

# Explaining the fame of Friedman's Presidential Address\*

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22 December 2018

## Abstract

It is noted that although in fact it lacks the revolutionary content commonly ascribed to it, Friedman's *Presidential Address* to the American Economic Association is very highly regarded as an original and formative contribution. It is argued that close attention to the literature shows it was not initially seen as original, and only after an interval of five years did the idea of its revolutionary status retrospectively, but suddenly become widely accepted. The explanation of this change of view is considered. Four explanations are suggested: one involving terminological confusion; one involving a change of theoretical priorities; and two involving debates of the 1970s which, although they did not in fact do so, appeared to build on Friedman's presentation, and by this appearance gave it an undeserved stature.

## I. Introduction

Since it is not just that it is highly cited, but also that it is so often and so firmly declared to be such an important paper, the repute of Friedman (1968) is surely not to be doubted. Mankiw and Reis (2018, p. 82), for example, contributing to a celebration of its 50<sup>th</sup> anniversary, said, 'it is no exaggeration to view Friedman's 1967 AEA presidential address as marking a turning point in the history of macroeconomics research'. Gordon (1981) thought it probably the most influential article in the previous twenty years; Blaug (1985, p. 681) went much further, saying it was 'easily the most influential paper on macroeconomics published in the post-war era'. The same views extend to those who are far from having a general inclination to agree with Friedman. Krugman (1994) called it a 'decisive intellectual achievement'; and Palley and Vernengo (2018), noting Friedman's undeniable importance, said that in their view, his Presidential Address had been his most influential contribution.

Commonplace as this sort of thing is, the views expressed arise from entirely mistaken ideas about the history of macroeconomics and indeed about the content of Friedman's paper itself and its place in Friedman's work. Though their ideas of the details may vary, all these authors took it for granted that the great innovation of Friedman (1968), perhaps along with one or other of various works of Phelps, was to introduce the idea of a 'vertical Phillips curve' as a challenge to the prevailing Keynesian orthodoxy, and in pointing out that as expectations of inflation developed, unemployment would return to equilibrium.

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\* This is a revised version of a paper presented at the History of Economics Society Conference at Chicago in June 2018. We are grateful to participants there, Roger Backhouse, and two referees for comments.

In Forder (2014a) it was argued that the story from which this kind of view is derived is a fiction. The particular point that if inflationary policy were pursued, wage bargainers would adjust to it, and any tendency of the policy to reduce unemployment would dissipate was shown to have been very widely appreciated for decades. Indeed, Phelps (1967) himself noted that point had been made before. In terms of making the point that the unemployment-reducing effects of inflation would be temporary, Friedman and Phelps therefore brought nothing of substance to the discussion.<sup>1</sup> It might of course be said that albeit that the high regard of Friedman's paper arises from a myth, it is still the myth that explains the regard. But that is not quite satisfactory because the supposed status of Friedman's paper is also one of the things that maintains the myth. Some further explanation of the development of these ideas is therefore wanted. This is all the more true when it is recognized that, as argued in Forder (2018a), Friedman's paper was not even principally about the Phillips curve at all, and far from being original in any other way, most of it is little more than a rehash of views Friedman had previously stated; and as argued in Forder (2018b), it is not very good either, being full of little mistakes of various kinds.

A closer enquiry into origins of the idea that Friedman's paper was pathbreaking reveals that it emerged not with the appearance of the paper, but very suddenly, five years later. It is a striking fact, but our detailed examination of the literature in part II shows that there was initially essentially no sign of Friedman's view being taken as theoretically novel. Yet in part III it is easy to show that idea emerged very clearly in 1973 or 1974. That point is of some wider interest since it means that this claim of Friedman's original theoretical insight very slightly predates the appearance of the story that policymakers had in fact sought to lower unemployment, found by Forder (2014a, p. 158-159) to appear only in 1975 and 1976.

The suddenness of this stark change of attitude clearly suggests that the explanation be sought in events around 1972 and in what follows we suggest four considerations. The first, considered in part IV, is simply that there is room for terminological confusion; the second arises from the perception of the 1960s that there might be benefits specifically of gentle inflation and how that idea may have affected the perception of aspects of the argument over the Phillips curve. That issue is considered in part V. Part VI and VII then consider two strands of literature which referred directly to Friedman's paper. Each of them, we argue, put Friedman's paper in a role in the development of thinking that it did not really have, but in each case that served to enhance the apparent importance of the paper.

## **II. Responses to Friedman's paper before 1972.**

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<sup>1</sup> It could of course be argued that some other aspect of their work is original and important. The quality of Phelps' analysis is not to be doubted, and it has been powerfully argued by de Vroey (2016, pp. 102-108) that Friedman's paper is notable for introducing the idea of expectations of inflation differing between employers and workers. Whatever the merits of such arguments, this is not the reason Friedman's paper is commonly held in such high esteem. In seeking to explain that standing, therefore, we consider its supposed role in introducing the expectations argument.

