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Commentary

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In nearly any endogenous growth model, a tax on investment reduces the steady-state rate of economic growth. V.V. Chari, Larry E. Jones, and Rodolfo E. Manuelli attempt to explain the apparent empirical relation between inflation and economic growth by postulating that countries which raise their inflation rates also raise their taxes on investment. However, the authors focus on an implicit tax rather than an explicit tax on investment. That implicit tax operates through firms' transactions costs. Chari, Jones, and Manuelli assume that firms finance marginal investment at least partly through banks and that banks are required to hold non-interest-bearing reserves on their deposits. As a result, an increase in the reserve requirement taxes investment implicitly by taxing the financial intermediation that is an input into that investment activity.

How plausible is this explanation for the apparent relation between inflation and economic growth in the data? Chari, Jones, and Manuelli argue that alternative models of inflation and economic growth predict a relation between the two that is quantitatively too small to match the data. Second, Chari, Jones, and Manuelli argue that their model—in which higher reserve requirements accompany higher inflation—can quantitatively match a number of features of the data.

I remain unconvinced by both arguments. I believe Chari, Jones, and Manuelli have understated the ability of alternative models to match the data quantitatively, partly by overstating the magnitude of the growth effects of inflation found in empirical work and partly by calibrating their model to U.S. parameters that are nonrepresentative of the sample of countries in empirical work on this topic.

Second, I believe that Chari, Jones, and Manuelli have overstated the ability of their model to match the data, because that model ignores the well-documented (negative) empirical relation between expected inflation and the currency-deposit ratio.

AN UNDERSTATEMENT

The authors cite estimates of the relation between inflation and economic growth in the data. They argue that conventional models of inflation and growth predict relations that are considerably smaller than the estimates found in the data. However, these estimates they cite involve a mixture of the level effects on real gross domestic product (GDP) of an increase in the rate of inflation and the growth effects on real GDP of an increase in the rate of inflation. Suppose an increase in the rate of inflation reduces the level—but not the growth rate—of real GDP and this effect takes time to manifest itself. (Perhaps an increase in inflation reduces the steady-state level of the capital stock, but not its growth rate. The transition to the new steady-state growth path would occur over time as the capital stock grows more slowly than it would have grown had inflation not increased.) With the sample sizes and simple linear regression techniques common to empirical studies on inflation and growth, these studies would be unable to distinguish these level effects on GDP from growth effects. In fact, most empirical studies of inflation and growth have simply assumed that inflation affects growth rates rather than levels (at least in the interpretations given to results in those studies).

This would not present a problem if Chari, Jones, and Manuelli were to include calculations of inflation's effects on the level of GDP, as well as on its growth rate, when evaluating alternative models of inflation and growth. But they don't. Instead

they contrast the implications of those alternative models for the steady-state growth rate of GDP with estimates from the data that include both the growth effect and the level effect. Consequently, it should come as no surprise that the estimates exceed the (steady-state) predictions of the models.

For example, Robert J. Barro, in his article in this issue, estimates that a 10 percentage point increase in inflation sustained over 30 years creates a level of output that is 4 percent to 7 percent lower at the end of that period than it would otherwise be. For the moment, take this estimate at face value and suppose that inflation has no effects on steady-state growth. An empirical study that covers a 30-year time horizon may falsely create the impression that a 10 percentage point increase in inflation reduces the average growth rate of output by about 0.2 percent per year. The exercise conducted by Chari, Jones, and Manuelli would contrast this 0.2 percent figure with a model that implies no growth effects of inflation and would falsely conclude that the model fails for this reason.

AN OVERSTATEMENT

Of course, there are many other reasons as well to believe that empirical estimates of the growth effects of inflation overstate the true relation. High rates of inflation often reflect packages of bad government economic policies. Because most of these policies are not measured, or accounted for in the empirical studies of growth, inflation appears (partly, at least) as a proxy for an entire array of bad economic policies.

