Friedman's *Presidential Address* and related matters.

I tried to give full, and very substantial, credit to Friedman for the brilliance of much of his work in my book on him, and in Forder (2022a). But much of the praise commonly heaped on him arises from the supposed innovation contained in Friedman (1968), his Presidential Address to the American Economic Association. That paper is, in my opinion, a piece of rubbish. Certainly, the story that an ongoing inflation will come to be expected and incorporated in wage bargains was nowhere near original. Making that case is a central objective of my work on the Phillips curve. So that is the end of Friedman's paper being brilliantly original. The point that it is in addition generally rubbish was one I started on in Forder (2016a) where I pointed to a minor inconsistency in his exposition right at the heart of his supposedly key argument. "Who cares about that?" cried a referee, correctly noting that the point was easily corrected without damage to Friedman's argument. "Forder has written a whole paper on something worth no more than a footnote!", I was told. Fortunately – so I thought – the journal editor was wiser and could see that the big question was "Where is that footnote?" This paper supposedly destroyed the Keynesian consensus. Supposedly it was the most influential paper in macroeconomics. Surely everyone read it? Friedman was a controversial figure, often thought to use slightly dodgy tactics in arguing. Is it really to be believed that there is a glaring mistake in it, and none of Friedman's detractors stopped even to write a footnote to make fun of him? It is more likely, I said, that the paper was in fact not widely read, and those who did read it thought it generally of little or no importance.

I might in the end have been a bit unlucky with my editor, since my paper contained a minor mistake (not relating to the main theme), which I corrected in Forder (2017), but more importantly, Friedman's paper contains a whole clutch of little mistakes. I had found the one at the heart of the (supposedly) key argument, but there were others scattered through the paper. Of them, *one*, seems to have been previously noticed. But the point is now more powerful – why did none of Friedman's detractors even write a footnote on these things? As it happens, it is clear that Friedman himself knew the paper was not up to much. In the early years after its publication he hardly cites it himself (and never for its supposed great innovation), and then in Friedman (1972) he wrote a very much better paper on very much the same theme. One cannot read the two paper and continue to think that the earlier one is any good. (One could continue to think that the early, inferior one contains the innovation, but that is simply wrong as a matter of historical fact). I compared the two papers in detail, making this case in Forder (2018a).

A further point is that if the story about the Phillips curve had been an innovation, or had been anything to do with making Friedman (1968) an important paper, that would be apparent from the paper and Friedman's discussions of it. The Phillips curve, or expectations, would, for example, be mentioned in the introduction; or failing that, in the conclusion. Or Friedman would have drawn attention to the paper in works published shortly after, etc, etc. But it is not so: The paper is simply *about* something else entirely, as I argued in Forder (2018b). The obviously does lead to the question as to what explains the paper becoming so highly regarded (and highly cited). Part of the answer is offered in Forder and Sømme (2019), where the contingencies of the development of thinking are considered. A crucial point is that Friedman's argument on expectations was not the original statement of that point, but on the contrary, at the time other seminal papers were written, it was the most recent. That led it its being cited in those papers, and others followed them. Another part is addressed in Forder (2021) which considers more generally why some papers are disproportionately well-cited and what lessons one might take from that.

Related to this line of thinking is the question of Friedman's Nobel Prize and what it was, in particular, that earned it. It was awarded in 1976, which is just the time that the Phillips curve myth was becoming established. One part of the citation points to his work on "the complexity of stabilization policy". That is the nearest it comes to suggesting the importance of anything he had said about the Phillips curve, and it is not very close (since the expectations story itself either leaves no room at all for such policy, or makes it very simple). In Forder and Monnery (2019), we argued that looking back on Friedman's work, there was a clear (though later neglected, except by connoisseurs) strand of work precisely on how difficult stabilization policy is. That definitely confirms that it would be a mistake to see that part of the Nobel citation as making reference to the Phillips curve. So again, it is apparent that at that time, Friedman (1968) was not seen as a path-breaking paper. (Interestingly, in the Nobel presentation speech – just a little later than the citation – a mythical version of Friedman's contribution on the Phillips curve was described. That is good dating-evidence of the emergence of the myth).

Friedman (1968) does mention the Phillips curve, of course, and it does make the expectations argument. But I argued in Forder (2018c) that its target was not any idea that stabilization policy might have been being organized around "choosing a point on the Phillips curve". Rather, the target was what elsewhere I called the "lubrication argument". The idea, fully though not originally, considered by Schultze (1959) and then muddled-in with a Phillips curve story by Tobin (1972) is that a gentle inflation allows more rapid equilibration of the real economy by smoothing away nominal frictions. It turns on the idea that for some reason – presumably a sociological or psychological one, rather than a fully-rational one – the number zero has a special power. So, an employer in financial difficulties can refuse a nominal wage increase much more easily than it can impose a nominal reduction. This argument remained commonplace well into the 21st century, becoming a great favourite of central banks explaining why they target 2% or so, rather than price stability. But Friedman's point was that the magic of "zero" might itself change in conditions of very long term steady inflation. If 2% comes to be the well-established norm, it might be that this amount becomes the crucial reference. Then the same smoothing can only be achieved at this rate of inflation. If the smoothing policy continues, then, that makes for a long-term acceleration of inflation. The difference between this and the usual argument about the Phillips curve is that here the work is done not by some calculation of the currently-expected rate of inflation, but rather just an appreciation of what is normal. It is interesting that central banks, who take so much rhetorical support from Friedman's supposed demolition of the Phillips curve are continuing to advance the argument he was in fact questioning.

In Forder (2016b) I sought to put the record straight on how much actual influence Friedman had on economic policy in Britain. If it is direct, personal influence we are interested it, arising from his supposedly close connections to Margaret Thatcher, that is another myth. He had practically none. In Forder (2022b) I argued that the "monetarist revolution", or "Friedman's monetarist revolution" owes rather more to Karl Brunner than is commonly recognized, and Friedman’s appreciation of its theoretical character and weight was rather limited in comparison to Brunner’s.

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