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Trade-offs in Monetary Policy

Milton Friedman

Phillips Curve

In 1958, A. W. Phillips came up with an empirical negative relation between the rate of inflation and the level of unemployment, quickly christened the Phillips curve (Phillips, 1958). Phillips himself did not present the curve as a policy tool, but less than two years later Paul Samuelson and Robert Solow published a celebrated article in the *American Economic Review* (1960) in which they did. Given the long period for which the Phillips curve appeared to hold in Britain, Samuelson and Solow concluded that it could be treated as a long-run structural equation which provided the missing equation that the then conventional Keynesian system needed. They treated it as a menu from which the monetary authorities could choose. By tolerating higher inflation they could experience lower average unemployment and vice versa.

Friedman/Phelps

In 1966 and 1967, E. S. Phelps and I criticized that approach, arguing that the Phillips curve was a short-term relation (Friedman, 1966, 1968; Phelps, 1967). In the long run, there was a natural rate of unemployment which could be combined with any level of inflation. The long-run Phillips curve was vertical. Inflation was a monetary phenomenon, not a real phenomenon.

The great inflation of the 1970s, labelled stagflation because both inflation and unemployment rose together, was a dramatic confirmation of the natural rate of unemployment view. That view became conventional wisdom in the monetary policy community.

The Taylor Curve

In 1979, John Taylor published an article that was to launch a fresh line of research. Its main objective was to estimate a simple general equilibrium model of the US economy incorporating rational expectations (Taylor, 1979). Taylor then sought to use his model to judge and develop monetary policy.

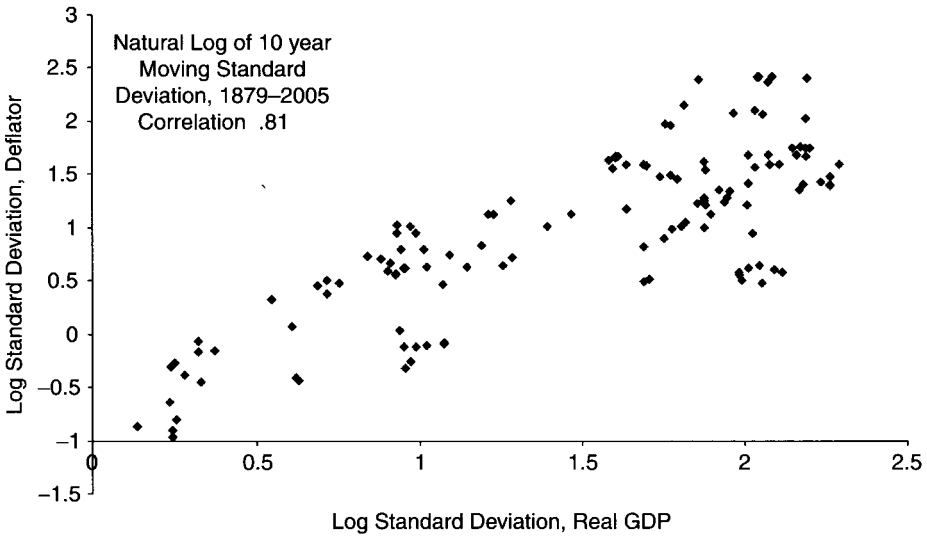


Chart 7.1 Variability of inflation vs variability of real income

He concluded, 'There is no long-run tradeoff between the level of output and the level of inflation in the model – the Phillips curve is vertical in the long run. However, there is a long-run tradeoff between fluctuations in output and fluctuations in inflation. In other words, there is a "second order" Phillips curve which is not vertical in the long run' (Taylor, 1979, 1280).

This comparison of the Taylor curve trade-off with the Phillips curve trade-off is not valid. The Phillips curve was based on empirical evidence, which was interpreted as reflecting a cause-effect relation: an increase in inflation will lead to a decline in unemployment.