Certain signs that Friedman (1968) and hence the supposedly-novel expectations argument, were not initially thought important were noted in Forder (2014a, p. 87-89). Brechling (1969, p. 161), for example, clearly recognized the argument as an old one, describing it as part of a 'long tradition in macroeconomics'. And in Forder (2018a, p. 535-538) it was shown that Friedman himself showed no sign of thinking the paper important until Friedman (1975) – *after* others had started to describe it that way. One point, for example, was that in Friedman (1969) – a collection of his papers – it was hidden away as chapter 5 (of thirteen).

A more detailed examination of the literature strikingly confirms this picture. The reviewers of Friedman (1969) evidently saw nothing of interest in the paper. Siaens (1969) did not mention it; Gurley (1969), in the *Journal of Economic Literature*, mentioned it only to say that it was included; Stein (1970) discussed only the title essay, though that shows what he thought important. Chow (1970) described Friedman (1956), which was in the same volume, as 'the classic paper that revived the interest of the profession in monetary economics' (p. 687) and then mentioned Friedman (1968), but only to say that Friedman suggested that opinion might have gone too far in thinking monetary policy powerful. Hahn (1971) said that Friedman took the view that long run equilibrium was independent of monetary policy, and criticized that view, but gave the expectations argument neither favourable nor unfavourable mention. Only Hicks (1970) had anything resembling a judgment on the Presidential Address as a whole, and that was that Friedman presented his views on monetary policy 'in what, up to a point, is a persuasive manner'. (p. 669) As to what those views were, he said nothing, presumably thinking them well-known and not, therefore, of revolutionary or any novelty at that time. Taking them together, there are about 55 pages of review of the book, all published by the end of 1971, and they contain not so much as half a line on the expectations argument.

When, in the first few of years after its publication, Friedman's paper was discussed – was mentioned, really, since more than a mention is a real rarity – it is never clearly said to have been a revolutionary paper. On the other hand, as regards the expectations argument, there certainly are cases of its being well-recognized – quite correctly – as putting an old idea. Akerlof (1969) considered whether, because of the benefits of contracting in nominal terms, the survival of that practice at low but positive inflation might make for a limited trade-off. The substance of the argument may be important in its own right but his reaction to Friedman's paper is instructive. He attributed to Friedman the view that despite price stickiness and the like, unemployment would be invariant to inflation, and then observed (p. 354 n2) that Lindahl (1939) had said, 'anticipated changes in the price level have no economic relevance, since they neither influence the relative prices of factors of production and consumption goods, nor the extent and direction of production.' He was therefore citing Friedman as a recent statement of a view on which he wished to comment – there was no suggestion of Friedman's statement being interesting except that it was recent.

Akerlof and Stiglitz (1969) were less explicit, but nevertheless clear that the expectations argument did not originate in Friedman (1968). They noted that interest in the Phillips curve had led to much investigation of the short run relation between wages and unemployment, and that although there had been less consideration of the long run relation there were several formulations, of which they considered two. Their first was, as they put it, (p. 271)

based on the presumption that in the long run, individuals will not suffer from money illusion, so that the wage bargain will be made in effect in real rather than money terms. (p. 271)

They quickly gave the idea a mathematical representation and, citing Friedman (1968) they said that the equilibrium rate ‘has been called the “natural” rate of unemployment by Friedman.’ Here, their appreciation is precise in that the idea of the adjustment of expectations was routine, but Friedman notable for having introduced a piece of terminology (although in fact its first appearance was in Friedman (1966)). As their second formulation they described what they called ‘More recent studies’ (p. 271) which, whilst also supposing there to be a determinate equilibrium level of unemployment saw wage change as being related to the rate of change of unemployment. Here they cited Kuh (1967) and the unpublished de Menil (1968).<sup>2</sup> If they were ‘more recent’, then the less recent must have predated Friedman (1968). The fact that in that case they apparently saw no reason to cite any specific source is of course an indication that they regarded it as well-known, but clearly Friedman was not regarded as original.

Leijonhufvud (1968) discussing Brechling (1968) referred to ‘Friedman’s argument’ (p. 739) that there was no long run trade-off, which might seem to suggest it was original, but he also said it had ‘all the authority behind it of the well-established basic propositions of price theory’ (p. 740), and later referred to it as ‘the standard argument against the “meaningfulness” of Phillips curves’. (p. 742) Tobin (1968, p. 49) said there had been a ‘series of attacks’ on the idea of a Phillips curve trade-off, and ‘the previous one’ – not the first – to which he had been ‘exposed’, was Friedman’s. Rees (1970, p. 227) – disputing the significance of the expectations argument – cited Friedman and Phelps as its ‘strongest proponents’; and Streit (1972), describing Friedman as attacking the Phillips curve said that the point about expectations had been implied by Lerner (1951).

Various early works of Gordon are particularly interesting because he worked so much on the Phillips curve in the 1970s and indeed, in due course, came to attribute great importance to Friedman (1968). Early on, though, he did not do that. In Gordon (1970), he referred to ‘the accelerationist hypothesis, associated in the professional economics literature with the names of Milton Friedman and Edmund Phelps. Presumably the implication is that there were other sources outside the profession. Gordon (1971) said there was a debate about

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<sup>2</sup> It is unfortunate that these authors treated Kuh and de Menil as offering ‘formulations’ of a view about ‘the Phillips curve’. The former actually described his work as an alternative to the Phillips curve. Such terminological laxity became commonplace, leading to much confusion of which a sample is highlighted in Forder (2014b)

expectations and cited Phelps (1968) and Friedman (1968) – describing the former as ‘a theoretical labor market model’ and the latter as ‘a model in which the expected increase in prices plays a dominant role’. Each of those clearly avoids attributing originality to those authors. Gordon (1972) was a discussion of ‘the shifting Phillips curve’ which again considered ‘the accelerationist hypothesis’ but made no mention of Friedman or anyone else as having proposed that idea. Gordon (1975) on ‘The demand for and supply of inflation’ was less concerned with assessing the expectations view, but again made no mention of Friedman (1968). None of that gives any hint that he thought Friedman’s views important or original.

