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Rules, Discretion, and the Role of the Economic Advisor

Robert E. Lucas, Jr.

Introduction

I take the purpose of this session to be to elicit views on economic policy from economists of different points of view.¹ The particular title of the session, "Macroeconomic Policy, 1974/75: What Should Have Been Done?" does not seem to me useful for this purpose, as I will explain below, so I will adopt a somewhat different approach. I will begin by stating a variation on the policy proposals advanced by Milton Friedman in "A Monetary and Fiscal Framework for Economic Stability" (1948) and *A Program for Monetary Stability* (1959). After some speculations on why the Friedman program has had so limited an impact,² I will identify and discuss some recent developments suggesting that its acceptance and influence may be greater in the near future. The paper concludes with an assessment of the case for the Friedman program as it stands today, a brief discussion of problems of transition, and some concluding remarks.

In centering the discussion around a proposal Friedman formulated, in its essentials, thirty years ago, I run an admitted risk of locking myself and others into positions we may have taken up years ago and not rethought seriously since. The alternative strategy of repackaging this proposal in more current language is one I find distasteful, and, in any case, it

The revision has benefitted from the suggestions of Stanley Fischer, Milton Friedman, and Robert Weintraub.

1. EDITOR'S NOTE: Comments and discussion for chaps. 6 and 7 appear in chap. 7.

2. Of course, Friedman's work in general has had an enormous impact on many dimensions. I am here referring only to his recommendation that monetary and fiscal policy be conducted according to fixed rules.

would quickly be found out. I will begin, then, on familiar ground and, for the most part, remain there.

A set of aggregative policies which would I believe, lead, and have led, to satisfactory general economic performance are, compactly described:

1. A 4% annual rate of growth of M1, maintained as closely as possible on a quarter-to-quarter basis
2. A pattern of real government expenditures and transfer payments, varying secularly but not in response to cyclical changes in economic activity
3. A pattern of tax rates, also varying secularly but not in response to cyclical changes in economic activity, set to balance the federal budget *on average*
4. A clearly announced policy that wage and price agreements privately arrived at will not trigger governmental reactions of any kind (aside from standard antitrust policies and the general policy of government preference for low over high bids)

The first three of these policy rules are taken directly from Friedman's writings.³ The fourth is simply a recognition of the fact that, since the time Friedman's proposals were originally formulated, intervention in the details of private price and wage negotiations has ceased to be viewed as an emergency measure so that a position on the generally accepted aspects of aggregative policy cannot omit mention of this fact.

In restating these recommendations, I have tried to follow Friedman in being concrete and operational concerning exactly which policies are being advocated. Under the principle that *natura non facit saltum*, these particular policies must have neighbors that would have nearly the same consequences, and one would certainly like to have an analytical framework within which one could assess the consequences of variations on them. The provision of such a framework is far beyond the scope of the present paper. I will proceed, instead, in an entirely different direction: first by recalling some of the main features of the intellectual environment, both within and without our profession, into which Friedman's framework was introduced and then by tracing some of the changes since in this environment.

The Employment Act of 1946

The dominant events influencing the minds of the intended readers of Friedman's "Framework" were the Great Depression of the 1930s and

3. Rules 2 and 3 are paraphrases of those in Friedman 1948 (1953, pp. 136–37). Rule 1 is from Friedman 1959, pp. 87–92, there presented as a desirable but second-best alternative to the requirement of 100% reserve banking advocated in Friedman 1948.

the “prosperity” (as measured by unemployment rates) of the Second World War. It is difficult to imagine a sequence of events that could more forcefully illustrate both the costs of high unemployment and the ability of government policy to affect unemployment. In all capitalist countries, this “lesson” had profound influences on policy. In the United States, it was embodied in the Employment Act of 1946.

To some contemporaries, the Employment Act was “a weak and meaningless wraith” (Bailey 1950, p. 253), and in some respects it is easy to see why. The act granted the executive no powers which had not been fully assumed during the New Deal period preceding, nor did it specify either the economic targets to be achieved or the policy tools to be utilized. The act did, however, require the executive in very explicit terms to forecast the state of the economy in the coming year and to prescribe policies designed to alter this state in a desirable direction. Moreover, it was clear in specifying exactly where the expertise required to carry out this task could be found: The Council of Economic Advisors was established by the act as the channel by which this expertise could be brought to bear on practical policy.

