



Why Did Milton Friedman Win the Nobel Prize? A Consideration of His Early Work on Stabilization Policy

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It is hard to imagine that many economists would dissent from the view that the combination of *A Theory of the Consumption Function* (Friedman 1957) and *A Monetary History of the United States 1867–1960* (Friedman and Schwartz 1963)—two books of fundamental importance in the postwar development of economics—were quite sufficient to earn Milton Friedman his Nobel Prize in 1976. The Nobel citation ([link](#)), though, whilst clearly pointing to his work in those areas did not stop there. It reads in full:

for his achievements in the fields of consumption analysis, monetary history and theory, and for his demonstration of the complexity of stabilization policy.

It might seem that “demonstration of the complexity of stabilization policy” signifies Friedman’s work on the Phillips curve and changing expectations. There, his *American Economic Review* article “The Role of Monetary Policy” (1968) is often said to be crucial in virtue of introducing the idea that ongoing inflation would be incorporated in the wage bargain so that any expansionary effects of inflation would dissipate—the ‘accelerationist hypothesis’ as it is sometimes called. There

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are clear reasons, however, to reject the view that this is the work to which the citation referred.

Although the opposite has often been claimed, as of the middle or late 1960s, there was nothing new in the expectations argument.³ In any case, it was the argument for rules rather than discretion, not discussion of the Phillips curve, that was the focus of Friedman's paper.⁴ Perhaps more importantly than those points, however, the idea that a policymaker might seek to hold unemployment below equilibrium with inflationary policy is not quite stabilization policy at all—it is an attempt to maintain a disequilibrium. And if that is treated as being a variety of stabilization policy, Friedman's argument was not that it was complex, but that it was impossible. Furthermore, the Nobel press release accompanying the announcement ([link](#)) discussed the three contributions listed in the citation, and then the question of expectations and the Phillips curve, followed by that of flexible exchange rates. These last two were therefore distinct from the ideas in the citation.

And finally, there is a much better candidate to be Friedman's "demonstration of the complexity of stabilization policy," since there is a line of thinking—or collection of lines of thinking—in Friedman's work that points precisely at such complexity. It is one of the bases of his advocacy of rules rather than discretion in policymaking, and in so far as it is summed up in the expression "long and variable lags" (see, e.g., Friedman 1948, 254), it might be said to be a well-recognized line of thinking. But that expression, in the context of Friedman's work, most commonly just labels an empirical observation about lags in the effect of monetary changes.

In his work considered more broadly, there is rather more to the difficulty of stabilization policy than that, and also to the ways in which Friedman's thinking on the matter developed. The single most notable contribution was "The Effects of a Full-Employment Policy on Economic Stability: A Formal Analysis," one of his *Essays in Positive Economics* (Friedman 1953/1951), but an earlier contribution of some interest is in a book review in the *Review of Economics and Statistics* (Friedman 1944), and there are later indications that he continued to think along the same lines, including most notably in *A Program for Monetary Stability* (Friedman 1960) but also in some of the less-noted passages of the 1968 paper.

3. The contrary claim, that Friedman (1968), or perhaps Friedman (1966) or else one or other of the works of Phelps, such as Phelps (1967), was revolutionary in making this point became well established in textbooks and other sources during the 1970s. In fact, there are numerous, prominent statements of the same point before these authors. A number were presented in Forder (2010), and more in Forder (2014, ch 4, part 1). Phelps (1968, 682) actually said that argument was not original to him. The award of the Nobel Prize to Phelps in 2006 is often said to have been for the same thing. If it was, that was a mistake. The citation in fact says "for his analysis of intertemporal tradeoffs in macroeconomic policy." The specifics of that analysis was original.

4. This point is established in Forder (2018).

The review of Oscar Altman's *Saving, Investment, and National Income*

Friedman (1944) first addressed the complexity of stabilization policy in a little-noticed review of Altman (1941). Altman's book was a monograph on aggregate demand. It presented a simple account of the Keynesian theory, focused on the point that saving and investment must be equal; and a large collection of statistics tending to suggest that the American economy would generate excess saving. The implication was therefore that full employment would not be maintained without public action to achieve sufficient investment.

