

Why is labour market adjustment so slow in Friedman's Presidential Address?*

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Abstract

It is noted that Friedman (1968) suggested the adjustment to a change in the rate of inflation would take decades and that this is rather a long time. Various suggestions as to why Friedman may have said this are considered. It is argued that he may have had in mind not a more or less rational change in expectations, but something more like a change in the habits of thought. It is noted that if this is correct, then despite the influence his paper is routinely said to have had, his view is not generally accepted.

I. Introduction

For all the fuss that is made over the supposedly revolutionary idea that inflationary policy would lose its expansionary power when expectations – or ‘anticipations’ as Friedman (1968) actually called them – adjust to reality, remarkably little is ever said about how long he thought that process would take. But having discussed the question of the adjustment of nominal interest rates to ongoing inflation, and observing that Irving Fisher thought that process took decades, and having accepted that a monetary stimulus could bring a ‘temporary’ fall in unemployment, he said,

‘But how long, you will say, is “temporary”? For interest rates, we have some systematic evidence on how long each of the several effects takes to work itself out. For unemployment, we do not. I can at most venture a personal judgment, based on some examination of the historical evidence, that the initial effects of a higher and unanticipated rate of inflation last for something like two to five years; that this initial effect then begins to be reversed; and that a full adjustment to the new rate of inflation takes about as long for employment as for interest rates, say, a couple of decades.’ (p. 11)

He added to that this estimate related to inflation at the kinds of rates that had been experienced in the United States and that adjustment would be faster for ‘much more sizeable changes, such as those experienced in South American countries’. (p. 11) But he gave no further explanation of what seems a very slow process.

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II. Did he really say it takes decades?

One argument, advanced by Nelson (2018), is that Friedman did not say adjustment would take twenty years – and the claim that he did is a ‘fallacy’. Arguing this, Nelson said that Friedman meant that unemployment would differ from equilibrium only during the two to five years and that ‘full adjustment’, ‘includes the lingering dynamics beyond the main dynamics associated with the initial two-to-five year period’. In support of this, Nelson cited a ‘1972 piece’ by Friedman – Friedman (1972) – as a clarification, also saying that it was little noted in the research literature, and said that in Friedman (1978) he had ‘reaffirmed’ his judgment from 1972.

There are several difficulties with this view. One is that the contribution in question was a letter to *The Times*, and the so-called reaffirmation in 1978 was a *Newsweek* discussion of policy and outcomes in Japan in the 1970s in which no general conclusions about lengths of lags were suggested or drawn. These may or may not reflect Friedman’s best thought-out and deepest-held views, but one must consider the possibility that they were quick, or argumentative, or expedient responses to the rhetorical requirements of the circumstances in which he found himself. The letter to *The Times* was a response to a rather sharp criticism, and the possibility that he was reinterpreting his own statement to escape the criticism cannot be ignored. The 1978 discussion is simply not long enough to be treated as decisive, and in any case, Friedman gave no indication at all that he had in mind his own views from a decade earlier. Rather, he gave every impression of merely meaning to comment on then-current circumstances in Japan.

In any case, whatever one makes of these later sources, the argument of Friedman (1968) itself is clear enough on this point. Nelson’s claim was that when Friedman described the ‘initial effects’ of a monetary expansion as lasting ‘two to five years’ this could mean either that at the end of that period, unemployment had more or less returned to equilibrium or that it had reached its trough by that time. But Friedman said that the initial effects of higher inflation last two to five years and this ‘initial effect then begins to be reversed’. (p. 11) It is the beginning, not the completion of the reversal which starts after two to five years. That is contrary to the conclusion Nelson drew, and shows that Friedman (1972) does not clarify what was previously said, but takes a different position.

Another point with the same effect is that before the remarks quoted above, Friedman said,

‘To state this conclusion differently, there is always a temporary trade-off between inflation and unemployment; there is no permanent trade-off. The temporary trade-off comes not from inflation per se, but from unanticipated inflation...’ (p. 11)

and then in the next paragraph,

‘But how long, you will say, is “temporary”?’ (p. 11)

followed by the words quoted above. Clearly the thing that is ‘temporary’ is the trade-off. Temporary is then said by Friedman to mean decades. It is therefore the tradeoff that lasts, in some degree, that long.

