David Laidler Reviews James Forder's "Macroeconomics and the Phillips Curve Myth"

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(For a response by the author click here)

The "myth" of Forder's title goes roughly as follows. In 1958, Bill Phillips published a paper, about an apparently long and well established relationship between wage inflation and unemployment in the UK, which immediately attracted wide attention. In the US, thanks mainly to Paul Samuelson and Robert Solow, this relationship was soon read as presenting policy makers with an exploitable trade-off between unemployment and price-inflation – this practice of shifting the analysis between labour and output markets, as the context required it, was a very early development. Subsequent attempts to buy higher employment with higher inflation in due course created rising inflation instead, just as Edmund Phelps and Milton Friedman warned in 1967-68. Friedman in particular stressed that standard theory about the determination of real wages had been misapplied to the behaviour of money wages, and, like Phelps, suggested the introduction of endogenous expectations about inflation into Phillips' original function, preferably with a unit coefficient. This insight, so the myth proceeds, came just in time for policy makers to begin to understand and eventually to bring under control the "stagflation" of the 1970s that their own previous errors had created.

This story is well entrenched in our discipline's folk-lore by now, but Forder argues that it is an ex post fabrication. I largely agree; but I do have reservations about certain details of his case, which has it that: (a) there is not a shred of evidence that macroeconomic policy anywhere attempted to exploit an inflation unemployment trade-off in the 1960s; (b) only a few people suggested that it should; (c) Phillips 1958 paper, peripheral both to his own research agenda and to an already flourishing empirical literature on wage and price inflation, was poorly executed, in the main badly received, and soon forgotten; (d) in the 1950s and 1960s, most economists showed little sign of being confused between real and money wages, and knew all about the importance of expectations for inflationary processes too; and (e) Friedman's claims to the contrary about these matters, made most clearly in his 1977 Nobel lecture, had more to do with exaggerating the originality and importance of his own 1968 paper than with setting out an accurate historical record. I shall discuss these matters in turn.

On count (a), Forder is completely right. The three committees that set the tone of monetary policy in the 1960s, Radcliffe (U.K. 1959), Commission on Money and Credit (U.S. 1961), and Porter (Canada 1964) were all aware of arguments for the existence of a stable inflation-unemployment trade-off, the first and last being familiar with Phillips' own paper, and all rejected those arguments. They had more traction with economists advising the Kennedy and Johnson administrations, but, when faced with what was often called the "dilemma model" – the term itself dates back to the 1940s – in which money wages began to rise before a satisfactorily high level of employment was reached, they refrained from recommending an inflationary sacrifice to secure employment gains. They preferred other policy tools – including "wage-price guidelines" – that could change the dilemma's terms. Forder argues correctly that this preference, widespread in other countries too, betrays the close relationship of macro-policy making in the 1960s to an already flourishing literature on cost-push inflation that had nothing to do with Phillips. Forder would dearly like to revive interest in this literature. Fair enough: it does form part of our discipline's history, though in my view, combined with the then widely underestimated capacity of monetary policy to generate demand-side led inflation, the ideas it promoted had a lot to do with what went wrong at the end of the decade.

On point (b), Forder is partly right. In the 1960s literature, explicit suggestions that the trade-off between inflation and unemployment could be treated as a constraint on policy, subject to which a social utility function in inflation and unemployment could to be maximised, are indeed few and far between. But Forder too quickly belittles this small literature's significance for the development of macroeconomics. He points out that the major contributions here were all Canadian, and hence – by inference, he is too polite to say so explicitly – were unlikely to generate much interest elsewhere. However, the second Canadian to construct a diagram in which such a function is constrained by a Phillips curve was none other than Richard Lipsey. He drew it at some time in 1964 while at Berkeley, during his transition from LSE to Essex, and presented the paper containing it at a conference there. Furthermore, Grant Reuber's original (1962) cost benefit analysis of the Phillips trade-off in terms of Harberger triangles and Okun gaps, was undertaken for the Porter Commission at the suggestion of another ex-patriot, namely Harry Johnson, then at Chicago but soon to add a chair at LSE to his appointments. Johnson invited Reuber to present his work at a money workshop in Chicago sometime in 1962-63, and published it the next year in the JPE. He also began citing Reuber in his own work in 1963, and continued to do so until at least 1970. Moreover, Ronald Bodkin, one of Reuber's collaborators on later Canadian analysis of these matters, had been tempted north from a position at the Cowles foundation, where in 1963 he had finally finished his University of Pennsylvania Ph.D. thesis, The Wage-Price Productivity Nexus – supervisor Laurence Klein, with whom he co-authored a related study for the Commission on Money and Credit. Evidently, then, these particular Canadians were a pretty well connected lot, so it is perhaps not surprising that in 1967, Phelps would cite one of them, Lipsey, as providing the static template for his own unquestionably influential dynamic cost-benefit analysis of inflation-unemployment interaction.