It would be a difficult and subtle task to trace the effects of the Employment Act on the policy performance of the U.S. government in the postwar years. There is nothing subtle, however, in the effects of the act (or of the events immediately preceding it) on the practice of monetary economics in the postwar period. Renamed *macroeconomics*, this subdiscipline *defined* itself to be that body of expertise the existence of which was presupposed in the Employment Act, and its practitioners devoted themselves to the development and refinement of forecasting and policy evaluation methods which promised to be of use in the annual diagnosis-prescription exercise called for by the act.

In many respects, the assumption of this rather specific, applied role had a very healthy effect on monetary economics. The set of common, agreed-upon substantive objectives helped to unify the field and lent it a quantitative, operational character in sharp contrast with the literary, doctrinal emphasis of so much prewar monetary and business cycle theory. A great number of talented scientists found this new character congenial.

The highly productive, collective effort to make the Employment Act “work” was just getting underway when Friedman’s “Framework” was published in 1948. This was a proposal “concerned . . . with structural reform [which] should not be urged on the public unless and until it has withstood the test of professional criticism” (Friedman 1948 [1953, p. 156]). Perhaps this description may be taken as a comment on the haste with which Keynesian theory, at that time regarded as difficult and controversial, understood by only a handful of American economists, had been embodied in federal legislation. In any case, it is an accurate de-

scription of the proposals which are, implicitly, a prediction that the diagnosis and prescription process called for in the Employment Act *cannot* be made to work, given the level of scientific understanding of monetary dynamics at the time. The proposals are offered rather as a *compromise*, promising economic performance superior to that which had been observed historically, yet promising less than the performance goals which are implicit, if vague, in the Employment Act. They constituted, Friedman hoped, “a minimum program for which economists of the less extreme shades of opinion can make common cause” (Friedman 1948 [1953, p. 135]).

In retrospect, it is clear that Friedman underestimated by far the extent to which his colleagues were united in the belief that the Employment Act, together with the Federal Reserve Act as supplemented by changes in the 1930s, provided a workable policymaking apparatus. Post-World War II macroeconomics has shown little interest in reforms of the institutional framework within which economic policy is conducted, and virtually no concern with formulating legislative guidelines or limits on monetary, fiscal, and now, “incomes,” policy. The professional forum for debating alternative monetary institutions to which Friedman addressed his proposals did not analyze them, consider them, reject them in favor of others. It simply passed out of existence. Instead, within the existing institutional framework, the role of the economic expert as day-to-day manager expanded rapidly, and the role of the academic macroeconomist became that of equipping these experts with ideas, principles, formulas which gave, or appeared to give, operational guidance on the tasks with which these economic managers happened to be faced.

From the perspective of this new role for aggregative economics, the difficulty with the Friedman proposals was not so much that they were demonstrably dominated by others, but that they were irrelevant. They speak to the question: Under what rules of the game, remaining predictably in force over long periods, can we expect satisfactory economic performance? The economic manager responsible for advising on, say, the size of the coming fiscal year deficit is simply uninterested in this question: it seems to him merely an academic exercise, unrelated to the tasks he has taken it upon himself to perform.

On one level, this reaction to the Friedman proposals is understandable. General economic performance in the twenty years following the passage of the Employment Act was, by any historical standard, highly successful. It is not surprising, then, that there was little general discussion of institutional change during this period and that this lack of interest was reflected in economists' choice of research problems. Yet the history of monetary and fiscal institutions, in the United States and else-

where, is one of repeated failure, and failure at very high social cost. One is not surprised that a large fraction of the profession found it worthwhile to attempt to provide the expertise presupposed by the existing institutions. Similarly, it should surprise no one that others continued to question the viability of these institutions and focused their work on the design of alternative frameworks which might ultimately replace them.

Some Signs of Change

Events of the current decade have brought about important changes in both public and professional confidence that economic expertise can deliver satisfactory performance within the framework provided by the Employment and Federal Reserve acts. They also provide examples of mechanisms, quite outside those established by this legislation, by which public opinion may be brought to bear on economic policy. In this section, I will briefly review a few of these, beginning with what is surely the most important: the experience of stagflation.

In a first course in econometrics, students discover upward-sloping demand curves and production functions which impute negative productivity to capital. Students find these shocking experiences for which nothing in their theory courses has prepared them. This is a standard developmental crisis, like discovering that one's parents are not perfect, and experience shows that if it occurs in a reasonably protected and supportive environment, it can be survived and resolved with no lasting harm done.