There are then also early mentions of Friedman (or Friedman and Phelps) where nothing is actually said that clearly indicates whether the author did or did not think his argument original. Hansen (1970), for example, discussing the effect of excess demand on wages observed that there was no difficulty in introducing expectations, and appended a footnote simply identifying Friedman (1968) and Phelps (1968) but said nothing more about them. Goodhart and Bhansali (1970, p. 83) cited Friedman alone, in a footnote directing attention to him for discussion of the point that the simple Phillips curve seemed to imply the existence of money illusion. Grubel (1970, p. 322 n10) said, ‘The concept of the Phillips curve and the implied trade-off has recently been challenged’ by Friedman and Phelps.

Since there is nothing in these remarks that clearly indicates that the authors concerned recognized the argument was an old one, they might be thought to suppose Friedman’s originality. That, however, would be a poor reading. The supposition is that Friedman and Phelps had advanced a revolutionary argument, certainly a surprising one, and perhaps even a shocking one. If that were the case, authors a year or two later, whose views were being revolutionized, or were surprised or shocked, would be expected to give some clear sign of that reaction. In any case, since it is a plain fact that Friedman and Phelps were not original in the relevant way, one would have to suppose these authors to be clearly in error. An uncharitable reading like that is out of place when there is nothing resembling a clear basis for adopting it. So these sorts of mentions do nothing to dispel the impression of Friedman and Phelps not being thought original.

Of all the earliest references to Friedman (1968), the one that comes closest to attributing striking novelty to him seems to be Laidler (1971, p. 95-96 n9). He noted the antiquity of discussion of expected inflation, saying it went back at least to Fisher (1896) and that the first ‘explicit’ discussions of it in ‘modern monetary economics’ were by Friedman (1956), Cagan (1956), and Bailey (1956), (all of which concern the demand for money) but then said, ‘It was introduced into the Phillips curve literature by Phelps (1967), who has also used the concept in work on monetary problems (1965), and Friedman (1968)’, also saying that Parkin (1970) was the only study mainly of the UK to use the idea.

He clearly meant to distinguish discussions of the effect of expected inflation on the demand for money from other ideas. If those other ideas are identified as the

effect of expected inflation on wage bargaining, then he must have been attributing that innovation to Phelps and Friedman. In that case, what he said is incorrect, but nevertheless it would be an attribution of originality to Friedman. On the other hand the remark might be read as being specifically about 'the Phillips curve literature', rather than the broader argument that inflation expectations matter to wage bargaining. The distinction may appear fussy, though as we argue in Part IV, it may well be of broad significance in understanding views of Friedman's paper. In any case, the most striking aspect of Laidler's discussion is that it was in an endnote. Other theoretical points about the development of the literature were in the main text. So it hardly seems likely that he meant to suggest the point was a fundamentally new one. But whatever interpretation is put on Laidler's treatment, there seems to be no one else in the first five years after Friedman's *Address* who described it as containing a novel argument.

That sample is by no means all the evidence that might be offered. There are plenty more cases like those – none, it seems, regarding Friedman as notably original. And then, of course there is evidence of a different kind because there are all those who make no mention of him at all, or do mention him, but make no reference at all to his ideas on expectations. That is already apparent in some of the works of Gordon, but otherwise, the sample of works so far considered, since they at least mention him, one would think would be heavily biased towards finding his paper important. Further evidence against his work being seen as original comes in the form of Johnson (1971), in his often-noted study of the 'monetarist counter-revolution' as compared to the Keynesian revolution. He discussed Friedman (1956), Friedman (1970b), and Friedman (1970a) but not Friedman (1968).<sup>3</sup> There is even Parkin (1970) – the one cited by Laidler (1971) – who made no mention of any specific inspiration. Similarly, Perry (1970) was an important investigation of the inflation-unemployment relation in the United States. He argued that its deterioration at the end of the 1960s could be explained by demographic factors in that the period saw an unusually high proportion of secondary workers – young people and married women – in the labour force. His idea was that when unemployed, they exerted less downward pressure on wages so that a high measured level of unemployment had less disinflationary effect than it would have done at earlier times. He considered the question of changing expectations as an alternative explanation, rejecting it on the basis of his econometrics, but saying nothing at all about where the idea came from – it was simply 'the accelerationist view'. (p. 430)

It is apparent, then, that perceptions of Friedman (1968) up to 1972 are worlds away from those of 2018, or of the 1980s or 1990s. There is next to nothing in

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<sup>3</sup> Johnson (1972) did mention Friedman (1968) to say that it was part of the 'restoration of respectability to the quantity theory' in that it argued that monetary policy could affect monetary but not real values (p. 61-62), and that his 'criticism of the Phillips curve for ignoring the influence of expectations' (p. 65) had led to some serious work. The first certainly does not suggest it was a revolutionary work, and it would be a sign of desperation to argue that the second does. As argued Forder (2017), Johnson was palpably inconsistent, and presumably confused, over the Phillips curve, but that point does nothing to make his remarks into evidence that Friedman (1968) was seen as important in those years.

that earlier period to suggest that Friedman's paper was recognized, or seen, or even suspected of being one of dramatic innovation. The impression of the later authors, then is not merely mistaken, but also unrepresentative of the way the paper was seen when first presented.

