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Reflections on Rational Expectations

Phillip Cagan

The hypothesis of rational expectations has rapidly gained attention because it is so natural and appealing. It must make its opponents furious, because, absurd as they think it is, to attack it is to appear to deny that behavior is rational, an uncomfortable position for an economist. Indeed, it is so appealing that one wonders why it took so long to develop. I must confess that I was no help. When I was testing adaptive expectations in my study of hyperinflations almost thirty years ago, I rejected a contemporaneous effect of price changes on real money balances because it did not fit the data well, and I used adaptive expectations as a more attractive alternative. I had some qualms about my estimates which showed very slow adaptations under hyperinflation. Nevertheless, the alternative formulation of expectations without a lag seemed to go too far. At that time, who would believe that price changes not only resulted from changes in the money supply but did so without a lag? Of course, technical developments in statistical technique since then have brought several problems to light, and it is now not so clear that these episodes are inconsistent with rational expectations as now formulated.

As a footnote to the historical discussion in Lucas's paper, I am impressed by the irony of the fact that thirty years ago very few economists thought that money had an important effect on aggregate demand. I remember the anonymous review in the *London Economist* that said of our *Studies in the Quantity Theory of Money* that, well perhaps money can explain prices during hyperinflation, but that surely is the only situation in which it plays an important role. Today, in contrast to that earlier view, not only does the profession assign money an important role in all situations, but in the models of rational expectations the public knows exactly how money affects aggregate demand and follows monetary policy expertly in forming expectations of those effects. Did the public always know this despite the earlier ignorance of economists? If the public is dependent on what economists know, it has made progress but its expectations still cannot be very good.

Nevertheless, rational expectations are not only intellectually appealing but have received uncontested empirical support in their application to financial markets and

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commodity exchanges. The efficient markets hypothesis—that price changes on exchanges cannot be profitably predicted from past general information because prices immediately reflect all of it—has been supported by many studies. Indeed, economists have put themselves out of part of this business by concluding that economic theory has no value to speculators in predicting changes in these prices. This conclusion is the basic ingredient of rational expectations.

Can rational expectations be applied, in a similar manner, to markets for goods and services? That seems to be the crux of the current debate. The rational expectations proposition that most prices in the economy will conform to the public's expectations of demand and supply is contrary to a long-standing interpretation of prices as being unresponsive in the short run to changes in demand. Since McCallum argues that rational expectations models can accommodate various kinds of price stickiness, it is desirable to clarify this issue in the debate. So far as the policy issues are concerned, the crucial question is whether real output is influenced by expected changes in monetary policy. If expected changes in prices are fully reflected in actual prices and nominal quantities, then expected changes in the money supply will produce corresponding adjustments in prices and leave real quantities unaffected. Real quantities will then be affected only by unanticipated changes in nominal policy variables or by policies that affect real rates of substitution and real rates of return. This basic proposition has two parts. First, expectations are rational and thus incorporate all available information (this is made operational by assuming they incorporate the predictions of the model under examination). Second, markets determine actual prices in such a way as to be consistent with the expectations of economic agents about demands and supplies.

We want to avoid tautology here. It is tempting to say that, if expected effects of money on prices are not fully reflected in actual price changes and thereby also affect output, the expectations are biased and will be improved to remove the bias. But for ineffectiveness it will not do simply to *equate* expectations with the determinants of prices and output, or to *assume* that prices are flexibly determined by whatever expectations of demand and supply people in some rational sense arrive at. As everyone recognizes, we must ascertain what the expected effects of money on prices are, and then test whether price behavior is consistent with these expectations.