The counterpart of the Phillips curve in terms of variability of inflation and output would be an analysis of the observed relation between the two as in the accompanying Chart 7.1 based on annual data for the United States from 1879 to 2005. Clearly, the observed correlation between the variance of unemployment and the variance of inflation is generally positive, not negative. There is no sign of the kind of trade-off offered by the Phillips curve.

The trade-off in the Taylor curve is not an inference from experience. It is an implication of a policy choice. The central bank is assumed to have two objectives: an inflation target and an output target. It seeks to minimize a loss function that is a weighted average of two terms: one based on deviations from the inflation target, one based on deviations from the output target. A zero weight on the output term reduces the bank's objective to inflation alone. Similarly, a zero weight on the inflation term reduces the bank's objective to output alone. As the weight varies between these two extremes the bank's objective shifts. Corresponding to each weight there is a policy rule that is optimal for the Taylor economic model. This policy rule will, in turn, imply for that model a variance of inflation and a

variance of output. Understandably, the greater the weight on inflation, the lower the implied variance of inflation, and the higher the implied variance of output. Increase the weight for output and the implied inflation variance goes up, the implied output variance goes down. Plot these points on a chart and they generate an efficiency frontier showing a trade-off, but only for optimum monetary policy. Moreover, that curve will be different for every assumed economic model and at its best is based on a rough approximation of the way in which the economy works. Three or four estimated equations are crucial for the Taylor economic model but the economy as a whole is determined by millions of equations. At most, we could hope to get a rough picture of it.

The Taylor Rule

The instrument used by the Federal Reserve to control monetary policy is the federal funds rate – the overnight rate charged by banks to one another. The Open Market Committee of the Fed. specifies a target federal funds rate, and uses open market and other operations to keep the market rate equal to the target rate. Taylor accepts this set-up and develops a Taylor rule for the target rate. The rule gives the target rate as the algebraic sum of three terms: (1) estimated long-run equilibrium real federal funds rate at the levels of inflation and output chosen by the Fed.; (2) the deviation of inflation from its target rate multiplied by a coefficient; (3) the deviation of output from its target rate multiplied by a coefficient. The coefficients of the final two terms are, in turn, the product of two numbers: (1) the reaction of the target rate to the deviation of inflation or output from the target; (2) the fractional importance attributed to each of the objectives.

The rule thus embodies the idea of a policy trade-off. Give 100 per cent of the weight to preventing inflation and the recommended real interest rate will be affected only by inflation deviations and will not react to output deviations. Similarly, give 100 per cent of the weight to the output term and the recommended real interest rate will react only to output deviations. Fractional weights will lead to intermediate results.

I believe that it is a mistake to treat the Fed. as having two separate and independent objectives. The Fed. exists to define a monetary system. In my opinion, it has one and only one function: to keep the price level steady. The price level and inflation are monetary phenomena. They are defined by what happens to the quantity of money relative to output. Output is a real magnitude, not a monetary magnitude. Treating the Fed. as having two separate objectives is an open invitation to engage in fine-tuning, something that has almost always proved a mistaken practice.

My own preference for an instrument has always been a nominal monetary aggregate. Taylor estimates in his article the effect of a policy of a constant percentage increase in the quantity of money. He finds it 'interesting that this simple rule gives an output variance [for his economic model] considerably below the actual U.S. performance' (1979, 1282). I do not find that surprising at all. If, as I believe to be the case, inappropriate fluctuations in the quantity of money

produce inappropriate fluctuations in output, keeping money growth constant would eliminate such inappropriate fluctuations in output.

On this interpretation, the Taylor rule is an attempt to specify the federal funds rate that will come closest to achieving the theoretically appropriate rate of monetary growth to achieve a constant price level or a constant rate of inflation. On these lines, the inclusion of the deviations in output from a target rate is not justified by a secondary objective of the Fed. It is rather to be justified by the inadequacy of inflationary deviations alone to generate the appropriate fluctuations in money. Suppose the federal funds target rate is equal to a Taylor rule that gives 100 per cent weight to inflation deviations. That may not be the right rate to achieve the desired inflation target because other variables, such as output or monetary growth, are not at their equilibrium levels. On this view, additional terms in the Taylor rule would reflect variables relevant to choosing the right target funds rate to achieve the desired inflation target.