And finally there is a little clue in the point that Friedman said that the period of adjustment for unemployment was similar to what Fisher thought the period for interest rates – and that was decades. It might be questioned how much weight should be put on that, or how carefully Friedman had read Fisher, but the implication for what he thought about the adjustment period of employment is apparent.

It is clear then, that however Friedman might later have changed his mind, the message of the *Presidential Address* was that the unemployment-reducing effect of inflation might last for a couple of decades. And it is clear, then, that there is something more to be explained.

III. A mistake?

One possibility that needs to be considered is that Friedman’s claim was simply a mistake. One point is that the suggested adjustment period seems unreasonably long, and another that the suggestion of it is therefore uncharacteristic of Friedman’s judgments on empirical matters. But the possibility that it is an error takes strength from the generally poor quality of the *Presidential Address*. Following up on Forder (2016) and Forder (2017), in Forder (2018a) I noted that amid an assortment of spelling, grammatical, minor factual mistakes and infelicitous expressions, the *Address* contains five or six pieces of argumentation or reasoning which seem deeply flawed. None of them, it must be said, is of fundamental significance – in all cases it is either easy to see how they could be corrected, or they are in any case incidental to the main line of argument and in that sense harmless. They do, though, show the paper to be carelessly written, and so make it easy to think that a clumsy statement about time lags might just be part of the pattern.

An indication otherwise, though, comes from the comparison of the general line of argument in Friedman (1968) with his other work in Forder (2018b). Then, it becomes clear that at the time it was written it was very much a rehash of arguments he had previously made; and that he later carried on making the same points. The mistakes identified in Forder (2018a), though, are overwhelmingly peculiar to the 1968 presentation – that, surely, is a sign of its being a quickly written account of his views. On the other hand, the idea of very slow adjustment, though not put in quite the same terms appears elsewhere in Friedman’s work. Unlike the mistakes in the paper, it seems to describe something much more like a consciously-held and fairly consistent view.

One point would be that in a general way, the recurring idea in Friedman’s work, including notably Friedman and Schwartz (1963), that any relationship between the quantity of money and other variables is reliable only over long periods is

consistent with the long lags described in the *Presidential Address*. But he also made much more specific points than that, such as in Friedman (1963/1968, p. 34) when he described the idea that inflation might raise prices more than wages and hence redistribute income to capitalists, thereby speeding development, and introduced Hamilton's work on the inflationary impact in Europe of Spanish-American silver. He said, 'Hamilton rather conclusively demonstrates that the resulting rise in prices had its origin in the specie inflow and strongly stimulated economic development presumably by redistributing income and wealth in the way suggested above.' Friedman gave no specific citation, but Hamilton's work concerned very long periods of time – in Hamilton (1929, p. 355-6), for example, he said that for a period of nearly 200 years wages lagged behind prices, to the benefit of capitalists, and suggested their profits might have been quadrupled by the inflation. So here too Friedman would seem to be accepted a very long adjustment period.

Similarly, after the *Presidential Address*, in Friedman (1970, p. 23-4) he said, 'In the short run, which may be as much as five or ten years, monetary changes affect primarily output. Over decades, on the other hand, the rate of monetary growth affects primarily prices'. That is obviously not quite the position he took earlier, but even ten years would seem to be a long time for expectational adjustment to a consistent inflation. And later, when Modigliani (1977, p. 31) said that Friedman had argued that a monetary expansion could only have a temporary effect on unemployment because 'expectations must soon catch up with the facts', he responded (in Friedman and Modigliani (1977)) by saying that Modigliani was attacking a straw man and quoted from Friedman (1968, p. 11), including the conclusion that the process might take two decades – in other words, it did not happen 'soon'. There, perhaps, the rhetorical imperatives were different from those at the time of his 1972 letter to *The Times*, and the comment should not be given too much weight. But here the point being made is not that these remarks clarify his view. It is merely that they show he did not think his view had been mistaken when he expressed it.

