

Rejoinder

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As with some of the other criticisms reviewed in my paper, Robert Gordon's strong attack on macroeconomic approaches that emphasize both contracts and forward-looking behavior deserves careful consideration. Moreover, his raising this criticism further illustrates the reasons for my caution against interpreting such approaches as forming a consensus at this time.

Gordon's first and most dramatically explicated criticism of the forward-looking contract approach is based on what he calls the "jamng discrepancy" between the events in the U.S. during 1980-1982—the painful disinflation—and the simulations of a model which I chose for the purpose of illustrating this approach in my paper. I see no such discrepancy. In fact, one reason for reporting the simulations was to show, as I summarized in the paper, that "the high unemployment rates we have experienced during this disinflation are consistent with this type of expectations model." The actual disinflation seems to have been much more abrupt than would be required to avoid such high unemployment according to the model. As I stated, "The deceleration was faster than could be sustained while maintaining full employment." Moreover, it seems to me that the disinflation which has occurred during 1980-1982 has been achieved at least partially through forward-looking expectations effects working simultaneously with the depressed economic conditions. It seems that backward-looking expectations-augmented Phillips Curves would not have predicted the rapid deceleration of nominal wage growth during 1981-1982.¹ This is why some economists have suggested that **the** Phillips curve might

1. I think it is still too early to rigorously assess the predictive accuracy of these models during the current period. Moreover, the models differ widely, and some which emphasize rate of change effects may have predicted more successfully than others.

have an inflection point at high unemployment rates. An alternative to the inflection point argument is simply that *expectations* of relatively high unemployment rates in the next few years, and *expectations* of relatively low inflation has been a factor in wage determination.

Gordon emphasizes the credibility problem in this criticism of the forward-looking expectations approach. Phillip Cagan also mentions the problem in his comments on my paper. There are good reasons to suspect that workers and firms might not believe that the monetary authorities will reduce money growth in the future. In fact, one of the aims of the simulations reported in my paper is to show that the deceleration in nominal GNP or money growth is so slow at the start that it strains people's credibility. Rather than ignoring credibility problems, this research illustrates why credibility is such a serious problem. In terms of Gordon's general criticism of forward-looking expectations, I think the credibility problem is much more serious during periods of transition from one policy to another, and I have studied this problem in a previous paper.² Many of the applications of rational expectations are explicitly concerned with a comparison of economic conditions under alternative policy regimes, rather than with economic conditions during a transition from one regime to another. The research I referred to in the latter part of my paper on alternative rules for monetary accommodation is an example of this. For these types of application — where economic events can be expected to be recurrent — the rational expectations approach seems reasonable.

But even during a transition period it is unclear to me why, as Gordon argues, a purely backward-looking expectations scheme would be preferred to one which deals explicitly with the credibility problem and thereby mixes elements of forward- and backward-looking behavior. The implication of research on transitions to rational expectations equilibria is not that we can retreat to a scheme where expectations can be modeled adaptively with fixed coefficients. This point is clearly illustrated in the paper by Meyer and Webster referred to in my paper. Similar objections can be raised to Gordon's criticism that in a decentralized economy it is unreasonable to assume that economic agents expect that other agents are forward-looking as is implicit in most rational expectations applications. The problem is much more serious for unusual or unique events than it is for recurrent events.

2. J.B. Taylor, (1975), "Monetary Policy during a Transition to Rational Expectation," *Journal of Political Economy*, 83, pp. 1009-21.

Moreover, a satisfactory method of dealing with the problem is unlikely to be found in purely backward-looking schemes. Evidence for this can be found in the research of Robert Townsend³ which has experimented with generalized rational expectations methods to deal with the problem.

In addition to his criticism of macroeconomic modeling with forward-looking contracts, Gordon makes several serious misinterpretations of statements in my paper. First, contrary to Gordon's claim, nowhere in the paper is there a statement that I have "dismissed all of Keynesian macroeconomics." I explicitly used the term Keynesian only to refer to an approach to expectations, not to all of macroeconomics. Second, there is no statement in the paper that monetary policy should only be concerned with price stability. As stated in the paper, the long-run objective of monetary policy is "stabilizing the fluctuations of unemployment and output" as well as price stability. In fact, much of the research on forward-looking contracts discussed in the paper is concerned with a particular characterization of the tradeoff between the two goals. Third, Gordon's ranking of alternative macroeconomic theories on a 1 to 10 scale misinterprets these theories by considering only one type of implication of the theories. His ranking scheme is one-dimensional. My paper tries to emphasize that some of the conclusions of forward-looking contract models are closer to the "new classicals" while others are closer to the "Keynesians." Because he focuses on only one conclusion (doubts about the effectiveness of accommodative policies) while ignoring another conclusion (confirmations about the effectiveness of employment stabilization), his summary evaluation is very misleading.

3. R.M. Townsend, (1983), "Equilibrium Theory with Learning and Disparate Expectations: Some Issues and Methods," in R. Frydman and E.S. Phelps (Ed.) *Individual Forecasting and Aggregate Outcomes: "Rational Expectations" Examined*.