Some recent work on the inflation-to-growth relationship (Sarel 1996, Bullard and Keating 1995, Barro 1996, Judson and Orphanides 1996) argues that the relationship is stronger (that $\hat{\beta}$ is more negative) among high-inflation countries than among low-inflation countries. However, it is presumably in these high-inflation countries where the variance of monetary shocks is more important and where we should therefore expect a weaker relationship. I would be interested to see a more thorough consideration of this puzzle.

Another explanation, point (3) above, would be that their model gets the sign wrong for the trade-off between monetary inflation and growth. Presumably, if higher long-run inflation leads to higher (not lower) long-run growth, then the mixture of the two effects we see in the data will be closer to zero than their model predicts. They mention one example of this—that is, a model where investment in human capital is a substitute rather than a complement to labour-market activity; another is the case where the loss in government seigniorage revenue from lower inflation must be recouped through alternative taxes that are more distortionary than the inflation tax. However, the problem is that the effects of inflation on labour-market activity in their model are so weak, and seigniorage revenue so small, that it is hard to believe that either is likely to get us close to the values we see in the actual data. What we seem to need for this kind of explanation is a mechanism whereby a low-inflation policy has much larger permanent negative effects on growth. In this light, it is interesting to consider the evidence for downward nominal wage rigidity as discussed in this conference's third session, and whether it could give a large enough effect.

Finally, point (4) suggests that $\beta_{\text{non-monetary}}$ might be much closer to zero if the authors' exogeneity assumptions are unrealistic, which I suspect is the case. To derive the coefficient of −1 for non-monetary shocks, we had to assume that such shocks affected neither money growth nor velocity. Suppose we instead assume that monetary policy will react to exogenous non-monetary shocks, although the reaction might change across time and nations. The resulting relationship is just

$$\frac{d\dot{y}}{d\pi} = \frac{d\dot{m}}{d\pi} + \frac{d\dot{v}}{d\pi} - 1 \,. \tag{5}$$

Ambler and Cardia argue that $d\dot{v}/d\pi$ is close to zero.[1] This means that, to explain the results in the data, we need a monetary policy that tends to accentuate changes in inflation due to non-monetary shocks ($d\dot{m}/d\pi > 0$). This may be the case if, for example, monetary authorities try to run countercyclical policies but take time to learn about changes in the level of potential output. Many observers think this is a good characterization of monetary policy responses to oil price shocks in the 1970s.

I hope that these considerations of the possible effects of downward nominal wage rigidity, Phillips curves, and monetary policy reactions to real shocks will be persuasive that there are several interesting and credible explanations for the gap between the authors' simulated values and the estimates of β, and that these explanations deserve additional careful scrutiny and comparison.

In these remarks, I have assumed that the main point of the Ambler and Cardia paper was to emphasize the distinction between the growth-inflation correlation and the trade-off between the two that is exploitable by policymakers. Others have interpreted the paper quite differently and concluded that the authors' main message was that the exploitable trade-off for policy is very, very small. While this is certainly the result that their model produces, I wish to explain why I do not see that as a central conclusion.

I think that Ambler and Cardia are careful to present their model as being simply one of many possible ways of modelling the effects of inflation in an endogenous growth framework. They make no claims that their model should give an upper or lower bound to the size of the policy trade-off, or that all reasonable models will give similar results. It seems to be generally accepted, for example, that the interactions of inflation with an imperfectly indexed tax system can increase the after-tax cost of investment, and that for reasonable parameters such effects are much larger than those produced by the "consumption tax" mechanism Ambler and Cardia consider. True, tax

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1. The thought experiment they consider is a one-time permanent shift in the level of inflation. We should not expect this to permanently affect the *trend growth rate* of velocity, although it may well permanently shift the *level* of velocity. However, it seems to me that this is not quite consistent with the authors' discussion of the time-series literature on inflation and growth. There they argue that historical inflation should be treated as a stationary variable. While I am interested in the econometric value of their arguments, I leave that for others to discuss. Instead, let me ask what is the possible relevance of questions about the effects of long-run changes in inflation if inflation is stationary, so that there can be no long-run changes? What precisely is the mechanism that prevents monetary authorities from influencing inflation rates in the long run? Does this imply that our current emphasis on low inflation can be nothing but a transitory policy fad?

effects are problematic for those seeking to justify a move to low inflation: Why not just reform the tax system? What about the costs of forgone tax revenue? Such questions deserve careful consideration. However, my point is simply that it is wrong to cite this paper as "proof" that the growth effects of a low-inflation policy cannot be large.

To summarize, Ambler and Cardia make an interesting contribution to the empirical literature on the relationship between inflation and growth. I think their most valuable message is that we must not confuse the correlation (conditional or unconditional) between these series with the relevant trade-off facing policymakers. While this paper does not yet allow us to answer Johnson's question—whether there is a link between inflation and economic performance—it makes me hopeful that subsequent work will give us a more complete understanding.

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