The problem with empirical tests of the rational expectations hypothesis is that, despite the best efforts of the researchers, it is difficult completely to avoid a kind of tautology. The recent empirical work of Barro and others purports to show that so-called rational expectations of monetary growth, which are derived from the past behavior of economic variables, do not affect real output. Given how these expectations are measured, the results largely reflect the fact that only the deviations of monetary growth from trend happen to be correlated with cyclical fluctuations in output and employment. It is assumed that the correlated short-run movements are unexpected, but alternative, more traditional interpretations cannot be ruled out, namely, that the trend of monetary growth will be reflected in price trends with or without expectations, rational or otherwise. To be sure, the empirical studies show that rational expectations are not inconsistent with much of the data, and it may be

possible to argue that estimates of certain parameters make sense only under the rational expectations interpretation. But, so far, the capability of this evidence to reject the traditional view is less than overwhelming. As has been pointed out, we require cases where changes in policy alter the part of monetary growth that is expected in order to see whether the relationship with prices and output is thereby affected. Such cases are difficult to identify.

I side with the traditional view that, if the Federal Reserve reduces monetary growth for at least a couple of quarters, even if this reduction is intentional and announced as the objective of policy, we shall have a slump in economic activity and very little initial decline in the rate of inflation. We had such episodes in 1966–67 and 1969–70, and we appear to be coming up with another one this year. Is this proposition inconsistent with rational expectations? Not necessarily, because it could be that the reduction in monetary growth is generally unexpected. After all, monetary growth is subject to large fluctuations, and no one knows whether a reduction for several months will continue. You can't go by what the Fed says, because it has so far exhibited considerable difficulty controlling monetary growth, and it may change its policy for many reasons. Moreover, no one today knows precisely how the money of economic theory should be defined in practice, or how the Fed defines money when it says it is going to reduce the monetary growth rate. Therefore, while the public forms expectations of the trend in monetary growth and builds that trend into the trend of prices, deviations from the trend are likely to be viewed as largely unpredictable in duration, amplitude, and timing. One might model this by dividing monetary growth into a permanent and a transitory component, as is done in a recent paper by Brunner, Meltzer, and Cukierman [1]. Changes in the permanent component are obscured by the transitory component and are only revealed over time. Expectations of such a permanent component are rationally estimated by an adaptive schema, as Muth long ago demonstrated in his seminal paper. Under these circumstances adaptive and rational expectations are the same. If the transitory component is quite large, one may question whether the rational expectations model, though valid, is a fruitful approach for such a situation.

But even if it is not fruitful here, I would not want to discourage the further development of this theory. Lucas's paper makes future developments sound very exciting. I hope that he is right and that I shall live to see them, and also—here I am a little worried—that I shall be able to understand them!

To return to my traditional view of a reduction in monetary growth and output, the outcome may be *inconsistent* with rational expectations models if, even though the reduction is expected, the rate of inflation does not decline commensurately and, contrary to the models, output declines—which is to say, the basic price and output equations of these models do not hold. Why might that be? The equations would not hold if prices are not fully responsive to *expected* changes in nominal demand. That is the crux of the price stickiness objection, which is based on the observation that prices and wages in many markets do not clear the available supply and the amount demanded at prevailing prices, and do so for an extended period in which everyone is aware of the lack of clearing. Although various theories have been