Friedman's review said Altman's book was "less significant in its own right than as an expression of the Keynesian saving-investment theory" (1944, 101). Indeed, Friedman had rather little to say about the specifics of the book, but seems to have regarded the review as an opportunity to record some objections to the Keynesian theory. Noting that Altman's presentation of the model was simplified, he briefly described a fuller version in which the equilibrium level of income equalized saving and investment, and said that changes in investment were treated as the major determinant of income, with saving simply being a function of income. He drew attention to theoretical limitations of this view, and questioned whether available data could be used to confirm it, but also clearly noted that the focus on the equilibrium of the system presupposed that it was 'quickly and smoothly attained' (ibid., 102). He had it in mind, presumably, that a dynamic, rather than equilibrium, analysis of the pattern and rhythm of the business cycle, like that of Wesley Clair Mitchell (1913) and the later National Bureau studies including, eventually, Friedman and Anna Schwartz's *Monetary History*, could be a preferable alternative.

On the question of the quick and smooth attainment of equilibrium Friedman wrote a long footnote presenting a numerical example of adjustment in a model characterized by specific lags in behavioural responses. Friedman presented the model verbally, but it can be summarised by the four equations:

$$\begin{aligned}c_t &= 0.85y_{t-1} \\q_t &= c_{t-1} + (s^T - s_{t-1}) \\s_t &= s_{t-1} + q_t - c_t \\y_t &= q_t + I_t\end{aligned}$$

where c is consumption, y is income, q is production, s is inventory stock, I is investment, s^T is the target level of inventory, and subscripts indicate time periods.

In Friedman's example the initial position was one of equilibrium at $y_t = 100$, $c_t = 85$, and $I_t = 15$, and he considered the effect of an exogenous fall in investment to 10. The equilibrium level of output would then be $66\frac{2}{3}$ and consumption $56\frac{2}{3}$. He reported the output level generated in the first 34 periods. They varied between 10 and 221, in a seemingly random way, with a mean of 79 (and a standard deviation of nearly 70, though he did not report that). Friedman noted the similarity to the 'cobweb' model, and he could have pointed to a model by Paul Samuelson (1939) based on a second-order difference equation as compared to Friedman's third-order one. He also said, though, that simple as it was, the model was entirely in the spirit of models of the Keynesian system of the time. Indeed, Arthur Smithies (1942) offered one of that kind, and another was the widely noted econometric study of the American economy of Jan Tinbergen (1939) which, as it happens, Friedman (1940) had also reviewed.

There is perhaps some curiosity value in aspects of Friedman's presentation of the example. One little point is that there is what is presumably an error transcribing his results since the sequence he gave, and hence his calculation of the mean, was not quite correct.⁵ Another is that had he produced a longer series of values he would have found income settling into a periodic oscillation between values of 10 and 123.⁶ So although intended to illustrate a point about a slow and volatile approach to equilibrium, Friedman's model in fact never reaches equilibrium. It is hard to say whether Friedman would have thought that damaged or strengthened his criticism.

In any case, Friedman had clearly called the value of such models into question. If the model needed a lag structure of the kind Friedman said, then with the volatility of outcomes it generated, it could hardly be of much use to policymakers. Actual lags, including lags in the operation of policy itself were surely more complex than those Friedman suggested, and that meant the outcome of policy would be, in practical terms, wholly unpredictable.

The "Formal Analysis" of the effects of a full employment policy

In "The Effects of a Full-Employment Policy on Economic Stability: A

5. There should have been a 179 and a 10 after the 21st value in Friedman's sequence, with the 36 values then having an almost unchanged mean of 80; and some of his values are incorrectly rounded.

6. Strictly speaking, Friedman's statement of the model was incomplete because he failed to note the necessity of assuming that production could not be negative, but that was clearly intended. Without this assumption the model would explode rather than oscillate.