Monetary Variability

I add two charts to bring out the role of monetary variability – the one item that central banks can control. I have used M2 as the monetary aggregate, though it may be that the base or some intermediate total would be a better instrument.

Chart 7.2 is a scatter diagram of the variability of M2 and the variability of real GDP – total output. The measure of variability is the log of a moving standard deviation of successive ten-year periods. There is clearly a strong positive correlation extending over the whole of the period.

Chart 7.3 shows the same data as time series. It brings out the sharp break between the period up to the end of the 1970s and the rest of the period.

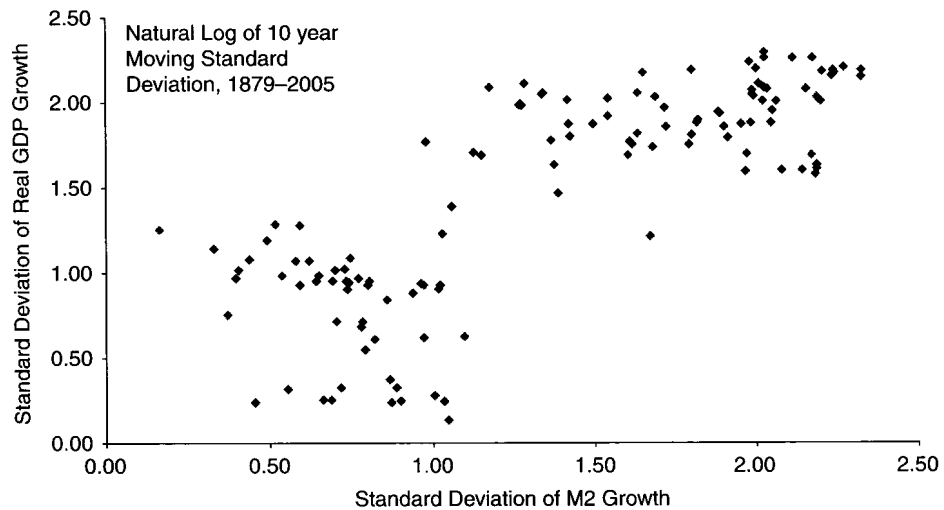


Chart 7.2 Variability of monetary growth and variability of output growth Correlation = .79

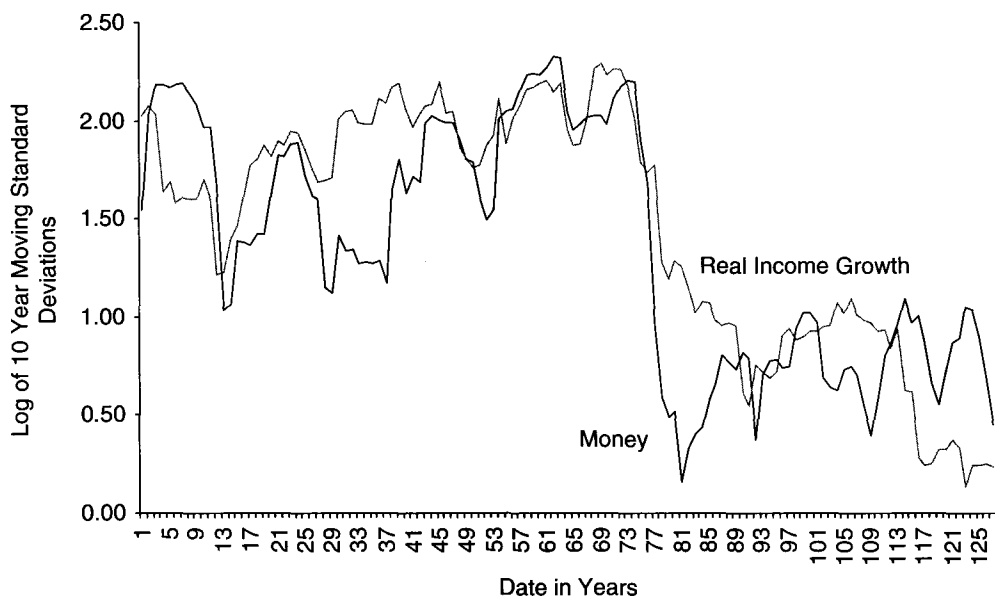


Chart 7.3 Standard Deviation of money growth and real output growth

The collapse of the variability of output is clearly an effect of the collapse of monetary variability. In my opinion, the same results could have been obtained at any earlier time and can continue to be achieved in the future. What is involved is not a trade-off but direct cause-effect.