### III. Responses to Friedman's paper after 1972

It is not, however, that the responses of 1971 or 1972 are so different from those of the 1980s and after that is the interesting thing, but that they are so different from those of 1973 or 1974. Rowley and Wilton (1973), for example, wrote that in response to the idea of a stable Phillips curve,

a strong theoretical attack, stressing the role of expectations, was launched by Friedman (1968) and Phelps (1967), and others, denying the theoretical existence of a stable trade-off curve. (p. 90)

There, there is no arguing that all that is meant is that they brought some old idea specifically to the Phillips curve literature because the attribution to Friedman and Phelps of a particular motive or attitude is so clear, and here the suggestion is surely that the 'others', whoever they may be, came after Friedman and Phelps. The point was clearer yet in Hall (1974) – indeed, it was completely unambiguous – when he referred to 'The Acceleration Theorem, discovered and advocated by Friedman and Phelps', citing Friedman (1968) and Phelps (1970a).<sup>4</sup>

A particularly striking case arises from the award of Friedman's Nobel Prize in 1976. The brief citation itself read, 'for his achievements in the fields of consumption analysis, monetary history and theory, and for his demonstration of the complexity of stabilization policy'. The intended referents of the first two items are clear enough and mention of the Phillips curve and expectations might have been expected to follow. But the third item can hardly be that. For one thing, Friedman's discussions of it have nothing to do with the *complexity* of stabilization policy. If Friedman's point is taken to be that points of low unemployment cannot be sustained, then it is about its impossibility; if it is that a money growth rule will achieve stabilization, then it is simplicity itself. But in any case, other work – notably Friedman (1951/1953) – fits the description much better.<sup>5</sup> So at the time this citation was written, Friedman (1968) did not feature. Yet in the press release which accompanied the announcement of the prize, it did.<sup>6</sup> There, the three things listed in the citation were discussed, followed by mention of the expectations and Phillips curve, and then flexible exchange rates. It said he was 'the first' to demonstrate the transitory character of the Phillips curve tradeoff. The press release no doubt had different authors, but would certainly have been written slightly later than the citation. Whether

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<sup>4</sup> Hall's case is an interesting one because in Hall (1970) he gave some sign of accepting the existence of a stable tradeoff while saying Friedman had challenged that view – but there, it was not just Friedman, but 'Milton Friedman, Edmund Phelps, and a number of other economists...' (p. 370 n3)

<sup>5</sup> The question is explored more fully in Forder and Monnery (2018).

<sup>6</sup> [https://www.nobelprize.org/nobel\\_prizes/economic-sciences/laureates/1976/press.html](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1976/press.html)

the change between the citation and press release is itself a symptom of the speed with which responses to Friedman (1968) changed probably cannot be determined. But clearly by the time the press release was written, the idea of its importance of Friedman's innovation in pointing to the importance of expectations had become well-established.

It is not too much of a surprise, then, to find Fischer (1977, p. 192) saying,

The distinction between the short- and long-run trade-offs formed the basis for the originally startling natural rate hypothesis of Friedman (1968) and Phelps (1967) which argued that, while there was a short-run Phillips trade-off, there was in the long run a natural unemployment rate, independent of the steady state rate of inflation.

In the same year, Frisch (1977) rhetorically asked, 'Is the Phillips curve a steady-state situation, so that a stable trade-off between the rate of inflation and the rate of unemployment exists?' and said that Friedman (1968) and Phelps (1972) were 'the first to question this interpretation' and said 'They introduced the distinction between *anticipated* and *unanticipated* inflation into the analysis' (p. 1294).

It is hardly necessary to cite more cases, though it would be easy to do so. These sorts of views, although they certainly cannot be said to be universally held, were clearly widespread, and as shown in Forder (2015), the idea that Friedman and Phelps had made strikingly original contributions spread into the textbooks over the same period. Though there seems to be nothing like that in the literature up to 1972, by 1977 – just another five years after Friedman's Address – not just the idea, but his supposed originality, was ordinary, routine material that students should know.

#### **IV. 'Expectations' and 'The Phillips curve'**

One part of the explanation of this rather sudden transformation may be that there was a change in terminology around about 1970 and 1971 which may have introduced some confusion to the matter. As further described in Forder (2014b), in the 1960s the expression 'Phillips curve' was applied mainly to specific econometric wage-change equations, but right at the start of the 1970s, it became commonplace to apply it abstractly, and to the relationship between inflation and unemployment. So as well as there being a new collection of econometric 'Phillips curves' which were actually inflation-equations, the expression also became a label for a general theoretical idea, and hence much more widely used than it had been.