Formal Analysis,” Friedman (1953/1951) again presented basic Keynesian theory. This time he was responding particularly to John Maurice Clark, Smithies, and coauthors (1949)—a report by a group of experts for the United Nations. Like Altman’s, the book was partly expository and partly hortatory, it described Keynesian theory in simple terms, and it carried the same emphasis on the possibility or danger of investment being insufficient to absorb full employment savings.

Here, though, Friedman (1953/1951) expanded on remarks he had made earlier (Friedman 1947; 1949), and he was more specific in saying the theory suggested that fiscal policy could straightforwardly compensate for variations in investment. He said that as the model was usually presented all that was required for full employment was a contemporaneous variation of fiscal policy to offset any exogenous variation of private investment. Noting that the assumed absence of any relevant lags was an important aspect of the model, he went on to say that the appearance of the theory was that so long as policy usually operated in the opposite direction to shocks and was not more powerful than the shocks, it would be stabilizing. However, that view, he observed, was in error for the purely statistical reason that (in the additive model he considered) the variance of outcomes after policy intervention is the sum of the variance of output without policy, the variance of the effect of policy, and twice the covariance of the two.⁷ Taking the policymaker’s ability to determine the correct direction of policy as imperfect and given, Friedman then showed that attempts at stabilization policy could easily be too powerful, and as a result be destabilizing, in the sense of increasing the variance of output. This demonstrated the fallacy in the common view that so long as policy had a weaker effect than the shock itself and was usually in the right direction, it was bound to be stabilizing. Actually, it needed to be much more accurately aimed than that.

So the overall success of stabilization policy depends on both the magnitude of its effects and its correlation with shocks. The strength of policy action, Friedman said, could be reasonably controlled since, for example, a larger fiscal response would be a more powerful policy. The magnitude of effect raised a more difficult matter since that would depend on the consequential changes brought by the policy action itself. Yet more difficult, though, was the question of whether policy would push in the right direction. Friedman treated that question in terms of the “timing” of policy, saying that “if the need for action could be recognized immediately, the recognition translated immediately into action, and the action

7. His model was ‘additive’ in the sense that if output under a full employment policy is Z , output without policy is X and the effect of policy is Y , then $Z = X + Y$. In that case $Var(Z) = Var(X) + Var(Y) + 2Cov(X, Y)$. The essential point he was making would carry over to non-additive cases.

immediately effective” (1953/1951, 129), then the covariance of shock and policy could be close to its ideal value of -1 , and that this was implicitly the assumption made by those who use that model. He said that in reality, however, achieving a covariance of that kind of value would require effective forecasting of outcomes both with and without policy action, and including all the lagged effects of past policy actions.

On the premise that such effective forecasting would not happen—as Friedman clearly and with much justice believed—he suggested that making the lags as short as possible was the only way to address the problem. Here, he noted that the lags in the effects of policy would be spread over some time and might be variable. Then, reprising what he had said in “A Monetary and Fiscal Framework for Economic Stability” (Friedman 1948), he distinguished three lags: the lag in recognizing need for action, the lag in taking it; and the lag in its having its effects. Friedman (1948) had argued in favour of the abandonment of discretionary policy and its replacement by a system of what would later be called ‘automatic fiscal stabilizers,’ with variations in the government budget met entirely by variations in the quantity of money; in making this case, he had argued that lags would on the whole be shorter with automatic rather than discretionary policy. In the “Formal Analysis” he contemplated the possibility that his proposal would nevertheless be destabilizing as a result of being too powerful relative to the proportion of occasions when it pushed in the right direction (1953/1951, 130)—a point he had not made in the 1948 piece. This point he contrasted with the claim of Clark, Smithies, et al. (1949) that automatic stabilizers would not be powerful enough to balance shocks and that only additional action could stabilize demand, saying “In the light of our analysis this statement is, at best, misleading; at worst, downright wrong” (Friedman 1953/1951, 131). Misleading, it certainly was.⁸

Friedman (1953/1951) did not draw attention to the kind of calculation he had made in his review of Altman, and whereas in the 1944 piece the key lags were in the behaviour of the economy, in the later piece they were in the response of the policymaker and in the effects of policy action. In that sense, the orientation of the two is distinct. On the other hand, both turn on an appreciation of the importance of lags in what others were thinking of as an analysis in comparative statics. And in both cases, drawing attention to these lags suggests that policymaking would be much more difficult than it might seem.