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Discussion¹

John B. Taylor

I greatly enjoyed reading Milton Friedman's paper and I appreciate the opportunity to offer some comments on it for David Laidler's *Festschrift* conference. The

paper's assessment of recent research on monetary policy trade-offs and rules, as well as the comparison with earlier work, raises important questions and suggests interesting new lines of research. The charts on money growth, inflation, and output variability convincingly document the recent improvement in macroeconomic performance and also demonstrate the role of monetary policy in achieving this improvement. The paper should be required reading for today's students of monetary policy, and, indeed, I required it of my students in my monetary theory course that I taught this past spring at Stanford.

Let me focus on some of the key points the paper makes about trade-offs and policy rules.

Milton argues that the trade-off between the variability of inflation and output, which I first estimated in my 1979 paper – the so-called 'Taylor curve' – is not strictly analogous to a Phillips curve because the two curves relate to the data in different ways. I agree that they are conceptually different. One has to transform the estimated regression coefficients from an econometric model to compute the variability trade-off, while one can obtain the Phillips curve directly from the estimated regression coefficients. You can see the Phillips curve – or more accurately the shifts in a Phillips curve – in scatter plots of the data, while you do not see the Taylor curve in scatter plots of the data. One therefore relies on a specific model, or a theory, in the case of the variability trade-off, while one does not in the case of the Phillips curve trade-off.²

I would argue, however, that using theory and a model to compute the variability trade-off has some advantages. That it is based on theory is one reason why the variability trade-off has a greater chance of being stable over time, and why it is more useful for policy than the original Phillips curve. To the extent that the theory is accurate, including the use of rational expectations, the variability curve is less susceptible to the Lucas critique. Milton rightly cautions that the variability curve 'will be different for every assumed economic model and at its best is based on a rough approximation of the way in which the economy works', and that 'Three or four estimated equations are crucial for the Taylor economic model but the economy as a whole is determined by millions of equations'. While I agree, I also note that all our theories are approximations; moreover, other researchers with different models (different approximations) have computed these variability curves, and, in my view, they appear to be robust to modelling differences to be useful as a framework for policy analysis.

Another difference between these two curves is that the variability curve is an efficiency frontier. It does not predict that the observed points will only be on the curve, but rather that points should also be above and to the right of the curve. Points closer to the curve represent better policy – more efficient policy. The scatter diagram in Chart 7.1 of Milton's paper illustrates this in my view. It shows that the variability of both inflation and output has come down over time. They represent better – more efficient – monetary policy. That the scatter of inflation and output variability observations is positive sloping rather than negative sloping does not therefore represent a contradiction to the variability curve. If policy were on this trade-off, then there would be a negative correlation in the data. But

if policy was in the inefficient area, which I believe it was in earlier years, there could be a positive correlation.

Milton raises another caveat about the trade-off curve, namely that 'Treating the Fed. as having two separate objectives is an open invitation to engage in fine-tuning, something that has almost always proved mistaken practice'. I agree that there is a potential danger, but I stress that the practical message in the trade-off is that one can go too far in trying to keep inflation within a very narrow band. This would require that the central bank move the instruments of policy around too much and would likely increase output variability.