There is a tendency on the part of many economists involved with Keynesian macroeconomic models to view the inflation and unemployment rate forecast errors of the 1970s in much the same terms. That is, the error itself is not denied (this is hardly a possibility) but is interpreted as indicating nothing deeper than a neglect in controlling for some other factors which, when properly taken into account, reveal the original basic structure to be sound. Thus we show our econometrics students that by controlling for income and other variables and by reducing contamination from supply side effects, the law of demand is revealed as clearly in the data as it is in the theory chapters of their textbooks.

I have argued elsewhere, most recently and comprehensively in collaboration with Thomas Sargent (Lucas 1975, Lucas and Sargent 1978), that these two cases are not at all analogous scientifically and that the misforecast of the stagflation period is in fact a symptom of much deeper problems. But a second, even clearer, difference in these two cases involves the context in which the error occurred. The stagflation error did not occur in the privacy of the seminar room, a puzzle of inter-

est to professionals only. It occurred *after* the idea of a stable inflation-unemployment trade-off had become accepted by the public generally as *the* central construct in discussing macroeconomic policy, and *after* wide public acceptance of the idea that movements along the Phillips curve were technically within the control of economic managers. Even if it were true (and I believe it is not) that the sources of this error are easily correctible and unlikely to be repeated, an enormous and far-reaching change has already taken place in the political climate in which economic issues are discussed.

Two early symptoms of this change are Arthur Laffer's influential "Laffer curve" and Arthur Okun's proposal for controlling inflation by a complex system of taxes and subsidies on individual producers. Though both can be supported by theory of sorts, provided one uses the term "theory" with sufficient looseness, neither follows in any way from any widely accepted theoretical framework, neither has received serious analysis by either proponents or critics, neither was even *mentioned* in the academic literature prior to the last year or so.

This is the legacy of stagflation: a general loss of confidence, whether scientifically warranted or not, in the formerly accepted framework guiding discretionary economic management. Since the demand for discretionary policies remains strong, we are seeing the proliferation of new "solutions" to "short-run" policy problems, defended by the promise of particular results but without basis in either theory or historical experience. Given the entry costs into economic advising of this sort, is there any real doubt what the future holds if economists continue to view themselves in a day-to-day management role?

The experience of stagflation has, then, brought about important changes in the nature of the postwar dialogue by means of which policy-oriented economists attempt to advance their ideas and to satisfy the immediate needs of economic managers. Recently, there have been a number of important developments occurring outside the now-traditional dialogue among experts and economic managers, the most striking of which has been the passage of California's Proposition 13, limiting property taxes. Similar measures are under consideration in other states and there are analogous attempts underway to influence the federal budget at the constitutional level.

The main impetus for this "tax revolt" is surely dissatisfaction over the general level of taxes and government spending, and not over the nature of stabilization policy. Yet there is a clear and instructive connection at the political level. In policies of either type, it is evidently impossible for large numbers of people to form opinions and exercise influence at anything like the level of detail at which legislators and economic managers and their advisors carry on their discussion. In contrast,

it is clearly possible for people to impose limits on these technical discussions, to *bound* levels and rates of change of economic aggregates. Public opinion generally can do little to *guide* the exercise of discretionary economic authority, but it has enormous potential to limit its scope.

To this point I have stressed developments external to the economics profession, as opposed to internal, scientific developments, as influences on the way economists and noneconomists view the possibilities open to us for influencing economic policy. This choice of emphasis reflects the opinion that public opinion generally (or what used to be called “political feasibility”) was far more important than were scientific considerations in influencing professional reaction to Friedman’s “Framework,” and that this situation is not at all unusual. (This observation is not intended as a lament: there is little to be said for isolating economics from general contemporary social thought, and the consequences of trying to do so tend to lead to reliance on sterile aesthetic criteria in guiding theoretical work.)

Nevertheless, research based on the idea of *rational expectations* has played a role in buttressing the case for thinking about policy, as Friedman argued we should, as a problem in selecting stable, predictable policy rules. The main argument turns out to be a positive (as opposed to normative) one: our ability as economists to predict the responses of agents rests, in situations where expectations about the future matter, on our understanding of the stochastic environment agents believe themselves to be operating in. In practice, this limits the class of policies the consequences of which we can hope to assess in advance to policies generated by fixed, well understood, relatively permanent rules (or functions relating policy actions taken to the state of the economy).