Friedman (1968) and the earlier of the Phelps papers appeared just before this change took hold. Consequently, they also became the first widely noted papers to conjoin the terminology of 'the Phillips curve' with the question of 'expectations'.<sup>7</sup> The econometric studies of the 1960s had clearly recognized the

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<sup>7</sup> They were not quite the first because of Samuelson (1965), Wallich (1966), and then Friedman (1966).



importance of price change in wage setting, but the matter was almost never put in terms of 'expectations'. There was no reason it should have been since those authors were not addressing the question of what would happen if there were steady, ongoing inflation.<sup>8</sup> On the other hand, in theoretical discussions, whilst it was often and clearly recognized that if there were ongoing inflation its relationship with unemployment would shift, that was never put in the language of 'the Phillips curve'.<sup>9</sup>

There is room, therefore, for confusion to arise either because by being the first to discuss the relationship of 'expectations' and 'the Phillips curve', Friedman and Phelps appeared to be introducing a theoretical innovation. As we noted above, the footnote in Laidler (1971) may have been intended to make precisely the point that they were the first to put the matter in that vocabulary. Certainly, during the 1970s and after, as observed in Forder (2014b), no notice seems to have been taken of the confusions creeping into the terminology, so it is not hard to see that remarks along the lines of 'Friedman and Phelps were the first to consider the effect of expectations on the Phillips curve' could easily have led to misunderstanding, and to the idea that those authors had made much more significant contributions than they had.

## **V. A *strictly* vertical Phillips curve**

An aspect of Friedman's discussion of the Phillips curve is that he may have appeared to be firmer than others in insisting that the long run Phillips curve is strictly vertical. Although, as noted, and contrary to what has often been implied, those estimating Phillips curves in the 1960s routinely included a measure of price change as an explanatory variable, it was rare that they found it to have a coefficient near to one. Authors' acceptance of such a result might seem to suggest naivety on their part, but there is much more to it than that and a number of the explanations offered in the econometric literature are considered in Forder (2014a, ch. 3 pt. 2).

One argument – prominently, though not originally, made by Schultze (1959) – was that gentle inflation could have the effect of facilitating labour market adjustment. In circumstances where there were continuous random shifts of demand from sector to sector, and nominal wages were sticky downwards, inflation would make it possible for real wages to fall in temporarily contracting sectors without those nominal wages needing to be cut. In that case, the inflation would make it possible to avoid the unemployment that would otherwise be necessary to reduce wages, and as was sometimes said, inflation had the effect of 'lubricating' labour market adjustment. One way of looking at that was that if aggregate demand were at a level appropriate to supply capacity, so that there could be full employment, nominal wages would be rising in some sectors and constant in others. If the effect were large enough (so that wage increases were not covered by productivity gains) there would then be inflation. That inflation could be stopped by reducing demand and increasing unemployment, so there

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<sup>8</sup> This argument is made fully in Forder (2014a, ch. 3).

<sup>9</sup> This is apparent from the accounts of those discussing expectations in Forder (2010) and Forder (2014a, ch. 4 pt 1).

would be a 'tradeoff'. But that tradeoff was due to whatever explained downward wage stickiness, not expectational error, so there was, on the face of it, no reason for inflation to accelerate, and the coefficient on price change in a Phillips equation would be less than one.

In Forder (2018c) it was suggested that arguments along the lines of Schultze's may have been the intended target of Friedman's (1968) remarks about the Phillips curve. The idea that he meant to say that ongoing inflation would convert downward nominal wage rigidity into downward real wage rigidity may be the best way of making sense of his otherwise peculiar idea that adjustment to ongoing inflation would take a matter of decades. In any case, whatever the intent of his detailed argument, some readers clearly thought Friedman's position was distinguished by the view that the long run Phillips curve should be *strictly* vertical.

Smith (1970) was clear on the point, saying that Friedman was the source of a revival of Classical theory and described a model of the Phillips curve said that the crucial issue was the value of the parameter on inflation expectations, and attributed to Friedman the view that it was one. A little earlier, Solow (1968) who, having drawn attention to Friedman's view, said that he could accept that a very stable and high inflation rate would be incorporated in wage bargains, but it was not clear that the position was the same with low and fluctuating rates. He had no apparent resistance to the idea that inflation expectations mattered, but could well be said to be rejecting an extreme view – Friedman's view – of that question. Similarly, when Donner (1972, p. 17 n4) described him as a supporter of the 'strong' expectations hypothesis, he surely meant to distinguish Friedman's view from a more general acceptance of the importance of expectations and to say that Friedman's view was more extreme.

In the econometric literature, Ashenfelter, Johnson, and Pencavel (1972, p. 47) singled out Friedman as authority of the view that in the 'normal unemployment hypotheses', the coefficient on expected inflation in a Phillips curve equation is one; Turnovsky (1972, p. 4) contrasted the views of those who thought the coefficient should be 'approximately one' – namely Friedman, Phelps, and Cagan – with what was then the evidence that it was substantially less than that. Although it is a later case, something of the same indication might be found in Gordon (1977, p. 265). He reported that – contrary to his own earlier findings – that the 'natural rate hypothesis' was confirmed. That was clearly intended to concede a point to Friedman, but since Gordon had previously accepted the relevance of expectations, and anyway did not attribute that idea to Friedman, it was not that point being conceded. Rather, 'the natural rate hypothesis', was presumably intended to identify the view that the Phillips curve was strictly vertical.

