Commentary: Monetary Policy and the Well-Being of the Poor

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This paper is a notable achievement that deserves a place in the Jackson Hole Hall of Fame. Christie and David Romer have managed to accomplish two things here. First, they actually add a new thought to the academic literature on the effects of macroeconomic performance on the distribution of income—a topic on which I wrote 20 years ago[1] and on which, I thought before reading this paper, there was not much new to say. Second, they reach a conclusion that cannot fail to warm the hearts of central bankers everywhere: "that compassionate monetary policy is, most likely, sound monetary policy."

But I am an academic now, not a central banker, so—mindful of that great tradition—I'll begin with a quibble. My quibble actually precedes the first word of their paper. I'd like to suggest a change in the title. Christie and David claim to have written a paper on how *monetary policy* affects the well-being of the poor. But when you read it, you find out that it is really about how *macroeconomic performance* affects the poor. In fact, when you dig deeper, you find out that the result they most emphasize is, basically, that lower inflation raises a society's *average* income. Whether or not it holds any special benefits for the poor is less clear.

It is perhaps a sign of the times that the Romers cavalierly identify "monetary policy" with "macroeconomic performance," as if they were one and the same. But they are not. Cyclically, such things as

fiscal policy, other demand shocks, and supply shocks influence the economy's short-run performance. Secularly, no one (certainly not the Romers) would suggest that either monetary policy or inflation is the main influence on long-run economic growth.

When I think about the effects of monetary policy, as opposed, say, to the effects of fiscal policy, on the distribution of income, I think of questions that relate specifically to interest rates, such as:

– Are the poor more likely to be debtors who pay interest or creditors who receive interest?

– Do the sectoral effects of interest rates (for example, on the shares of housing, consumer durables, and net exports in GDP) affect the distribution of income?

– Do the intertemporal allocative effects of interest rates (for example, on saving and investment versus consumption) affect the distribution of income?

These questions do not engage Christie and David much, and perhaps for good reasons. So my quarrel is only with the title of the paper, not its content.

The paper carries two main messages. First, that the standard cyclical finding—that the poor are hurt by unemployment but not by inflation—is less important than is commonly believed. Second, that the poor have a strong long-run interest in price stability. I will take up each of these in turn, arguing that while the Romers are basically right on both counts, they overstate their case. The lady and gentleman doth protest too much, methinks.

The first part of the paper is about the effects of the business cycle on the economic status of the poor in the United States. It has been known for a long time that poverty rises and the share of low-income families falls when unemployment increases, while higher inflation has little effect on either. This finding is confirmed again by Romer and Romer though, puzzlingly, they find no systematic effect on the

shape of the income distribution. But they add an interesting twist, which was not emphasized by earlier authors.

Under the conventional view of the natural rate hypothesis, the average unemployment rate must equal the natural rate over any period of time during which the inflation rate does not change. If the poverty rate depends positively on the unemployment rate (as it does), it follows that any decline in poverty achieved on the upswing must be given back on the downswing. With a linear Phillips curve—which is a pretty good assumption for the United States—the swap is exact: What the boom giveth, the slump taketh away. As far as macroeconomic policy is concerned, then, there is a "natural rate of poverty," corresponding to the natural rate of unemployment.

As an 85 percent believer in the natural rate hypothesis, I basically accept this conclusion. But let me take a few minutes to see if we can escape from the Romers' Iron Law of Poverty, at least around the edges.

One possibility, which they discuss but dismiss, is that inflation might change permanently. Consider first a period of disinflation, which most of our countries have recently been experiencing. To push inflation down, the central bank must hold unemployment above its natural rate, on average. So the poverty rate must also be above its natural rate. Hence, the basic conclusion from the older literature is maintained: "The poor pay a disproportionate share of the burden when high unemployment is used to wring inflation out of the system."[2] This disproportionate share stands as an extra cost of disinflation. Now suppose, instead, that the central bank's job is *raising* the inflation rate, as in contemporary Japan. Then the alleviation of poverty becomes an additional benefit from reflation.

My point is simply that the natural rate hypothesis does not rob cyclical findings of *all* interest, as Romer and Romer come very close to suggesting. But they are surely right that it does rob them of quite a lot, and this is worth pointing out.

As a second possible escape from the Iron Law of Poverty, let me raise the issue of hysteresis. Until quite recently, most of us felt

comfortable in dismissing hysteresis in unemployment as theoretically interesting but empirically unimportant for the United States. The natural-rate Phillips curve just worked too well. But, as is well known, the United States is an outlier in this respect: Virtually no other country has such a well-behaved Phillips curve. For many other nations, especially in Europe, hysteresis—and thus a *permanent* tradeoff between unemployment and inflation—remains a live possibility. And recent data raise the specter of hysteresis even in the United States.

Furthermore, hysteresis might be present in the relationship between unemployment and poverty. For example, perhaps a prolonged tight labor market enables marginal workers to break out of the "cycle of poverty." The Romers do not investigate this possibility. In any case, if there is hysteresis in either the Phillips curve or the relationship between unemployment and poverty, then a business cycle can leave a permanent imprint on the poverty rate.

Finally, I must take issue with the way the Romers state the implication of their cyclical conclusion for policy. Their penultimate paragraph states that:

> "...the usual emphasis on the short-run effects of monetary policy on poverty is fundamentally misguided. It is certainly true that expansionary policy can generate a boom and reduce poverty temporarily. But the effect is unquestionably just that—temporary."

They suggest that this temporary effect is hardly worth worrying about.