Incidentally, Forder casts doubt on the strength of the Johnson- Chicago connection in all this by speculating that the absence of a reference to Phillips 1958 paper in Johnson's still famous 1962 AER Survey of "Monetary Theory and Policy" indicates that he had not at that time so much as heard of it. This is implausible. Much more likely, the omission is explained by the fact that Johnson was aware that a companion Survey of Inflation Theory by Martin Bronfenbrenner and Franklin Holzman was planned, where Phillips' paper and its place in the then current literature would most likely be thoroughly discussed. As to Chicago more generally, in 1960-61, Johnson's colleague Friedman had already challenged his price-theory students – essentially the department's whole Ph.D. class, for whose instruction in macroeconomics Johnson was responsible that year – with a take-home question that mentioned recent work by William Bowen and Phillips, and then, among other things, asked them to explain why "Considerations derived from price theory give no reason to expect any systematic long term relation between the percentage of the labour force unemployed and the rate at which money wages rise". They knew all about Phillips' paper at Chicago from the outset, and Friedman, at least, was already worrying about that real-money wage distinction.

They knew about this paper elsewhere, too, which brings me to Forder's point (c). He argues that the paper was peripheral to Phillips main research on stabilisation policy (which, incidentally, Johnson did cite in 1962), that its execution was rough and ready, that it was much criticised by contemporaries and often justly so, that a huge and lively literature on money wage behaviour already existed in 1958, and that very few researchers directly followed up Phillips' own contribution. All of this is correct, but to infer, as Forder does, that Phillips' paper was "negligible" and had "very little impact", with memories of it living on mainly in the label "Phillips curve", is simply not true. It is not just that Lipsey and Samuelson and Solow followed it up in 1960, which facts Forder of course discusses. Rather, the point is that these papers were in turn widely read. Phillips' paper was still sufficiently well- known in 1963 that Bodkin (not published until 1966) could refer to it as "Professor Phillips' already classic piece", and in 1966 it was reprinted, (along with Samuelson and Solow) in M. G. Mueller's extremely successful collection of Readings in Macroeconomics meant for advanced undergraduates. And in his 1972 AEA Presidential address, James Tobin, while recognising that the idea of an inflation-unemployment trade-off had a long and complicated history before and after 1958, nevertheless referred to Phillips paper (not his curve) as "probably the most influential macro-economic paper of the last quarter century"; and so on. Evidently, negligibility and lack of impact come in many forms.

Forder is on firmer ground with his point (d), but again he pushes his conclusions too far. Of course, economists were sensitive to the difference between real and money wages before 1968. But, as another of those Canadians, Stephan Kaliski, suggested in 1964, they worked with money wages, and let the data choose the coefficient taken by prices (or their rate of change) in their equations, because they thought that the presence or absence of money illusion in the labour market was an empirical question, not to be settled a priori. Friedman's above-mentioned take-home question shows that he put much more faith than most of his contemporaries in the propositions of simple price theory, and he along with Phelps deserves more credit than Forder grants them for finding a way to reconcile this faith with a body of empirical evidence that had long seemed to contradict it. A similar comment applies to the matter of inflation expectations. Of course, the problems they posed had been widely recognised since the 19th century. More specifically, accelerationist ideas were indeed very popular in the 1950s, particularly in what we would now call "Austrian" circles. But all too often this analysis seemed to predict too much – that creeping inflation always threatened to break into a trot before taking off into a gallop. Long before 1967-68, empirical evidence about the potential durability of low but ongoing inflation, some of it of course presented by Phillips (1958), seemed to have discredited such arguments. Thus, it was also a major part of Phelps' and Friedman's accomplishment to clarify, with the aid of that unfortunately (in my view) labelled concept, the "natural unemployment rate", the circumstances under which accelerationist predictions driven by theories about the endogeneity of inflation expectations were and were not likely to be relevant.

So where does all this leave Forder's suggestions about the origins of today's Phillips curve myth – point (e) above? Surely, his denials of an important role to confused thinking about real and money wages in the macroeconomic theory of the 1950s and 60s, and of a critical influence of this theory on the subsequent conduct of policy, stand up very well indeed. By and large, the economists of the period were indeed neither stupid nor confused, nor did policy-makers implement misguided advice about trying to buy more employment with a little inflation. But some economists, being a little too confident in the prevailing empirically based ideas about how the macro-economy worked, did offer that kind of advice. To be sure, these were in the minority of what was then a much smaller profession, but they got a wider hearing among their academic colleagues than Forder suggests. As a corollary, for Phelps and Friedman to have challenged their ideas theoretically in the late 1960s, in time to produce a body of analysis capable of analysing the experience of the early 1970s as it was happening, was also a more important accomplishment than he is willing to allow.

The history of the Phillips curve was nevertheless not quite as Friedman sketched it out in his Nobel lecture, and it is a matter for real regret that, with a big boost from the rational expectations revolutionaries whose influence on macroeconomics would so soon become dominant, Forder's "myth" has become so widely accepted as the truth. This has been good neither for the discipline nor policy. But Friedman did not simply make up his account. It had a basis in reality, albeit a partial and selective one, perhaps coloured by the simple fact that among his colleagues at Chicago were Harry Johnson, a persistent advocate of the policy trade-off view of the Phillips curve in the 1960s, and Robert J. Gordon, the author of a series of decreasingly sceptical empirical studies of the Friedman-Phelps version of the relationship in the early 1970s. In laying out many of the complexities and ambiguities in what the record suggests actually happened across the profession more broadly, Forder seems to me to have sometimes given insufficient consideration to those parts of the story that give a little support to Friedman's account, and to the fact that some of them would have been highly visible to him at Chicago. But if his book is not the last word on these matters, it is nevertheless a considerable accomplishment and well worth reading.