Using the trade-off, as I originally calculated it, I showed that a fixed money growth rule – a Friedman rule – would have led to better performance than actual policy in the post-World War II period up until the time I published that paper. This was an important finding in my view, and I am glad that Milton brings attention to it in his paper. I stressed this point in the final lines of my 1979 paper, while also stating that a money growth rule which responded to economic developments could do even better. Since then I have found that policy rules in terms of interest rates – so called 'Taylor rules' – have worked better as practical guidelines for central banks, and Milton mentions one interesting reason for this which deserves careful study: 'The Taylor rule is an attempt to specify the federal funds rate that will come closest to achieving the theoretically appropriate rate of monetary growth to achieve a constant price level or a constant rate of inflation.'

Another issue raised in Milton Friedman's paper is the relationship between the weight on output in the objective function used to derive the Taylor curve and the weight on output in the Taylor rule. In general, a zero weight on output in the objective function does not imply a zero weight on output in the Taylor rule. The reason is that output is a factor in determining inflation; it even leads inflation according to statistical tests. So an interest rate reaction to output (as well as to inflation) can help stabilize inflation. Even if you had a zero weight on output in the objective function, therefore, it would be optimal to put some weight on output in the Taylor rule.

Finally, let me mention a connection between David Laidler's research and the work on money growth and interest rate policy rules that we are discussing here. I first got to know David Laidler by attending many monetary policy conferences with him in the late 1970s and 1980s, and I came to respect him greatly for his skills, knowledge, and judgement as a monetary economist. In writing my policy rule paper (Taylor, 1993), I was motivated in part by a concern that practical work on policy rules was being abandoned, stating in the paper that 'Even those who have advocated the use of policy rules in the past seem to have concluded that discretion is the only answer', and I was thinking of David Laidler when I wrote that sentence. Indeed, in my paper, I quoted a remark that David had recently made at a Bank of Japan conference which we both attended: 'We are left, then, with relying on discretionary policy in order to maintain price stability' (Laidler, 1993). That remark made an impression on me.

Since the quote was from a then unpublished piece, I sent a copy of a draft of my paper to David, enquired whether the quote was still accurate, and asked for his

comments on my paper. He wrote back, half in jest, 'I'm never one to pass up the chance of being quoted in a paper that's likely to be as widely read as yours', but then added on a more serious note that 'former supporters of the money growth rule and variations on it, such as myself, could, with your rather broader and more helpful use of the term "rule" be thought of as still supporting it' (Laidler, 1992).

It is a real honour for me both to discuss Milton Friedman's paper on policy trade-offs and rules, and to participate in this conference in honour of David Laidler.

1. This is a written version of informal remarks presented by video from San Francisco at the conference in honour of David Laidler, which was held in Canada on 18 August 2006. I thank Milton Friedman, David Laidler, and Robert Leeson for comments on an earlier draft.
2. I note, however, that Phillips may have had a model in mind when he originally estimated his curve, though Milton and Ned Phelps later showed that popular policy interpretations (developed later) of the estimated curve had some serious problems. David Laidler (2002) discusses this possibility in his review of Robert Leeson's (2000) collection of Phillips' papers, which contains additional useful references. For a detailed historical review of how the estimated Phillips curve came to be viewed as a policy trade-off see Laidler (1997).

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Discussion

David Laidler

I

To contribute to my own *Festschrift* is perhaps a little unusual, but is amply justified, even required, by the role that Milton Friedman has played in my career. I first encountered his work in 1957 when, as a second-year undergraduate at the LSE, my tutor E. J. Mishan had me read 'The Quantity Theory of Money – a

Restatement' as an antidote to the very orthodox IS-LM macroeconomics to which I was then being exposed. I'm bound to say that I didn't understand a word of it. I did a little better the following year with *A Theory of the Consumption Function*, even though I was warned by another teacher, who had better remain nameless, of the heretical implications of a straight-line-through-the-origin relationship for the then sacrosanct macroeconomic case for income redistribution – Milton's work was already an object of considerable political suspicion in the 1950s! Above all, however, it was 'The Methodology of Positive Economics', read just after I had completed Karl Popper's course in scientific method, that really caught my imagination.