Viewed as presenting this 'extreme' position, there is nothing in Friedman (1968) which would properly be called 'original' and certainly not 'strikingly original', but in rejecting the lubrication argument (or other arguments to a similar effect) there is something that was distinctive to him. The lubrication

argument was very widely noted, and almost as widely approved,<sup>10</sup> so this interpretation of Friedman surely put him in minority. As things turned out, though, inflation quickly rose to levels where the lubrication argument had no relevance and it disappeared from discussion almost completely. In those circumstances, the question of whether the Phillips curve was vertical took on a different aspect. It is easy to see that from the later point of view, with the lubrication argument ignored or forgotten, and with Friedman appearing to be the first to have addressed 'expectations' and 'the Phillips curve', he would have seemed prescient in describing what was the only reasonable position.

## **VI. Tobin's Presidential Address**

The lubrication argument, as it happens, also very much featured in Tobin's (1972) Presidential Address. As Dimand (2018) noted in the course of describing Tobin's strategy in detail, he overtly presented himself as responding to Friedman, and said he was offering a presentation of macroeconomic debate around Keynesian economics and leading to a 'view of the world suggested by the Phillips curve', (p. 2) which had no specific level of full employment, but a trade-off, and which he said was an analogue of Keynesian theory. Tobin clearly associated the idea of the natural rate of unemployment with Friedman, and rejected it, saying it was the contemporary version of the outlook Keynes attacked. The basis of his view was an elaborate version of the lubrication argument, and Tobin gave something of an impression of that being a novel argument too.<sup>11</sup>

In all this, Tobin surely created the impression of specifically Friedman (1968), not any of Friedman's other work, being a crucial paper – if not *the* key paper – challenging the Keynesian view he was advocating. By creating an impression of his own response being more original than it was, and as capturing something distinctively 'Keynesian' in answer to Friedman, he can only have made the debate between himself and Friedman seem more central to macroeconomics than it was, and hence to have enhanced the appearance of Friedman's paper even more. And of course by his choice of language, he appeared to link both papers and the debate between them to the 'Phillips curve' literature of the 1960s.

Others seem clearly to have followed Tobin's lead in treating this outline as describing the state of debate, and certainly in seeing Friedman (1968) as a pivotal paper. Feige (1972) cited Friedman for the idea of the natural rate of unemployment, before saying it offered little practical guidance to policymakers, and then Tobin for the contrasting idea of a menu of inflation and unemployment rates. Steinmann (1973, p. 119 (English abstract)) specifically said he adopted Tobin's view that the Phillips curve and the natural rate were the post-war analogues of Keynesian and Classical views. Donner and Lazar (1973), who

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<sup>10</sup> There is plenty of evidence in Forder (2014a, pp. 77-79 and pp. 234-5n20).

<sup>11</sup> That point, and signs that Tobin's argument was taken by others as being original are considered in Forder (2014a, p. 183-4).

actually cited Tobin (1968),<sup>12</sup> adopted his view that the debate between the advocates of the Phillips curve and Friedman (1968) mirrored that between Keynes and the Classics. Havrilesky (1974) explained the search theoretic interpretation of the Phillips curve derived from Phelps (1970b), and accelerationism which he attributed to Friedman (1968) before citing Tobin (1972) as a Keynesian who denied that the Phillips curve was vertical. (p. 73) Then Rothschild (1974) presented Tobin as offering a response to Friedman's view and Kuipers (1975) cited Friedman for the idea of expectations adjusting and Tobin as his exemplar of those who denied that the Phillips curve was vertical.

All these authors – all from 1972 or very shortly after – saw Tobin as presenting an important view, and most see him in a crucial debate with Friedman – and specifically the supposed argument of Friedman (1968). But again, the rise in inflation that followed meant that in the end, Tobin not only promoted Friedman (1968) much more than it deserved, but having done so, found himself in a position where it seemed he was wrong, and Friedman was right.

## **VII. The origins of the rational expectations revolution**

Like Tobin, the authors of the earliest work in rational expectations macroeconomics also presented their work in relation to Friedman (1968), though of course in this case, not as opposing it, but as building on it. Later accounts invariably treat Muth (1960) and Muth (1961) as the fons et origo of the idea, which then lay dormant until Lucas (1972a) realised its potential, with Sargent (1973) and Barro (1976) other early, crucial papers in its application to macroeconomics. All three of these authors, though, indicated that their ideas were in some way developments from Friedman's.