I have developed the reasoning underlying this point elsewhere (Lucas 1975). (Indeed, it follows from modern control-theoretic views of policy evaluation almost independently of one’s views on expectations formation.) I have been impressed both with how noncontroversial it seems to be at a general level and with how widely ignored it continues to be at what some view as a “practical” level. One could ask for no better illustration of this than the question motivating this session: “Macroeconomic Policy, 1974/75: What Should Have Been Done?” The question presupposes one of two possible situations. The first is that households and firms in 1974/75 were describable by a fixed set of decision rules, so that given any hypothetical selection of 1974/75 policies, one could simply read private-sector responses off these fixed curves to determine the response of the economy as a whole. The second situation under which this question is meaningful imagines firms and households attempting to solve maximum problems involving not only current policy actions but expected, future actions as well. The economist evaluating 1974/75

policy is in this case required to understand what these expectations about the future were, and how they would have been influenced by policy actions taken in 1974/75.

Does anyone seriously argue that either of these two situations prevails in fact? If so, on what scientific ground? If not, then why are we discussing this spuriously practical question at all?

This seems to me by far the most fundamental sense in which recent work on expectations reinforces the viewpoint toward policy which Friedman espoused in his 1948 paper. It emphasizes the fact that analysis of policy which utilizes economics in a scientific way *necessarily* involves choice among alternative stable, predictable policy rules, infrequently changed and then only after extensive professional and general discussion, minimizing (though of course never entirely eliminating) the role of discretionary economic management.

Though an agreement to focus on alternative policy *rules* would, in my view, be the major step toward restoring some degree of rationality to aggregative policy discussions, it does not necessarily follow that the particular set of rules advocated by Friedman would dominate others. On the one hand, several researchers have developed particular examples in which a 4% monetary growth rule is not dominated by monetary policies which react to the state of the economy (Sargent and Wallace 1975, Barro 1976, Lucas 1972). Moreover, Sargent (1976) has shown that one can find models of this class which account very well for the behavior of postwar, U.S. time series. On the other hand, John Taylor (1979) has developed an empirically implemented example in which monetary policies which react to the state of the system dominate (in a particular sense) a fixed monetary growth rule, though the latter is also shown, in this context, to dominate actual postwar policies. It seems clear at this point that the choice among alternative sets of policy rules will necessarily depend on the answer to difficult substantive questions involving the sources of business cycles and the nature of business cycle dynamics. Though there seems good reason to expect that the principle of rational expectations will prove to be a powerful tool in attacking these questions, it is clearly not sufficient in itself to dictate the nature of desirable countercyclical policies.

The Case for the Friedman Program

I began this paper with a brief summary of a variant of Milton Friedman's well-known program for stabilization policy, and then advanced some conjectures of a sociological nature about why professional discussion of this program has been so unsatisfactory in the past and some

reasons for believing that the terms of the discussion may now be shifting toward those which Friedman presupposed in his 1948 paper. Yet beyond an unelaborated endorsement of this program, I have devoted no space to its defense or to an assessment of its likely consequences, if adopted.

To an extent which, until a recent rereading, I had forgotten, this absence of a clear defense and assessment also characterizes Friedman's "Framework." There, in outlining his strategy, Friedman says that "I deliberately gave primary consideration to long term objectives. That is, I tried to design a framework that would be appropriate for a world in which cyclical movements other than those introduced by 'bad' monetary and fiscal arrangements, were of no consequence. I then examined the resulting proposal to see how it would behave in respect to cyclical fluctuations. It behaves *surprisingly* well . . ." (Friedman 1948 [1953, p. 133]; italics mine). How well is this? "The proposal may not succeed in reducing cyclical fluctuations to tolerable proportions. . . . I do not see how it is possible to know now whether this is the case" (Friedman 1948 [1953, p. 156]).

The strategy, then, was to design a workable stabilization policy not dependent in any way on detailed knowledge of business cycle dynamics. The program would (I think on this there is no serious professional disagreement) *fully* protect the economy against sustained inflation. It would *fully* insure against the kind of monetary collapse which was so important a factor in the early stages of the Great Depression of the 1930s. It would entirely eliminate erratic monetary and fiscal shocks as independent sources of instability. Surely these are modest claims when compared with what can be accomplished via the application of optimal control to purely hypothetical economies which provide a *complete* description of business cycle dynamics. Yet as compared with actual performance in both the distant and recent past, their appeal is evident.

In my view, recent research has added little to strengthen Friedman's case, except in what might be called a negative way. Friedman's case was built largely on the presumption of *ignorance* of the nature of business cycles. Many of us confused the methodological advances in economic dynamics that took place in the 1950s and 1960s with the substantive narrowing of this ignorance and consequently with the increasing feasibility of sophisticated, reactive countercyclical policy. We have learned, I believe, that the list of economic propositions sufficiently well grounded in theory and evidence to be useful in formulating aggregative policy is no longer now than it was in 1948. This situation is discouraging and also, I think, improvable, but in the meantime we should be grateful that, in the face of our ignorance, we can still do "surprisingly well."