There were many reasons why, in 1960, I ended up a graduate student at Chicago, including an offer of financial support which arrived at more or less the same time as Yale's refusal of admission, but the prospect of studying with Milton was high among them. Nor was I disappointed: his course in price theory, not to mention the B that he gave me in the first quarter – it still smarts – taught me to take my economics more seriously than I had previously thought possible – in more ways than one; and my summer 1961 work with his co-author Anna Schwartz (whose own contribution to this *Festschrift* is also a source of particular pleasure) on the *Monetary History of the United States* gave me a respect for data and the role of narrative history in their interpretation that I have tried to live up to ever since. All of this now seems a long time ago, and I am deeply moved to be the recipient of an essay written by a man who has been a constant source of help, encouragement, and criticism throughout my professional life.

Milton has written a paper on the theory of monetary policy, an area in which my own work could not have been done without the foundations he was already laying even before I was a student, and I am delighted that so distinguished a colleague as John Taylor is commenting on it. John was one of the very few members of the generation that came on the scene after Milton who did not let the theoretical attractions of clearing markets and rational expectations blind him to the existence of real-world frictions which, even if theory could not explain them, demanded to be taken into account if monetary economics was to retain its empirical relevance. The importance of his work for the post-monetarist development of the theory of monetary policy needs no advertisement from me, and I am flattered indeed to be the object of his attention today.

II

So what is there to be said about Milton's comments on the work of his most important successor on the trade-offs between real and nominal variables that monetary policymakers might or might not face as they go about their business? First, surely, that he is right about the part played by the old Phillips curve in the theory of monetary policy as it developed in the 1960s. The role then assigned to the trade-off between the rates of inflation and unemployment was indeed different to that which John's work now assigns to the trade-off between their variability. Though I think that Milton attributes too much to Samuelson and

Solow (1960) here, when the real culprits were Grant Reuber (1962, 1964), Dick Lipsey (1965) and Harry Johnson (1968), there is no doubt that mainstream macroeconomics did come to treat the Phillips curve as a structural relationship that provided a constraint, subject to which policymakers could maximize a social utility function.¹ The Taylor trade-off, on the other hand, is recognized to exist only when the economy is subjected to a particular kind of stabilizing policy, and Milton's charts provide more than enough evidence that policy of a different sort not only can reverse its slope, but all too often has done so.

Even so, let me suggest that there is a little more to be said about the comparison between these two trade-offs than Milton's argument might initially lead one to think. Ever since the Phillips curve was discredited by the painful experience of the 1970s and 1980s, we have tended to forget something rather important about that relationship: namely, that it was clearly visible in the pre-World War I British data that Phillips (1958) analysed, and in the post-World War II data against which he then tested it.² If it was not a structural relationship, and Phillips, be it noted, made no clear claims that it was, then it must still have been implicit in the macroeconomic systems ruling in those periods as some sort of reduced form. Nor, with the benefit of hindsight illuminated by four decades of work on the role of policy-regime-induced expectations in economic life, is it hard to form conjectures about what features of the two systems were particularly important here. In both episodes, British monetary policy rested on a credible overriding commitment to a fixed exchange rate for sterling against a stable international money, gold before World War I, and the US dollar after World War II. It is plausible that the workings of these regimes gave rise to a stable expected inflation rate around which variations in actual inflation were then correlated (positively) with output and (inversely) with unemployment.³

In short, though its exponents did not present it as such in the 1960s, the Phillips trade-off was, like the Taylor trade-off, a relationship generated by an economy operating under a particular monetary policy regime, and likely to disappear when that regime changed. The two differ, however, in that no policy regime that tries to exploit the Phillips trade-off in its original static form can preserve its existence, while this is not true of the Taylor trade-off.⁴ This analytic contrast between the two reveals features of the problems facing monetary policymakers that Milton's (nevertheless accurate) contrast between the roles that they have played in the theory of monetary policy at different stages of its development tends to obscure.