Lucas (p. 103-4) said his results were suggestive of the views of Friedman (1968), and that his theory explained why changes in the quantity of money had (temporary) real effects, (p. 121) implying that they gave a better theoretical rationale for them than Friedman had. Then Lucas (1973, p. 326 n1) described Friedman (1968) and Phelps as offering 'The most useful, general statements' of the natural rate idea. Sargent (1973) started by asserting that 'The interaction of expected inflation and nominal rates of interest is a topic that has received its share of attention since Milton Friedman gave Irving Fisher's theory a prominent role in his presidential address to the American Economic Association'. (p. 429) He also referred to 'the Phelps-Friedman hypothesis of a natural rate of unemployment', and said that the fuller study of his rational expectations model of the interest rate in a general equilibrium setting depended very much on Lucas' work on the natural rate and that 'In important ways, the structure of the argument in this paper resembles that of Friedman's presidential address, in which the close connection between the hypothesis of a natural rate of

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<sup>12</sup> As already noted, the lubrication argument in Tobin (1972) was more elaborate than many other versions, but in general orientation had none of the originality Tobin seemed to imply. Actually, various aspects of his paper were foreshadowed in Tobin (1967) and Tobin (1968), the second of which put Friedman in a similar role to that of Tobin (1972).

unemployment and Fisher's theory of the real rate of interest was brought out.' (p. 434) Then Barro said that the model in his paper 'generates a Phillips-curve-type relation in a framework that builds on the work of Friedman (1968) and Lucas (1973)'. (p. 1)

There is nothing in any of those comments which is strictly incorrect, but the impression created by this kind of thing gives Friedman – and in particular, his *Presidential Address*, much more prominence in the pre-history of rational expectations macroeconomics than is appropriate. They could almost give the impression that no one had thought of the idea of the neutrality of money until 1968, whereas, as is apparent from Lucas (1972a) and argued by de Vroey (2016), the origins of rational expectations macroeconomics are methodological and econometric, and in both those aspects, far from Friedman's thinking.

It is clear, though, that later authors adopted the view that the rational expectations theorists were building on Friedman, and Friedman (1968) in particular. In one version, Laidler and Parkin (1975), although they had rather little to say about rational expectations, did comment that Lucas (1972b) and Lucas (1973) provided evidence supporting the natural rate hypothesis. (p. 769) That clearly put him in the role of following Friedman. Others, though, gave much more emphasis to a continuity of theoretical development. Barro and Fisher (1976) gave the Phillips curve a prominent position in their survey of monetary economics, and presented it as coming in various versions, one of which they said was rationalised by Friedman (1968), (p. 156) with Lucas and rational expectations clearly presented as offering a further development. (p. 158-160) Fischer (1977), in presenting what became a widely-noted model in which overlapping wage contracts generated Phillips-type results in a rational expectations framework, traced the development of theory from Phillips, to Friedman and Phelps, and thence to Lucas. He described Friedman's argument and then commented, without elaborating, that Phelps and others had provided better rationales for the same effect. So there, Friedman's centrality as originator of a line of thinking is clearly implied, notwithstanding that Phelps (1967) was also cited. Then, he said that the fact that the approach made the existence of a tradeoff dependent on expectational error led to the consideration of approaches to expectations-formation which would mean there was no tradeoff, and hence to rational expectations, (p. 193), citing Lucas (1972b), Lucas (1973) and Sargent and Wallace (1975).

Modigliani (1977) described a Phillips curve model with expectational adjustment, attributing it to Friedman (1968) and moved immediately to say that 'the death blow to the already battered Keynesian position was to come only shortly thereafter by incorporating into Friedman's model the so-called rational expectations hypothesis'. (p. 31) Similarly, Modigliani and Papademos (1978) considered 'alternative views' of the inflation-unemployment tradeoff, describing 'An important theoretical development' (p. 739) due to Friedman and Phelps before saying that more recently even the existence of an exploitable short run tradeoff had been called into question by the rational expectations school, citing Lucas (1972b) and Lucas (1976) as well as Barro and Fischer and Sargent and Wallace. Gramlich (1979) describing the theory of the Phillips curve

said, 'There are two standard models of changing inflationary expectations'. (p. 135) The first was attributed to Friedman and Phelps, but then described in terms of 'a typical Friedman story'. (p. 136) The second was that of Lucas (1972b) and Sargent and Wallace (1975). And then Fischer (1979) even adopted the expression 'Friedman-Phelps-Lucas-type Phillips curve', citing Friedman (1968) and Lucas (1973).

This picture of the rational expectations revolution taking its lead from Friedman (1968) clearly caught on, with Tobin (1981) again playing a role. His idea of 'Mark I' and 'Mark II' monetarism highlighted the point by specifically making the first version was Friedman's, and the second, the rational expectations version, a development of it, rather than the wholly new departure as which it might be seen.

Similarly, the earliest books describing the development of rational expectations put Friedman clearly in a central role. Begg (1982) described the rational expectations macroeconomics of the 1970s, starting with Lucas and Sargent and said it formalised the idea of a monetary rule from Friedman (1959) and that it provided, 'a theoretical underpinning for Friedman's (1968) Presidential Address to the American Economic Association in which he disputed the power of monetary policy to affect real variables'. (p. 133) Even more striking was the way it was put by Sheffrin (1983, p. 27). He said Lucas', 'several seminal papers in the early 1970s took Muth's concept and pushed it to the forefront of current economic thought' and,

'The background for these papers was a the growing professional interest in the formation and effects of inflationary expectations, a topic that was brought forcefully to the attention of the profession by Phelps (1970) and Friedman (1968). Any discussion of the rational expectations hypothesis in macroeconomics must therefore begin with the Phelps-Friedman 'natural rate' revolution.'