I beg to differ. Change the word "poverty" to "unemployment," which is legitimate according to their regressions, and the statement becomes:

> "...the usual emphasis on the short-run effects of monetary policy on *unemployment* is fundamentally misguided. It is certainly true that expansionary policy can generate a boom and reduce *unemployment* temporarily. But the effect is unquestionably just that—temporary."

Where have you heard that before?

As many of you know, I have never accepted the view that central bankers should ignore the effects of monetary policy on unemployment simply because they do not last forever. In fact, we have at this conference the greatest fine-tuner in history. I do not believe that the efforts of Alan Greenspan and the Federal Open Market Committee to limit recessions and reduce unemployment have been "fundamentally misguided." On the contrary, I think America has benefited enormously from their success.

Turning now to the Romers' secular findings, let me begin by emphasizing a caveat that they correctly enter before they analyze their multination, cross-sectional data. It takes a pretty big leap of faith, probably several leaps, to believe that, say, the effects of inflation on the incomes of the poor are the same in all countries. Some nations do extensive indexing; others do not. Wage-setting institutions differ. So do tax-transfer systems. Furthermore, these institutions and others that bear on the distributional effects of inflation not only can but do adapt to changes in the inflation rate. To take a close-to-home example, there are now many fewer escalator clauses in wage agreements in the United States than there were in 1980.

So I tend to take cross-country regressions with many grains of salt—unless the findings are extremely strong or there is good reason to believe that the relationship is roughly invariant across space. To my mind, a set of time-series regressions, one for each country, that tells more or less the same story would be far more convincing.

That said, look at Charts 6 and 8, which show the Romers' main secular result: that low inflation is associated with higher incomes of the poor. (The direction of causation is unclear.) I pick these two charts because I prefer removing the outliers; to see why, take a peek back at Chart 4, which includes eight high-inflation countries omitted from Chart 6. These data look to me like a nearly horizontal scatter for the eight high-inflation countries joined to a nearly vertical scatter for the remaining 58. I see little virtue in joining the two datasets.

Chart 6 blows up the scatter for the 58 countries and shows, if you look hard enough, a negative relationship. The regression (equation 3 in Table 4) says this negative relationship is significant, though barely so. But I think the picture gives us a better feel for how reliable the relationship really is—and also for why the regression shows such a large slope. In brief, these data, which have an R^2 of 0.07, would not tempt you to make a large bet on the proposition that lowering the inflation rate will boost the real incomes of the lowest quintile.

Chart 8, which is restricted to the 19 traditional Organization for Economic Cooperation and Development (OECD) countries, is rather more convincing. Notice that the R^2 in the regression is 0.66 (equation 1, Table 6) instead of 0.07 (equation 3, Table 4). Thus we seem to have some evidence that higher incomes for the poor accompany lower inflation in OECD countries, but not in the rest.

Suppose it's true. Let's ask why. In the last parts of their paper, the Romers show that most—but not all—of this negative relationship comes from the negative association between inflation and long-run growth. This last finding is, by now, pretty well known, though not terribly robust. Their cross-sectional coefficient, like Barro's,[3] says that a 10-point drop in the inflation rate would add about 0.2 point to the long-run real growth rate.

But, intriguingly, this is not the entire story. Surprisingly, Romer and Romer uncover a positive relationship between inflation and inequality, as measured by the Gini coefficient. This correlation is visible to the naked eye in Chart 12. It suggests that central banks that deliver lower inflation also contribute to greater equality. Or do they? The closer look at U.S. time-series data offered in Table 2 points in precisely the opposite direction: lower inflation *raises* the Gini coefficient, though not significantly. We are left, I think, wondering about the impact of inflation on the shape of the income distribution.

So what are central bankers to conclude from all this? Let's do a thought experiment. Suppose the legal mandate of a central bank were broadened to include an inequality or poverty objective. Would that change monetary policy? Not very much, I think, if the mandate

already includes concern with unemployment—as the Fed's does, but the European Central Bank's does not. It would simply raise the weight on unemployment in the bank's objective function.

Monetary policy can presumably affect poverty over the same "run" that it affects employment—that is, at least several years. That has always seemed to me a long enough period to matter. And the apparently high discount rates of the poor—as evidenced, say, by the interest rates they pay to pawn brokers and loan sharks—reinforces this conclusion. If we "overweight" the well-being of the poor on general egalitarian grounds, we wind up (partially) importing their high discount rates into the social welfare function.

As to the very long run, the finding that lower inflation helps the poor seems to rest on perilous statistical foundations. We do not really believe that Ghana and Nicaragua are poor countries *because* they have had high inflation. But neither is there evidence that inflation hurts the poor.

So I come to much the same feel-good conclusion that the Romers do, albeit with much more emphasis on short-run stabilization: An egalitarian central bank should pursue sound monetary policies.

Endnotes

[1]Alan S. Blinder and Howard Y. Esaki, "Macroeconomic Activity and Income Distribution in the Postwar United States," *Review of Economics and Statistics* 60 (November 1978), pp. 604-9.

[2]The quote comes from the opening paragraph of Rebecca M. Blank and Alan S. Blinder, "Macroeconomics, Income Distribution, and Poverty," in S. H. Danziger and D. H. Weinberg, eds., *Fighting Poverty: What Works and What Doesn't,* (Cambridge, Mass: Harvard University Press), 1986, p. 180.

[3]Robert J. Barro, "Inflation and Growth," Federal Reserve Bank of St. Louis *Review,* (May/June 1996), pp. 153-169.