It may also be important—in the context of the question that Chari, Jones, and Manuelli ask—to treat the growth rate of the money supply as endogenous and to ask what determines its differences across countries. If the answer includes real economic performance, then empirical estimates of the connection between inflation and real growth mix effects running in two directions. If temporarily bad conditions

create pressures for monetary expansion (whether to finance government spending when a fall in the growth of output creates a fall in the growth rate of tax revenue, or to try to exploit short-run non-neutralities of money), then monetary expansion will tend to be associated with a subsequent increase in the growth rate of real output, partly masking any negative growth effects of inflation. Economists need a better model of the political economy of monetary actions (than we currently have available) to begin to answer such questions.

The effects of inflation on the level or growth rate of real GDP may depend critically on certain features of an economy, such as the degree to which financial markets are developed. Well-developed financial markets may reduce the real effects of inflation by reducing the importance of currency (and the inflation tax on it) in transactions, and by reducing the sizes of wealth redistributions from inflation. The “typical” country in the Summers and Heston (1991) International Comparison Project differs from the United States in ways that may affect the connection between inflation and growth. One important difference is that investment is considerably more money-intense in many less-developed countries than in the United States, making these countries more like the Chari, Jones, and Manuelli “CIA-everything” model. Not only is cash a more important method of formal finance in much of the world than it is in the United States, but cash also plays an important role in the common practice of bribery (for licenses, protection of property rights, and other important forms of capital broadly conceived). Consequently, the effects of inflation on real GDP are likely to be larger in countries with less-developed financial markets. Because Chari, Jones, and Manuelli calibrate their model to U.S. data (which are nonrepresentative of the sample of countries in empirical work on inflation and growth), their calibration may lead the model to understate the predicted relation between inflation and GDP.

In addition, their article overstates the ability of their new model to match those data. Previous studies have documented a strong relationship between expected inflation and the ratio of currency to bank deposits. Higher expected inflation induces people to substitute out of currency and into deposits, reducing the currency-deposit ratio. Because banks hold reserves on deposits, this raises the fraction of the monetary base held by banks as reserves. The model proposed by Chari, Jones, and Manuelli ignores this effect and assumes that the entire increase in the fraction of the monetary base held by banks as reserves results from an increase in required reserves. As a result, the authors overstate the extent to which an increase in inflation is accompanied by an increase in reserve requirements. Therefore, they overstate the increase in the implicit tax on investment, through this source, that accompanies an increase in inflation. This leads them to overstate the extent to which their model conforms to the data.

Although Chari, Jones, and Manuelli view an increase in the reserve requirement ratio from .04 to .35 as “moderate,” this increase is quite large. Their model of the money growth rate and changes in required reserves implies much too large an effect of actual changes in required reserves on the growth rate of output. For example, the effect of the Fed’s increase in required bank reserves in 1937 appears to have produced a much smaller change in real GDP than implied by the Chari, Jones, and Manuelli model, as did the 1980 changes associated with the Monetary Control Act and banking deregulation. Moreover, a study of cross-country differences in required reserves would probably show vastly smaller effects on real GDP than their model implies.

A NEED FOR STRONGER EVIDENCE

To make their argument more persuasive, the authors would have to provide stronger evidence on two claims: (1) that increases in inflation are typically accom-

panied by large increases in required bank reserve ratios and (2) that increases in required bank reserve ratios have large effects on the growth rate of real GDP. More generally, are increases in inflation more strongly associated with increases in required reserves or with decreases in the transactions demand for money (the focus of the cash-in-advance models that Chari, Jones, and Manuelli criticize)? The authors have made a valuable contribution in this article by calculating the quantitative sizes of the growth effects of inflation in a variety of monetary models with endogenous growth. For the reasons outlined above, I am less convinced by their use of those calculations to suggest that the models underpredict the true effects of inflation in growth and by their proposed alternative model.

REFERENCE

Summers, Robert, and Alan Heston. “The Penn World Table (Mark 5): An Expanded Set of International Comparisons 1950–88,” *Quarterly Journal of Economics* (May 1991), pp. 327–68.