In neither case did Friedman offer any pretence of being able to describe the details of the lags, or indeed of thinking it might be a practical possibility to

8. Clark, Smithies, et al. (1949) certainly had not appreciated the kind of points Friedman was making and therefore did not specifically say that they were assuming policy always moved in the right direction and without lags, but allowing them that assumption, what they said was correct.

do so. In this respect he was less ambitious, though perhaps more realistic, than A. W. H. Phillips (1954; 1957). Phillips clearly hoped to make progress on the actual estimation of lag structures with a view to refining policy, although in the end he was unable to answer the questions he had raised. For Friedman though it was not the precise specification of lag structures that was at issue, but rather the drawing of attention to the sensitivity to them of optimal policy. In 1944 the volatility in the behaviour of the model suggested policymakers could hardly know what they were attempting to do; in the later version, the dependence of the point on a specific version of the Keynesian model had been replaced by a much more general point about the dangers of attempting stabilization based on any model. Lags would arise in any system, and Friedman's way of looking at that was to think of them as impairing the timing of policy and the policymaker's ability to control the magnitude of its effect. The lags thereby both move the covariance of policy and shock away from its ideal value and made it hard to be confident that policy would not then be too powerful. This argument of statistical theory, combined with Friedman's discussion of the various lags in policy, made the case that stabilization policy is much more complex than it seems, and goes to the heart of policymaking presumptions at least as powerfully as do *A Theory of the Consumption Function* and *A Monetary History of the United States*.

The later neglect of Friedman (1953/1951)

It seems very notable that in recent times, the formal core of the Friedman (1953/1951) argument has not been nearly so noted as might be expected. A vaguer understanding of the importance he attached to long and variable lags is certainly widespread; but in the period after the award of the Nobel Prize, this key paper itself has not been nearly so noted as it might have been. Allan Meltzer (2016) raised the question of the reason for the award of the Prize itself but his reaction to the point about stabilization policy was to defer to Harry Johnson (1976). Meltzer said he had summarized Friedman's relevant work, including "his demonstration of the complexity of stabilization policy," especially his December 1967 Presidential Address to the American Economic Association," but made no mention of Friedman (1953/1951). Actually, Johnson had said that the theme ran through Friedman's work, but indeed, "The Role of Monetary Policy" (Friedman 1968) was the only piece he specifically identified in that connection. James Tobin (1976) wrote a piece that accompanied Johnson's, but made no mention at all of the complexity of stabilization policy. Perhaps most notably, Niels Thygesen (1977), specifically describing Friedman's work in the context of his award of the Prize, made nothing of it. Thygesen discussed the importance of lags in Friedman's think-

ing about monetary policy, but principally only as a point leading to various controversies (1977, 77). Friedman (1953/1951) was referred to only towards the end of the paper in a round-up of undiscussed topics emphasizing the diversity of Friedman's work (Thygesen 1977, 82); even then the paper was not actually identified but only presented in the disguise of being one of the "two essays on a stable framework" in *Essays on Positive Economics*. Edward Nelson (2018, 494) stands out as having recognized the point when he described the paper as being "alluded to" in the Nobel citation, though he said no more about the relation of the paper to Friedman's award.

But it is not merely that the reason for the Prize has been overlooked; Friedman's broader thought on this issue has been much neglected. Some might be persuaded by the point that Friedman (1953/1951) has been cited only something like one thirtieth as often as Friedman (1968).⁹ Citation counts are always treacherous, and in this case particularly so,¹⁰ but here they tell a story consistent with other evidence.