proposed to indicate how this behavior should be accounted for, the theoretical issues remain unsettled. My own explanation of this price stickiness is that there are many plausible reasons why most firms that are price setters rather than takers find it in their interest to set prices according to a long-run equilibrium path and to ignore short- and intermediate-run changes in demand, even those that are expected. As a result many prices are largely set in conformity with unit costs at a standard level of output. When a decline in demand occurs, firms may in some cases know the decline is general throughout the economy owing to a tight monetary policy and that there is some decline in all prices and wages which would allow all industries to maintain the previous rate of sales in real terms. But if, in the face of a change in nominal demand, real demand is to remain largely unchanged through a real-balance effect, the required change in the general price level implies a degree of coordination that the economy is not capable of except over an extended period of time. The reason is that there are two expectations necessary to such coordination, the second of which poses an obstacle. There is, first, an expectation of what has happened to nominal demand in a firm's own industry and in other industries, and, second, an expectation of how other industries will respond to it. Rational expectations may well keep a firm informed of how aggregate demand is affected by monetary policy and other influences, but predicting the behavior of other industries that are price setters is quite another matter. A firm in one industry cannot, when nominal aggregate demand declines, expect to sell the same output and reap the same real profits by reducing its price unless the general price level falls commensurately. Indeed, it cannot afford to reduce its own price by that amount unless its unit costs fall commensurately. Not knowing how the rest of the economy and particularly its suppliers will respond (and cognizant of historical downturns), each firm in this scenario holds its price and waits to see what happens to its input prices and the general price level. If everyone does likewise, prices are sticky, real demand falls, and everyone reduces output. This reduction in output can be attributed to a lack of price coordination across the economy, since in theory as said there is a fall in all prices which would maintain real demand and make everyone better off; but no one has an incentive to be in the vanguard. Contracts fixing prices and wages also delay this adjustment of prices, but I think that a more important reason is the sheer complexity of the coordination required. To be sure, such coordination is achieved by trial and error in the long run, but our models do not deal with the dynamic characteristics of an adjustment in which one firm's behavior depends on expectations of what all other firms will do, and at the same time each firm's likely behavior influences the expectations of all other firms. Markets composed of price setters cannot be expected to behave dynamically the same as do markets composed of price takers. There is a move afoot to label this proposition as disequilibrium economics and to contrast it unfavorably with an equilibrium theory of business fluctuations. I think such a classification at this stage of our knowledge is largely terminological. It may turn out that sluggish price adjustments can be described quite well as equilibrium behavior and not only as disequilibrium dynamics.

The assumption of rational expectations models that expectations of monetary

growth are fully reflected in the general price level without a substantial lag, therefore, seems to me totally unrealistic. Indeed, price behavior seems to have become more and more the opposite of what present rational expectations models describe. We observe today considerable independence of price trends from short-run cyclical movements in monetary growth, movements which cannot be totally unexpected. My interpretation, as suggested above, is that most of the cyclical fluctuations are not perceived as permanent, and most prices are set to follow trend paths according to the permanent component of monetary growth. Prices thus largely disregard the short-run fluctuations in demand, which affect output instead. Since rational expectations may thus view most cyclical fluctuations in monetary policy as transitory (even though, once they occur, their continuation for a short period of time is to be expected), prices may be *less* responsive to these fluctuations in policy than would be true under rapidly adaptive expectations, and policy may thus have *larger* short-run effects on output than we may have thought, contrary to the theory of rational expectations.¹

In policy considerations, I would allow for the possibility that price behavior may undergo change. Nominal interest rates seem to have followed the rate of inflation in the past decade more closely than they did in earlier periods. Perhaps price behavior will gradually develop a greater responsiveness to short-run swings in demand and eventually conform more closely to rational expectations models, which though premature may be prescient of future behavior. As with interest rates, perhaps as rational expectations models become standard fare in business schools, market behavior may adapt to this theory! But that remains highly conjectural.

Under present circumstances, monetary policy *can* affect output and employment in the short-run span of business cycle stages. This says nothing, of course, about the desirability of countercyclical policies. The long-standing objection to such policies—that in practice they do more harm than good—still receives strong support from the past performance of policy. The traditional argument against an active policy is that lags in effects and forecasting make countercyclical monetary policies prone to large errors, that the existence of such policies hardens expectations that the permanent component of the inflation rate will rise because downward pressures on the economy will be swiftly countered and upward pressures only hesitantly resisted, and that fluctuations in policy inject an undesirable degree of uncertainty into the economy. These objections do not necessarily argue that all countercyclical policies are unwise, to be sure, only that they should be conducted on a more modest scale than in the past. But there may be no limit to how modest they should be.

Yet rational expectations have introduced an important new element into the evaluation of the effects of policy, on which I shall close my comments.