There was also a discussion in Friedman (1977) which is about a slightly different matter, but which may be related. There, he considered the point that apparently contrary to both the idea of an inflation-unemployment tradeoff and that of the natural rate hypothesis, higher rates of inflation seemed to be associated with higher unemployment. This, he suggested was because higher inflation was associated with greater volatility of inflation and hence with more disruption of the market and the fact that the 'public has not adapted its attitudes or its institutions to a new monetary environment'. But he went on to speculate that given long enough – a time 'to be measured by quinquennia or decades not years' (p. 470) – that adaptation would occur, and implied that high rates of inflation would then have no effect on unemployment. All these sources, then, tend to suggest Friedman thought these kinds of adjustment were slow to occur.

IV. A Fisherian analysis?

In seeking to explain this view, the facts that Friedman (1968) invoked Irving Fisher in relation to the adjustment of interest rates, (p. 6) and that he appeared to draw a parallel between that and the adjustment of wages may suggest there is insight to be found in Fisher's reasons for thinking interest rate adjustment was so slow.

In his discussion of interest rates, Friedman raised the possibility of a central bank trying to keep interest rates down by open market operations, thereby increasing the quantity of money, and said there would initially be three effects. There would be an increase in spending and hence in income and this would increase the demand for money and the demand for loans, and possibly that it would raise prices, thereby reducing the real money supply. Of these, he said they would tend, after a period, to return interest rates to their original level. He then considered a fourth effect, saying 'when and if it becomes operative' it would go further than the others and lead to nominal interest rates being higher than they otherwise would have been. He said,

'Let the higher rate of monetary growth produce rising prices, and let the public come to expect that prices will continue to rise. Borrowers will then be willing to pay and lenders will then demand higher interest rates – as Irving Fisher pointed out decades ago. This price expectation effect is slow to develop and also slow to disappear. Fisher estimated that it took several decades for a full adjustment and more recent work is consistent with his estimates.' (p. 6)

Here, it is just the 'price expectation effect' which lasts over decades, whereas in the quotation from page 11 it was 'the several effects', but that is surely no more than an example of the poor drafting of the paper discussed in Forder (2018a). More interesting is that Friedman clearly contemplated that adjustment might occur *without* expectations changing – the increase in the demand for loans, etc., might be enough in themselves; changing expectations are to be considered only 'when and if' they come into play. Friedman might have taken a similar approach to explaining the effect of surprise inflation on the labour market, but he did not. He could have said that inflationary stimulus raises the demand for labour more than it raises the supply, and hence there is a tendency for the wage to rise; and he might have argued that this, and any other points along the same lines, could be sufficient to bring complete adjustment themselves. Instead, he moved directly from 'initial' effects to the consequences of changing expectations. That, of course, might be just another little mistake; or it might be intended that the two processes are quite different – but there is nothing there to explain why that should be so.

In any case whilst the discussion of interest rates might be said to fill-in the explanation of why their adjustment takes a long time, what it does not do is explain why the expectations themselves are so slow to respond to developments. Indeed, by saying 'when and if', Friedman seems to emphasize that this aspect of the process is slow. Even here, he gave no explanation of why that should be.

And then there is the point that if Friedman intended, as it may appear, that his story about the adjustment of interest rates should be a reflection of the argument of Fisher, it seems to be, as Rutledge (1974, p. 21) noted, rather a poor reading. His failure to give any citation makes it a little difficult to be sure what argument he had in mind, but the most likely possibility seems to be Fisher (1930).

In chapter XIX, Fisher first gave a clear account of the relevance of perfectly foreseen price changes and argued as a matter of theory that they should be expected to be fully incorporated in nominal interest rate changes. Seeking to elaborate on the importance of expectations, he further argued that in certain historical instances, differences in interest rates between different securities traded at the same time could be accounted for by differences in expectations. For example the on-going depreciation of Rupee Bonds against Gold Bonds of the same issuer was roughly matched by interest differential.