The Problem of Transition

From the point of view of those involved in economic management, the position that policy should be dictated by a set of fixed rules seems at best a partial response to the question: What should be done, now? To one with some responsibility for monetary policy in 1974, say, it is not very helpful to observe that monetary growth “should have” proceeded at a constant 4% rate for the 25 years preceding. Moreover, even if a move toward a policy of fixed rules were desired, it could be done in innumerable ways, presumably with different consequences, and a criterion based on long-run average performance offers no help in choosing among them. What advice, then, do advocates of rules have to offer with respect to the policy decisions before us *right now*?

This question does have a practical, men-of-affairs ring to it, but to my ears, this ring is entirely false. It is a king-for-a-day question which has no real-world counterpart in the decision problems actually faced by economic advisors. In the current system of discretionary economic management, no one or no small group has the job of deciding what to do right now and into the middle distance with respect to the main aggregative decision variables. None of these managers is in a position to influence the economy in any significant way toward a regime of fixed, non-reactive policy rules. They are simply reacting, sometimes well, sometimes badly, to current difficulties, with no more capability of affecting policy five years hence than of affecting what happened five years before.

Economists who pose this “What is to be done, today?” question as though it were somehow the acid test of economic competence are culture-bound (or institution-bound) to an extent they are probably not aware of. They are accepting as *given* the entirely unproved hypothesis that the fine-tuning exercise called for by the Employment Act is a desirable and feasible one. In criticizing Friedman’s 1948 proposal from this point of view, they are simply missing its main point. It is not a recipe for making the Employment Act “work” but rather a prediction that it *cannot* be made to work, and an outline of an alternative set of policy arrangements.

If one does try to think in a politically serious way about possible scenarios leading to a fixed-rule regime, one is led to assign the primary roles to actors *outside* the executive–central bank system of economic management. An encouraging example is provided by the House Concurrent Resolution 133, requiring that the Federal Reserve Board announce monetary growth targets in advance and account for deviations afterward.⁴ One can imagine this resolution hardening into legally binding

4. The substance of this resolution became an amendment to the Federal Reserve Act in 1977. See Weintraub 1978.

limits on monetary growth rates. A second example is politically less advanced: movements for constitutional limits on the federal budget deficit.⁵

In cases such as these, existing economic managers will not program a transition in any formal way, though they could certainly help to minimize disruption. But the inherent gradualism of the legislative and constitutional processes will mean that any actual move toward fixed rules will necessarily occur with ample advance warning and a great deal of prior adjustment on the part of both government and the private sector. Analytical elegance will clearly not be one of the virtues of such a transition, but I see no reason to expect large economic disruption, at least by the sorry standards of the past decade, to be an inevitable or even a likely consequence.

Concluding Remarks

As an advice-giving profession we are in way over our heads. The Employment Act of 1946 placed heavy demands on the ability of economists to guide executive authority granted very broad powers. In the early postwar years, and even through the sixties, it appeared that the framework provided by the Keynesian theory of income determination was, intelligently applied, capable of meeting these demands. As confidence has ebbed in our ability to use general monetary and fiscal policy to carry out the aims of the Employment Act, professionals and nonprofessionals alike have turned to a wide variety of complex, selective interventions in individual markets. Even to begin to assess the likely consequences of these policies in anything like a scientific way is clearly well beyond the current limits of our discipline.

One response to this situation is to attempt to deal with this ever broadening range of management questions, working and hoping for advances sufficiently dramatic to enable us to regain the intellectual control we thought we had in the sixties. If, as I believe to be the case, this will require scientific improvements of a fundamental or basic nature, then this response is not likely to succeed. Basic research, to be successful, requires some degree of control over the questions to be asked and the results that can be delivered. Though stimulated by practical demands, it is rarely carried out by those in an active managerial role, even at one remove.

An alternative response is to attempt to make clear to our fellow citizens the questions that currently available expertise can hope to answer

5. For a proposed amendment to this effect, together with an economic and political analysis, see Buchanan and Wagner 1977.

successfully, to base policy recommendations on the well-understood and empirically substantiated propositions of monetary economics, discouragingly modest as these may be, and to make it as clear as possible that the main task of monetary and fiscal policy is to provide a stable, predictable environment for the private sector of the economy.

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