Tobin, Begg, and Sheffrin were writing well after Friedman's paper had started to be seen as important and so cannot themselves be the explanation of that development. They do though give the impression of reporting views which would not be doubted, and treat the matter as if the development of thinking was very natural, even if, in Tobin's' view, misguided. The explanation of the acceptance of those views is surely to be found in some combination of a generally exaggerated esteem in which Friedman's paper was held, and in the remarks of the authors of the rational expectations revolution themselves – Lucas (1972b) and Sargent (1973).

## **VIII. Conclusion**

The reputé of Friedman's Presidential Address takes some explaining. It is a poor, hurriedly written paper, and the point over which it is so highly regarded – that expectations adjust to reality – was barely part of the subject matter of the paper at all, is a rather obvious one, and one which was demonstrably part of the common understanding of economists at the time the *Address* was given. And the problem is brought into focus by the fact that it is only five years after its

publication that it started to be seen as important. It is this *change* of perception that we have sought to explain.

Part of the background may well be the rapid acceptance of the idea of the 'natural rate of unemployment', or the of the vertical Phillips curve, itself. While also noting the importance of Sargent (1971), Goutsmedt and Rubin (2018) trace this development particularly through the influential work of Gordon. His importance arises from the volume and quality of work he did on the topic in the 1970s, but also from the fact that his own view changed. Initially denying that the Phillips curve was vertical, by 1977, he had concluded that it was, and furthermore wrote a textbook very much emphasizing the point. Even though, as Goutsmedt and Rubin also argue, his acceptance of the idea was not the simple and unqualified matter sometimes suggested, in changing his position, he may well have led opinion. In itself, though, that was opinion about the economics of the question, not the novelty of Friedman's argument.

Similarly, whilst it might be said, perhaps, that it is not uncommon for revolutionary work to be recognized as such only slowly, that is not the point being made. Friedman's paper was *not* revolutionary – far from it. And it is not that it was initially unnoticed, but that it was seen perfectly clearly as restating a point that had been stated many times before. Nor is its eventual elevation to be attributed to any supposed special talent Friedman had for self-publicity, because just what it notable is that for seven years after its publication – until Friedman (1975) – he showed no sign at all of thinking it worth publicizing.

Some part of the explanation may lie simply in what was at the time his unusual choice of terminology in putting an old argument in terms of 'the Phillips curve'. But we have also pointed to three other considerations. One is that the disappearance of the lubrication argument from everyday discussion – a disappearance easily understood in the light of its irrelevance to the situation that developed – changed the argument Friedman seemed to have been making. In the context of wide acceptance of lubricating effects of inflation, an insistence that there could be no such effects would be rather an extreme position, and certainly an unusual one. But when those effects ceased to be part of daily discussion, the fact that Friedman was understood as having been unusual in insisting on a vertical Phillips curve suggested that others had been very seriously in error, and Friedman to have been prescient and uniquely insightful. Then Tobin chose to present Friedman (1968) as a crucial paper, and the crucial debate as being about the Phillips curve, and others followed his lead. Again, when inflation rose, any sense in which there might have been an exploitable 'Phillips curve' arising from Tobin's lubrication argument disappeared from view and Tobin appeared to have lost the argument – again creating the impression of the importance of Friedman's paper. The circumstances in which the lubrication argument disappeared – the circumstances of higher inflation – developed very quickly in the 1970s, but it so happens that in 1972 and 1973 the rational expectations theorists also put Friedman, and specifically Friedman (1968) at the centre of macroeconomic thought.

The combination of the influence of Tobin (1972), Lucas (1972b), and Sargent (1973) in framing impressions of Friedman's paper seems very clear. Both stories are thoroughly misleading since Friedman (1968) had no true role in the development of either line of thinking. But it is easy to see how a similar impression of its importance, coming from two quite distinct sources, combined with the confusions of terminology around 'the Phillips curve', and of thought around the lubrication argument could create a powerful impression of the importance of Friedman's paper.

The speed with which this erroneous view emerged, with no objection raised, may still seem surprising, but what is visible in the literature is only what authors chose to write. We cannot see how many there were in the second half of the 1970s and later who observed other authors incorrectly attributing originality to Friedman and Phelps. They might have written comments to correct the claims, but it would probably have seemed pedantic if they had. It is only over the much longer term that it becomes apparent that perceptions of the historical shape of developments have been distorted.

Nevertheless, the widespread presentation of Friedman and Phelps as originating the idea of expectational adjustment was rapid. That, though, fits this account neatly into the wider story of the development of mistaken ideas about the Phillips curve generally. In Forder (2014a, pp. 158-61) evidence was presented that the wider Phillips curve myth emerged in 1975 and 1976. That wider myth included inter alia, the originality of Friedman and Phelps and the idea that policy had in fact been set on the basis of a stable Phillips curve. The authors we considered who were the first to attribute originality to Friedman and Phelps – in 1973 and 1974 – did not also say that policy had been set on the wrong course by a misunderstanding of the curve. With the closer study of the responses to Friedman's paper, we can therefore see the myth certainly taking shape very rapidly, but also piece by piece.

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