The point is much more clearly apparent from the failure of authors to refer to Friedman (1953/1951) in contexts where, had its point been appreciated, they would surely mention it. There are those who do discuss it, of course, but many notable cases of those who do not. Eamonn Butler (1985), writing at the time the only book-length biography of Friedman focusing on presenting a distillation of his economics, does not mention it. Robert Cord and J. Daniel Hammond (2016) edited a volume of 860 pages in 40 chapters on Friedman, and none of the chapters mentions the paper. One would certainly expect the paper to appear in accounts of the rules and discretion debate, yet it hardly does. David Laidler (2017) noted Friedman's (1960) advocacy of rules. Laidler (2017, 20–21 n.22) also commented in a footnote that an anonymous referee had pointed out that Friedman's case for rules arose from uncertainty about the operation of the monetary system, but despite noting the importance of lags in Friedman's thinking, he did not otherwise take the discussion in the direction of Friedman (1953/1951). John Taylor's NBER working paper "Rules Versus Discretion: Assessing the Debate Over the Conduct of Monetary Policy" (2017) mentions Friedman (1948) and Friedman (1960), but not the 1953 paper. Surveys of the rules and discretion literature include Victor Argy (1988), Stanley Fischer (1990), and William Van Lear

9. The data is from Google Scholar (Feb. 23, 2019), which shows the earlier piece cited 287 times, as against 8886 for the later one.

10. Friedman (1953/1951) was originally published in French (Friedman 1951), and its later English publication came in a book (Friedman 1953), which in itself makes it less likely than an AEA Presidential Address to attract attention. Secondly, the 1968 paper has come to be cited for its presumed influence on the discussion of the Phillips curve. As Forder and Sømme (2019) show, it ended up being cited for very much the wrong reasons, so that its true influence is called into question.

(2000). Of them, only Argy cited Friedman (1953/1951) at all, and then only for the existence of a variety of lags, not for Friedman's point of statistical theory. And an article by David Glasner (2017) that is perhaps not exactly a survey but is titled "Rules Versus Discretion in Monetary Policy Historically Contemplated" also made no mention of Friedman (1953/1951).

Perhaps a particularly interesting case is William Brainard (1967),¹¹ who adopted a theoretical disposition similar to Friedman's. Brainard's presentation had aspects of greater formalism than Friedman's, and there are differences in detail, but the essence of his argument is very similar. The idea of "Brainard conservatism"—suggesting that in the presence of uncertainty, policymakers should act less firmly than they otherwise would—became fairly well understood. That is not the way Friedman put his argument, but it is equally an implication of it, and the theoretical explanation in the two papers is very similar. Brainard himself, though, did not cite Friedman (1953/1951). Brainard's paper was well-cited, perhaps thereby diverting attention from Friedman's earlier insight.

And then evidence of a slightly different kind comes from Thomas Sargent and Neil Wallace (1976). They were considering the consequences of the assumption of rational expectations and noted that with adaptive expectations, but not with rational expectations, there would always be a feedback rule for policy that would be superior to a simple money growth rule. On the assumptions they were making, which left no room for the considerations motivating Friedman's position, that is correct. But the interesting point is that this led them to say: "Within the context of macroeconomic models as they are usually manipulated, Friedman's advocacy of a rule without feedback seems indefensible" (Sargent and Wallace 1976, 170). They would hardly have been making a point like that in relation to Friedman's position had they had the rationale of his view in mind.

Explaining the neglect of Friedman (1953/1951)

There is therefore something of a puzzle as to why Friedman (1953/1951) has in this later period been so little noted. Part of the answer may be that its importance was obscured by the creation of an impression that Friedman's crucial contribution related to the Phillips curve. As Forder (2018) shows, even in Friedman's writings that is something that occurs only in 1975—just about the

11. The relationship between Friedman's and Brainard's papers, and Brainard's non-citing of Friedman, are also discussed by Nelson (2018, 428).

same time as the Nobel award. But it is also interesting that even when discussing the complexity of stabilization policy, Friedman himself seems to have downplayed the argument of statistical theory when data accumulated to suggest that actual policy outcomes were poor.