He then considered the relation of changes in commodity prices and money interest rates and showed that whilst changes in the interest rate were in the expected direction, they were not as large as would be expected if there were perfect foresight so that when inflation changed, the change in the real interest rate was in the opposite direction to that of the nominal rate and that adjustment was much less than complete, with real interest rates sometimes being substantially negative. He continued, 'Another symptom of the same imperfection of adjustment is the fact that the adjustment is very *slow*'. (p. 415-416, emphasis in original) Over the following pages he then argued that the statistics showed the effect of price change on interest rates was distributed over decades-long periods. That is what Friedman said, but the interesting point is Fisher's explanation. First he said,

'A further probable explanation of the surprising length of time by which the rate of interest lags behind price change is that between price changes and interest rates a third factor intervenes. This is business, as exemplified or measured by the volume of trade. It is influenced by price changes and influences in turn the rate of interest.' (p. 429)

In due course, this led him to say,

'Two facts have, I think, now been well established. The first, that price changes influence the volume of trade, has been shown in earlier studies made by me. The second, that the volume of trade influences the rate of interest, has been shown by ...[listing several authors]

The evidence for both relationships is not only empirical but rational. Rising prices increase profits both actual and prospective, and so the profit taker expands his business. His expanding or rising income stream requires financing and increases the demand for loans' (p. 439)

This is significant because it shows that the speed of approach to equilibrium is not determined by specifically the adaptation of perceptions to reality, but by the

speed with which other factors change. That does not seem to cohere at all well with Friedman's account.

There might be a question as to whether it was this, or some other work of Fisher that Friedman had in mind. But nothing much is changed by considering other likely sources. The discussion of the adjustment of the nominal interest rate to inflation shocks in Fisher (1896) is briefer and less sophisticated, but even there, foresight of price changes is given a role and it is noted that adjustment is incomplete so that foresight is evidently imperfect, and the further explanation turns on the consequences of imperfect foresight in generating circumstances where the demand for loans changes. That change in demand is then a second factor affecting nominal interest rates. So when Fisher there found adjustment closer to complete over longer than shorter periods (p. 71) it was not, or not simply, because expectations were catching up with reality. The discussion in Fisher (1907, ch V) makes the points about anticipated and unanticipated inflation, but does not consider how expectations adjust through time. Then there is a discussion in Fisher (1911/1922, ch IV). That mainly concerns the transitional behaviour of an economy when the quantity of money changes. Fisher offered an analysis of the development of business cycles in which the incomplete adjustment of the interest rate to inflation was an essential. There, he argued that an initial disturbance would introduce oscillations, and that in the absence of further disturbances they would eventually settle down, but the story of the adjustment is not about the gradual movement specifically of expectations into conformity with reality – it is about the movements of prices, the interest rate, and the currency-deposit ratio.

So, Fisher's idea was that expectations adjust quickly – immediately if one reads some of his work literally – but they adjust only partially. It is other factors, working through the economic system which bring a fuller adjustment. It is probably not a fair reading to treat his analysis too literally – certainly that of chapter IV of Fisher (1911/1922) is too specific as to sequencing and strength of effects to be treated seriously as more than a sketch of developments. But that is not really the point since there is no sign that the crucial process bringing equilibrium is anything very much to do with the gradual approach of expectations to reality.

Perhaps, then, Friedman simply did not understand Fisher. But if he did, it seems that, despite appearing to link the two arguments, Friedman did not intend to present the same kind of story for the wages as Fisher did for the interest rate. He cannot have intended that kind of story to explain why it takes decades for inflation expectations to adjust.

V. An adjustment of habits of thought?

One of the great misperceptions about Friedman (1968) is that it was an innovative paper, in particular in presenting the expectations argument. Its supposed path-breaking originality is so often asserted or taken for granted that it seems an extraordinary thing to deny. Yet it is a simple matter to find earlier statements of the idea – and earlier statements by prominent economists,

writing in widely-read publications. Plenty are considered in Forder (2010); and in Forder (2018b) I pointed to several earlier statements of it by Friedman himself. In these and Forder (2014, ch 4 part 1) I also noted various other indications that the argument was no surprise in 1968 – such as the fact that the argument did not receive the comment it would have done, had it contained any surprises, the fact that the paper itself gives no emphasis to the argument, and that when Friedman reprinted it in Friedman (1969), it appeared as chapter five of thirteen, with no suggestion it was an important paper. And in any case, as Rivot (2017) said the general importance of expectations was widely appreciated in Keynesian economics.