After Friedman (1968) and Phelps (1967), if one was willing to set aside results about possible non-super-neutralities that were available in the then thriving money and growth literature, one could argue that policymakers should not try to exploit a static Phillips relation because the economy's structure made it impossible to do so. One can't say that about the Taylor relation, so if a case against trying to trade off variability in output and employment against variability in inflation is to be made, it has rested on the undesirability, not on the impossibility, of doing so. There does, indeed, exist a class of policy regimes

within which the trick apparently can be worked, and that is why Friedman and Taylor, who agree about so much, can still find room to debate this issue. Their disagreements can, I suspect, be directly traced to the different policy implications that Friedman (1968) and Taylor's early co-author Phelps (1967) drew from their co-discovery of the expectations-augmented Phillips curve. For Friedman, that relationship made an analytic case against directing monetary policy in any way towards real variables, while for Phelps, as the title of his paper made quite clear, it revealed that a trade-off, widely believed to be static, was in fact intrinsically dynamic and, in that form, remained exploitable for policy purposes.

III

Present-day discussions of the theory and practice of monetary policy are often marred by an unfortunate ambiguity in their use of the phrase *policy rule*, which it would be helpful to clear up at this point. This phrase can refer first of all to a legally, even constitutionally, binding constraint on the conduct of monetary policy. That is how Henry Simons (1936), following in the footsteps of Irving Fisher (see, e.g., 1920) used it. Or it can mean a systematic policy reaction function, employed by the monetary authorities to govern the adjustment of their chosen policy instrument – the function's dependent variable – in the light of the behaviour of the economic variables that appear on its right-hand side. That is how it is used when we speak of a Taylor rule. We often get confused between these two very different usages because Friedman's (1960) *k* per cent money growth rate proposal happened to be a rule in both senses. It specified a particular (albeit degenerate) feedback function for his chosen policy instrument, namely the money supply, which was also to be legally binding on the Fed.⁵

Though it would be possible in principle to enshrine a Taylor rule in an administrative decree or even in a central bank's governing legislation, the wisdom of so doing would be at the very least questionable, and I am unaware of any proposals anywhere to do so. Starting in the early 1990s with New Zealand, Canada, and the UK, however, an increasing number of countries have adopted rules in the Fisher–Simons sense for the goal of monetary policy, albeit rules requiring stability in the rate of change rather than the level of some price index. Furthermore, the central banks that are required to implement these rules have typically – I suspect universally – found some form of Taylor rule to guide the behaviour of the short-term interest rates under their direct control a helpful guide as they carry out their obligations. Also, it is implicit in the structures of the economies in which they are operating that, even where the sole prescribed goal of policy is an inflation rate, the behaviour of real variables intrude upon the policy scene. They do so in two interrelated ways, both stemming from the fact that output and employment variations are central to the mechanism through which monetary policy's effects are transmitted to prices in an economy where money wages and prices are less than perfectly flexible.

The first of these, sufficient by itself to open up space for real variables in a policy reaction-function, should cause no discomfort even to the most ardent exponent of the view that monetary policy should concern itself with the behaviour of the price level alone. Monetary policy takes time to have its effects on prices, so under such a rule, today's policies must make use of forecasts of future inflationary pressures. If, as seems likely, today's levels (and perhaps rates of change) of output and employment contain information about those future pressures, then current monetary policy should use that information. Precisely how, and to what extent, is a purely technical question, but if a Taylor rule systematically geared to real variables turns out to be a useful guide for those seeking to stabilize the inflation rate, then so be it.⁶

Real variables also intrude on inflation-targeting regimes in another way. As the Bank of Canada tells visitors to its website: 'Inflation control is not an end in itself... Low inflation... contributes to better economic growth over time and works to moderate cyclical fluctuations in income and employment.' Furthermore, an injunction to pursue 'low inflation' is not sufficient to define a policy rule in the Simons-Fisher sense. Choices must be made about: what price index to target; whether to set an upper bound for inflation, or to pursue symmetry around some central tendency; whether to prescribe a time path for the price level or merely for the inflation rate; over what time horizon errors should be corrected when they occur; and so on. Each of these choices (and no doubt others too), once implemented, will have implications for the behaviour of real variables, which will, therefore, be taken into account in designing any inflation-targeting regime. Moreover, as our knowledge of how inflation targeting works evolves over time, so will the way in which these problems are addressed. An inflation-targeting regime will (and should) be open to modification in the light of experience, including experience of its effects on real variables.