In the pre-rational-expectations days, when I was in the trenches doing my duty battling the Keynesians, our side argued that monetary growth had to be reduced to subdue inflation and that, if policy tried to maintain the prevailing rate of inflation,

¹A greater short-run effect on output of a rational as opposed to adaptive expectations model resulted from the tests presented in my book, *Persistent Inflation* [2, chap. 9]. A similar kind of model was developed in Meltzer [3].

the rate would inevitably escalate over time. That prospect was and is so alarming that a gradual reduction, no matter how costly the one-time adjustment might be, is in my view preferable to the inevitable stop-go policies that are and will be undertaken in response to escalating inflation. But in that battle we could never topple the barricade to an anti-inflationary policy imposed by the short-run costs of lost output and employment.

Then the rational expectations reinforcements appeared and began to attack that barricade with a bombshell: namely, that those costs would not be incurred and that any observed declines in output were the result of maximizing behavior and not to be interfered with. I had to marvel at the audacious technology of this new weapon. It was right on target and so deceptively simple in its logic. I feel like a traitor in not using it.

But, lo and behold, the missile from this weapon has a delayed warhead. If policy adheres to a path of declining monetary growth, expectations of the permanent component will gradually take account of the *change in direction* and not follow behind it as under the old adaptive adjustment of expectations. This reduces the costs of curbing inflation far below what is estimated from past periods in which the permanent component could not have been expected to decline. This is the burden of Fellner's argument. It might be interpreted as saying that there is a third kind of capital. In addition to physical capital and human capital, we should also recognize credibility capital—the stock of public confidence in the determination and ability of policy to guide the price system along a particular path. Under the pre-1914 gold standard, everyone believed that general price movements were narrowly bounded, and the dynamic behavior of prices was constrained accordingly. That particular capital has been eroded, with important changes in the dynamic behavior of the price system. No one believes the price level is constrained anymore, and bounds on even the rate of inflation appear to be widening.

Unfortunately, it is very difficult to obtain evidence on the importance of credibility capital. Fellner presents some that is suggestive, but as he notes it is, again, less than overwhelming.

Yet, if prices are importantly influenced by expectations, the degree of public confidence that policy will follow a particular path seems bound to respond to a maintained change in policy. We cannot, therefore, assess the cost of a policy of reducing inflation from past periods in which the credibility capital of that policy was declining. We must allow for the possibility that, although the stock of credibility capital in an anti-inflationary policy is now at an all-time low, it will increase as the authorities demonstrate adherence to such a policy.

But credibility is elusive to measure and to influence. The credibility of an anti-inflationary policy may be irretrievably lost—short of returning all the way to the gold standard. Certainly no one can promise that the cost today of an effective anti-inflationary policy would be low, even if high estimates of that cost based on past experience are questionable. Still, given the evidence that inflation tends to escalate if it is not effectively controlled, I do not see any viable alternative to a policy of gradual reduction. Indeed, I am so alarmed by our failure to carry through with such

a policy to control escalating inflation that I would give up a lot to see such a policy put in place. Since many people remain doubtful that the credibility of such a policy will ever be restored sufficiently to achieve much reduction in its costs, the initiation and maintenance of an anti-inflationary monetary policy that will be viewed as effective needs help. Many people believe that some kind of controls program such as TIP would influence expectations favorably. Such a program in my view would be largely placebo (and runs the danger of misleading monetary policy), while a reduction in monetary growth would do the job. But, if a combined program of controls and reduced monetary growth is the only way to put an effective monetary policy in place and stick with it, such a package founded in desperation is better than escalating inflation.

My point in this digression on controls is that concern over the credibility of policies involves us willy-nilly in the realm of political acceptability and consensus. It is not a realm in which the advice of economists is widely sought or followed. But as we go from adaptive to rational expectations, we introduce credibility capital into consideration of the effects of monetary policies, and in so doing we slip from the traditional confines of economics into the complexities of the political and psychological realms. For better or for worse, rational expectations introduces a can of worms into economics.

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[Footnotes]

¹ Anticipated Inflation and Unanticipated Price Change: A Test of the Price-Specie Flow Theory and the Phillips Curve

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