The question of how Friedman's paper came to be seen as so revolutionary was considered by Forder and Sømme (2018) and one point there was that Tobin (1972) made it seem important by appearing to present a new argument in response to the idea of the vertical Phillips curve. That was an argument to the effect that if there is downward nominal wage rigidity, and there are various labour markets which are partially segregated, for example, by region or industry, and subject to differential shocks, then adjustment to these shocks can be facilitated by inflation. Contracting sectors can achieve real wage reductions without bearing the costs of overcoming nominal rigidity, so that inflation 'lubricates' adjustment, as it was sometimes put. This was in fact an old argument, having been very fully developed by Schultze (1959), and as I showed in Forder (2014, p. 79 and p. 234-5 n. 20) it was very well known in the 1960s. But as Dimand (2018) noted, Tobin's paper was very much presented as a response to Friedman (1968) and as I argued in Forder (2014, p. 183), he may well have conveyed the impression that his own argument was much more original than in fact it was.

A possibility that is obscured by the appearance that Friedman's paper was revolutionary, and Tobin's a response to its central argument is that it was Friedman who meant his remarks as a response to the lubrication argument. One sign that this may be the case is that in Friedman (1958) he had previously addressed that argument, arguing that in the long term, behaviour would adjust to the ongoing inflation because, for example, such inflation would promote wage indexation. That, obviously, would mean that downward nominal rigidity would be transformed into downward real rigidity, and in that case, the lubricating effects of inflation would disappear. That argument was put in terms of changing institutions and behaviour, rather than 'expectations' or anything of that kind, but that is quite sufficient to make the case for what would later be described as a 'vertical Phillips curve'. In this case, though it is quite clear that the argument was being made in response to the lubrication argument, rather than in response to the naïve idea that simply inflating demand could produce a sustainably low level of unemployment.

Furthermore, in that 1958 paper, Friedman drew a distinction between inflations with different origins. Having discussed the adjustment of behaviour to on-going inflation, he said,

‘From this point of view, there may clearly be a major difference between the effects of a superficially similar price rise, according as it is an undesigned and largely unforeseen effect of such impersonal events as the discovery of gold, or a designed result of deliberative policy action by a public body.’ (p. 252)

He did not there expand on that, but there is a clear resonance with what he said a few years later in Friedman (1963/1968). There, elaborating on his remarks about Hamilton’s findings, he said that the influx of precious metal to Europe raised profit and lowered wages,

‘because it was unexpected. As a result, people clung to traditional prices and traditional levels of interest rates with the result that there was a transfer of income from wage earners to profit recipients... One reason the inflation process was unanticipated is that nobody planned it that way. It was, as it were, an act of God. Somebody found gold and silver; it flowed into Europe and raised prices.’ (p. 35)

This, it should be noted, came from before the time when economists habitually use the word ‘traditional’ to mean simply ‘in the past’, and so it had a much stronger sense of being related to tradition than it might seem on later reading. He immediately contrasted this with another possibility, saying,

‘I am exceedingly sceptical that any similar result can be obtained by a deliberate process of expanding the money supply without its degenerating into hyperinflation. If it is done deliberately, many people will know about it and will act so as to prevent the redistribution from taking place. If you announce to the public that you are going to adopt the deliberate policy of increasing prices at the rate of 3 per cent a year everybody will adjust to that announcement.’

How much serious criticism this contrast would withstand is one question – Friedman apparently thought that the Spanish inflation lasted 150 or more years without becoming expected – but the interesting issue concerns the contrast he is drawing. The Spanish inflation was ‘unexpected’ because unplanned; and that is contrasted with a ‘deliberate’ policy which, it seems to be suggested, leads to a quick – or perhaps immediate – adjustment of behaviour. One is forced to the view that the idea – a very reasonable idea, of course – was that the speed with which expectations adjust depends on much more than the observed economic outcomes. It also depends on the wider context of what is understood by the public.

There are also some small clues in Friedman (1968) itself. One is simply the point that in discussing the Phillips curve, Friedman said that what he had to say was ‘against the grain of current thinking’. (p. 7) Once it is recognized that the expectations argument was widely known, that appears as a strange remark. Again, it could be sloppy drafting, but the lubrication argument was widely accepted, so to question that would be going against the grain of thinking.