IV

Inflation-targeting regimes do not ignore real variables, that is to say. Like older-style regimes, they see monetary policy as a means of promoting a desirable performance on the part of the real economy, but they differ from them in the way they implement this vision by pursuing a purely nominal target within a framework designed to maximize policy's beneficial *indirect* impact upon that performance. But, in focusing *directly* only on a nominal goal, inflation targeting is nevertheless entirely within the spirit of Friedman's own long-standing prescriptions for monetary policy (and of those of Simons from whom Friedman drew much of his inspiration). If its conduct in some jurisdictions also generates an inverse correlation between the variability of inflation on the one hand, and of income and employment on the other, and even relies explicitly on a Taylor rule with real variables on its right-hand side, one should not be disturbed by this.

However, I suspect that when Milton thinks about the deployment of a Taylor rule to guide monetary policy, it is not in the context of a regime ruled by inflation targets, such as Canada's, but of the US regime, which currently lacks that important self-imposed constraint, and within which, therefore, a temptation to implement policies that seek to maximize a social utility function in which both inflation and real variables have equal status is clearly present. That temptation is, moreover, made all the stronger by two other characteristics of the US policy scene: namely that the Fed. is already widely regarded as being subject to a so-called dual mandate to pursue both price stability and high employment, and that memories are beginning to fade of the damage that such policies did the last time they were tried and failed.⁷ Moreover, the relationship of the politics of US monetary policy to politics more generally seems to be such that, were the Fed. to seek a mandate to pursue an explicit inflation target as the sole goal of monetary policy, it would risk opening up a political process that it would have no ability to control. Milton's concerns about monetary policy in the US these days, and the role of the Taylor rule, therein, are thus well justified, and given the US dollar's role as the world's prime international currency, even those of us who live under local inflation-targeting regimes should share them.

Notes

I am indebted to John Taylor and Edward Nelson for assistance in connection with this chapter.

1. I have discussed this matter in detail in Laidler (1997, as reprinted 2004). For a statement of the prosecution case against Samuelson and Solow, however, see Robert Leeson (1997).
2. The US data presented by Samuelson and Solow (1960) are much less convincing in their support for the relationship's existence. That these authors' conjectures about the existence of a trade-off were taken so seriously on the basis of such flimsy empirical analysis is a minor mystery in the history of postwar empirical economics whose resolution probably does have a great deal to do with the political background to work on the Phillips curve discussed by Leeson (1997).
3. That is how I interpreted postwar British experience in my (1972) Lister Lecture.
4. That this was not understood until well into the 1970s is attested to by the frequent references in the literature of that time to the freedom that the adoption of a flexible exchange rate would confer on policymakers to exploit the Phillips curve. Michael Sumner (1976) was a pioneer in debunking this dangerous myth.
5. I here set aside the complications arising from the fact that the money supply was not under the direct control of the Fed., so that its manipulation required some kind of feedback rule linking a variable that was under direct control – the monetary base in Friedman's analysis – to its evolution.
6. They are precisely the same types of question that must be asked about the helpfulness to policymakers of the behaviour of monetary aggregates. In economies such as Canada, where inflation targeting has produced a decade and a half over which inflation has remained on target on average, while moving out of its assigned two-percentage-point range rather infrequently and seldom for long, it is becoming harder and harder to assess the information content of any variables for future inflation, money growth, and output and employment gaps alike. The very success of the policy regime has all but eliminated the degree variability in inflation needed to keep such assessments up to date.

7. It is nevertheless the case that the 1977 Humphrey-Hawkins Act that put the Fed's explicit dual mandate in place (and set several unworkable and now largely forgotten policy goals, too) has now lapsed, with only that Act's requirement that the chairman of the board of governors testify twice a year to Congress remaining in force. It is now embodied in the American Home Ownership and Economic Opportunity Act of 2000. On this, and the original Humphrey-Hawkins Act, see John Crow (2002, 40–41).

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