In Friedman (1944), Friedman (1948), and Friedman (1953/1951), his discussions of the importance of lags were all essentially theoretical. But in Congressional testimony in 1958, Friedman noted that since 1907 the peak rate of growth of the quantity of money had preceded the peak of the business cycle by between thirteen and twenty-four months, and that in the case of troughs the range was between five and twenty-one months (Friedman 1969/1958, 180). His first conclusion was that the lags were too variable to expect active stabilization policy to be effective.

At that time, the further conclusion Friedman drew as regards policy was that the lag was apt to create an incorrect impression that monetary policy was generally powerless. He cited the inflation of 1956 and 1957, which had occurred while money was tight, and the lack of growth of the money supply in 1928, which was simultaneous with a period of expansion but, he said, contributed to the severity of the downturn in 1929. Furthermore, he said that this kind of outcome had the effect of misleading policymakers so that they were induced to take stronger action than appropriate. Referring to the same two incidents, and saying that 1920 offered an even clearer instance, he said that the appearance that policy was not having the desired effect led policymakers to believe that stronger measures were required, and so to take action which turned out to be excessive. On the other hand, he also said that in 1932 the failure of a mild monetary easing to bring results created the impression that monetary policy was ineffective, and that impression contributed to the Federal Reserve allowing the money supply to decline. These things, he clearly felt, led to a failure to recognize the long-term importance of monetary policy. The two conclusions together naturally led to the view that achieving a steady rate of growth of the quantity of money was the best practical option.

Friedman's views on lags brought a critical comment from John Culbertson (1960) to which Friedman (1961) responded, again addressing the question of the relationship between long lags and the quality of policy. Citing Friedman (1953/1951), Friedman said that his view was not that policy interventions were generally perverse but that "they are largely random relative to the actions that in retrospect would have been appropriate. The result is to convert actions taken for counter-cyclical purposes into additional and unnecessary random disturbances" (1961, 464).

Friedman (1960) made an argument along similar lines to that of the 1958 testimony, whilst proposing that policy be set to achieve a constant rate of growth of the money supply. He said, "I doubt that many, if any, informed students of

monetary affairs would disagree with the judgment that the actual behaviour of the money stock has clearly been decidedly worse than the behaviour that would have been produced by the simple rule” (1960, 93), and over the next few pages listed a selection of cases where the rule would have done better than actual policy. This led to the conclusion, amongst others, that in a rough and ready way, one might think that in the postwar period, policy had pushed in the appropriate direction 47 percent of the time. Citing Friedman (1953/1951), he pointed out that anything like a 50-50 result meant that the rule would have been decidedly better, saying that the variability of money supply growth resulting from the absence of a rule “is simply a disturbance that introduces instability” (1960, 96).

Friedman’s (1960) advocacy of the money supply rule involved a retraction of his proposals from Friedman (1948). There, he had proposed a range of reforms, including the implementation of 100-percent reserve banking and the cessation of the issuance of government debt other than currency. The centerpiece, though, was a plan for automatic variation of the money supply. Friedman (1948) had proposed that spending and taxation rules would be fixed so that the business cycle would automatically generate fiscal imbalance—an arrangement for ‘automatic stabilizers’ as such fiscal responses would later be called. But rather than varying the quantity of interest bearing debt, Friedman proposed that fiscal imbalance be reflected in variations in the money supply. There would therefore be an automatic response of the money supply to business fluctuations.