Later, having argued that monetary policy could not effectively target a real variable, he turned his attention to what it can do, and said,

‘If the end of a substantial war offers the country an opportunity to shift resources from wartime to peacetime production, monetary policy can ease the transition by a higher rate of monetary growth than would otherwise be desirable’ (p. 14)

He doubted that policy would actually be effectively conducted, but otherwise did not expand on the point. It appears, though, that he had some version of the lubrication argument in mind since what was required was for some sectors, such as armaments manufacturing, to shrink – suggesting that real wages should fall there – while others expanded. In that case, in this passage, Friedman was accepting the possibility of a temporary benefit arising from inflation in those special circumstances, and his recognition of the importance of the argument in policy discussion is then clear, and his questioning of whether it could have a long-enduring effect would fit neatly into the picture.

Then there is Friedman’s definition of the ‘natural rate of unemployment’ as being the level that would be ‘ground out by the Walrasian system of general equilibrium equations’. (p. 8) That was overtly a metaphorical piece of argumentation, rather than a momentary conversion to Walrasianism, but the metaphor was perhaps one of a very full, very long run, adjustment. The grinding out suggests not merely the development by the public of an understanding of policy, but also the adaptation of habits, or even traditions. It is, as it were, the equilibrium prevailing only in the longest of long runs.

Thirdly, there is his use of the word ‘anticipations’. That seems usually to be regarded simply as a synonym for ‘expectations’, and Friedman’s choice of word as being of no significance. There may, however, be a better view. In Friedman (1957, p. 15) he distinguished the two, saying that an axis of certain diagram might be interpreted as measuring,

‘expected consumption in year 2, where ‘expected’ is used in the sense of ‘mean value’ rather than of ‘anticipated.’

There, it seems Friedman may have been drawing a distinction between the forecast that would be derived from rational analysis, and something else. In the context, it seems most likely that the other thing would be something like the psychologically realised impression about the future, and hence the actual, empirical driver of behaviour. Thinking along those lines, the choice of word in Friedman (1968) may not have been merely a personal foible, but might have been intended specifically to identify the forces determining behaviour, as distinct from highlighting the outcomes of rational analysis – and in those forces, customary behaviour, and such things as traditional prices might be ones that matter.

Putting these things together, and attributing to Friedman a broadly consistent view, the impression is that what he had in mind was the slow effect of

adaptation to the facts of a gently inflationary environment. The inflation would be unannounced – not actually an act of God, but nor an advertised policy. And the ‘anticipations’ that were to adjust were not ‘expectations’ as that word was later used in economics, but the forces determining a pattern of behaviour. ‘Anticipations’ describe the state of mind that arises when experience makes inflation the normal state of affairs; the state of affairs to which behaviour is naturally, and automatically, rather than consciously, adapted. It is more like a change of habit than a change of understanding.

A family of such responses might well be considered, so that for example the habits of thought of early modern Europe, perhaps influenced by the idea of a just price might take longer to change than the ‘anticipations’ of postwar workers and their unions, while the changes under discussion in the Nobel lecture that are required to prevent permanently high and volatile inflation reducing unemployment would be in another category again.

That is, to be sure, a long way from a post-1980 understanding of ‘expectations’ in economics. But that just might reflect the fact that Friedman was not invoking an idea like that. And certainly, the sense of the gradual alteration of habitual understanding offers a much better explanation of the idea that the process might take decades than does anything else on offer.

VI. An early response to Friedman

Later understandings of Friedman’s *Address* clearly preserved no aspect of this understanding, but it can perhaps be seen in earlier ones. Solow (1969), following up and expanding on Solow (1968), is the fullest of the earliest works which are clearly motivated by the goal of responding to Friedman. Solow rejected the vertical Phillips curve for low rates of inflation, but found that in some econometric formulations it was confirmed at high rates. Discussing his results, he said,

I can believe that a 10% annual rate of inflation, maintained steadily, will eventually become built into expectations just as the hypothesis describes. But it is not clear that this requires me to believe that a sequence of mostly small, irregularly varying, rates of inflation is fed into the economic system’s memory in the same way to produce an expected rate of inflation. Solow (1969, p. 15)

The natural response of an economist of the 21st century is to think that however small and irregularly varying inflation is, it has a mean rate, and that can be incorporated into expectations. But an economist of the 1960s could perfectly well have thought that. But if the point is under discussion is less about rational wage-setting, and more about not-quite-rational psychological predispositions, Solow’s concern seems sharply focussed – small, irregularly varying rates of inflation do not affect those predispositions in the way that a higher, steady rate, eventually would. They do not affect habits of mind and traditional pricing in the way that steady continuous inflation surely would. Here, then, is an indication that in the immediate commentary on Friedman (1968), his point may have been

seen as being about something like the lubrication argument, rather than as being about the effects of a steady policy of creating excess demand to 'exploit the Phillips curve'.

VII. Conclusion

Friedman (1968) is not carefully enough written for it to be reasonable to express much certainty about what the details of the presentation were intended to mean. But there is clearly some suggestion that his discussion of changing anticipations was intended as a response to the lubrication argument, not as a general forecast of the futility of a policy of continuous excess demand. It means Friedman was presenting an argument on a point that was reasonably in question, rather than pushing an idea that had never been seriously doubted. It puts Tobin (1972) in a different light too since a large part of his Presidential Address, intended as a response to Friedman's simply misses the point. It was Friedman's argument that was answering Tobin's.

On the other hand, two frequently-advanced claims about the importance of Friedman (1968) are very much called into question. One concerns the question of the character, rather than merely the substance of Friedman's argument, and in particular the idea that his argument was in some important way a precursor of that of Lucas (1972). Along the lines of the ideas of 'Monetarism Mark I', and 'Monetarism Mark II' of Tobin (1981), Friedman is sometimes seen as inventing the thought that expectations matter to rational behaviour, but modelling them in a way that was not fully consistent with such behaviour. But taking the separate idea of 'anticipations' seriously, Friedman was doing nothing like that, but was pointing to the operation of slow-moving, habit- or tradition- based patterns of behaviour which had nothing strictly to do with 'expectations', rational or otherwise, at all. He neither advocated nor was thinking of anything so formulaic as 'adaptive expectations', but of a much looser process of acclimatization. While certainly consistent with the distinction drawn by Hoover (1984) or de Vroey (2016) between Friedman's 'Marshallian', and Lucas' 'Walrasian' approaches, this idea goes rather further than they did in that general direction. Friedman's economics is another world from Lucas', and he might be seen very much as being in the kind of institutionalist line in which Hammond (2003), considering only earlier work than this, put him.

Indeed, a further point can be made about his ideas of how expectations are formed and the idea that Friedman adopted 'adaptive expectations' – meaning by that, expectations determined in some formulaic way from past values of the variable in question. In Friedman (1968) itself he contemplated faster adjustment to high rates of inflation, 'such as those experienced in South American countries' and in the discussion in Friedman (1958) and Friedman (1963/1968), he distinguished the deliberate or announced policy from uncontrolled events, just for the purpose of saying that they would affect expectations in different ways. So the idea that he adopted 'adaptive expectations', not only describes an outlook that is much too formulaic than his writings suggest, but also too specific. Friedman was much more sensible than

that, recognizing the importance of expectations, and recognizing that in different contexts they will be formed in different ways.

But there is also the question of the influence of his thinking. The presumption of a great and lasting influence comes from an erroneous idea about the history of economics. That story is simply not true, but there is a further point since if he was making an argument about the disappearance, over decades-long periods, of the lubricating effects of inflation, it is far from clear either that he was right or that most economists ever accepted his argument. Price stability was not achieved after Friedman's address, yet, nearly three decades later, Akerlof, Dickens, and Perry (1996) continued to advocate the argument I have suggested he was criticising, and two decades later still, Praet (2016) used it to justify the European Central Bank's 'fight against low inflation'! So five decades after Friedman's presentation, it is not clear that he was right; much of the world still believed in the lubricating effects of inflation; and steady, positive inflation is widely advocated for just the reasons Friedman was rejecting.

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