Friedman (1948) had described his principal objective as the removal of discretionary policy from policymakers—something which he, following Henry Simons (1948/1936), thought essential to the maintenance of democracy. It was a happy coincidence that, as he argued, the plan would be likely to bring effective stabilization policy as well. The argument that the proposal would bring good economic results was, however, conjectural. Friedman argued that such automatic policy would be subject to shorter lags than discretionary policy, but there was no reason to suppose that the lags would be short enough, or that responses to those policy actions would be such as to bring stability. The proposal was, in principle, subject to the central criticism of Friedman (1953/1951): that the covariance of shock and policy might be such as to make the whole scheme destabilizing. But that was not true of the argument for a money supply rule made in Friedman (1960), because there the argument was empirical. It was a fact—so he argued—that actual policy had performed worse than the rule would have done. As Friedman (1960, 98) admitted, he had no very persuasive argument that targeting the money supply would work in theory, but the case for it was that it worked in practice.

The same idea—that evidence showed that policy was pushing in the right direction insufficiently often—appeared in Friedman (1968). Indeed, perhaps there Friedman came close to saying policy was perverse rather than merely ran-

dom. He said that policy should have been expansionary before 1965 when it was not, but when it became so in 1966 it went too far: “And this episode is no exception. Time and again this has been the course followed—as in 1919 and 1920, in 1937 and 1938, in 1953 and 1954, in 1959 and 1960” (1968, 16). The lesson was, then, that policymakers acting with discretion were not coming close to improving outcomes. This time there was not even a mention of Friedman (1953/1951). The whole case had become one of a lesson of experience: Stabilization policy was much more complex than it seemed, and that was shown not abstractly but by the record of failure.

The idea that “long and variable lags” create a significant difficulty for stabilization policy is a recurring theme of Friedman’s work, and surely widely recognized as such. It would not be surprising if some find it natural to see the Nobel citation as referring to that. That phrase, however, does not in itself invoke the analysis of Friedman (1953/1951). It might seem to suggest only the practical experience of the difficulties of policy rather than the argument of statistical theory that Friedman also made.

Conclusion

The lines of Friedman’s thinking arising from Friedman (1953/1951) and some of his later work on the variability of lags fits the description of the Nobel citation much better than do stories about the Phillips curve. It might at first sight seem questionable whether they are weighty enough to warrant an appearance in the Nobel citation, but it is to be recalled that, as of 1976, debate raged over monetarist proposals, and the case for rules rather than discretion was one of the issues at stake. In retrospect those lines of thinking may seem to have left less of a mark than *A Theory of the Consumption Function* or *A Monetary History of the United States*, but perhaps such marks are not reliable indicators of economic insight.

And in fact, the explanation of the muted appreciation of those lines of thinking may lie in the development of Friedman’s own thought and his presentations of the matter. He had, of course, a strong predisposition towards rule-governed policy from an early stage, for reasons of political accountability, rather than economic effectiveness. The little model of Friedman (1944) offers no more than an illustration, albeit one that captured a problem with the then-fashionable models. In Friedman (1953/1951), reacting to very much the same model, he had an argument of much more general significance, although still with the havoc caused by lags at its heart. It did mean, though, that his own proposals from Friedman (1948) were called into question as well, as there was no very clear reason to believe automatic responses would be better correlated with required policy than

discretionary ones. Then, though he continued to cite Friedman (1953/1951), his position changed in two ways. One is that he adopted the simple money supply rule, and the other is that the case he presented for preferring the rule to discretion became very largely empirical.

That case was, firstly, that retrospective observation revealed that the simple money supply rule would in fact have delivered better policy than what was actually done—a comparative result that hardly could have been argued in Friedman (1948) because it then was much less clear what outcomes would have arisen from the whole combination of reforms he had proposed and hence impossible to compare them with the actual results of policy. Secondly, there was the point that not merely would the rule have sometimes performed better than discretion, but errors in discretionary policy were frequent. The case was, precisely as Friedman (1960) put it, that rules appeared to work in practice, even though a convincing theoretical case was hard to devise. Once Friedman saw sufficient evidence of the failure of stabilization policy, the point of statistical theory from Friedman (1953/1951) may have seemed redundant to the making of his case. The point that policy which was right half the time was nowhere near good enough was certainly still important, but it was like a bonus from Friedman's point of view, rather than being the basis for conjecture about how difficult stabilization might be. In his own treatments, he then did not greatly press that argument.

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