Central Banking in Theory and Practice

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1 Targets, Instruments, and All That

1 Introduction

I realize that these are the Robbins lectures, not the Ricardo lectures. But please pardon a momentary digression on comparative advantage nonetheless, for I have long believed that one true test of whether a person is an economist is how devoutly he or she lives by the principle of comparative advantage. And I don't mean just *preaching* it, but actually *practicing* it. For example, I always harbor doubts about my economist friends who tell me that they mow their own lawns, rather than hiring a gardener, because they actually enjoy cutting grass. Such a claim is suspect on its face. But, more to the point, a true believer in comparative advantage should be constitutionally incapable of enjoying such activities; the David Ricardo inside him should make him feel too guilty.

As a devotee of comparative advantage, the topic of these lectures virtually chose itself. Our profession boasts greater economic theorists and more skilled econometricians than I. But there must be relatively few people on earth who have been as deeply immersed in monetary policy from both the academic and central banking sides as I have. Therein, I presume, lies my comparative advantage and the topic of these three lectures: the theory and practice of central banking.

To keep things manageable, I have pared the topic beyond what a literal reading of the title may suggest. First, central bankers, I can assure you, are busy with many matters that are related tangentially if at all to monetary policy—such as managing the payments system and supervising banks. But I will stick to monetary policy proper. Second, I will deal much more with the behavior of central banks than with the monetary transmission mechanism. In these lectures, short-term interest rates are more often left-hand than right-hand variables.

The various issues covered in these three lectures are parts of a mosaic that could be taken up in many different orders. But a lecturer must draw some boundary lines, artificial though they may be, in order to divide the subject matter into lecture-sized portions. This I have done in the following way. The first lecture deals mainly with a variety of complications that a practical central banker must confront in trying to implement the classical targets-instruments approach. This presentation begs two important questions that I will address in the second lecture: What policy instrument should the central bank use? And should it attempt discretionary policy at all, rather than just relying on a simple rule? Finally, the last lecture is devoted to various positive and, especially, normative aspects of central bank independence.

As these lectures progress, it will become apparent that central banking looks rather different in practice than it does in theory. Having seen it from both sides now, I deeply believe that both theory and practice could benefit from greater contact with and greater understanding of the other. Hence, I will periodically point out opportunities for cross-fertilization—places where central bankers have more to learn from academic research, and places where academic economists could profit from greater awareness of the practical world of central banking. Arbitrage should take care of the rest!

2 Targets and Instruments: The Rudiments

Monetary policymakers have certain objectives—such as low inflation, output stability, and perhaps external balance—and certain instruments to be deployed in meeting their responsibilities, such as bank reserves or short-term interest rates. Unless it has only a single goal,[1] the central bank is forced to strike a balance among competing objectives, that is, to face up to various *trade-offs*. Unless your education in economics is very thin (or very recent!), these two sentences immediately bring to mind Tinbergen (1952) and Theil (1961). So let us begin there, at the beginning.

In theory, it works like this. There is a known model of the macroeconomy, which I write in structural form as:

$$y = F(y,x,z) + e \qquad (1)$$

and in reduced form as:

$$y = G(x,z) + e. \qquad (2)$$

Here *y* is the vector of endogenous variables (a few of which are central bank objectives), *x* is the vector of policy instruments (which may be of size one), and *z* is the vector of nonpolicy exogenous variables. The vector *e* of stochastic disturbances will fade in importance once I assume, with Tinbergen and Theil, that *F*(.) is linear and the policymaker's objective function,

$$W = W(y), \qquad (3)$$

is quadratic. In principle, the policymaker maximizes the expected value of (3) subject to the constraint (2) to derive an optimal policy "rule":

$$x^* = H(z). \qquad (4)$$

All very simple.

What's wrong with this simple framework? Both nothing and everything. Starting with "nothing," I do believe that—once you add a host of complications, several of which I will discuss in this lecture—this is the right way for a central banker to think about monetary policy. You have an economy. Except for the policy instruments you control, you must accept it as it is. You also have multiple objectives—your own, or those assigned to you by the legislature—and you must weigh them somehow, though perhaps not quadratically. To a significant extent, though usually quite informally and to my mind not quite sufficiently, central bankers do think about policy this way.

[1] One example is a central bank that must fix the exchange rate. A number of people have suggested that central banks should pursue price stability to the exclusion of all other objectives.

But, as is well known, there are many complications. Let me list just a few, some of which I will dwell on at length in the balance of this lecture and the next:

1. *Model uncertainty:* In practice, of course, we do not know the model but must estimate it econometrically. Since economists agree neither on the "right" model nor on the "right" econometric techniques, this is a nontrivial problem. It means, among other things, that policy multipliers—the derivatives of G(.) with respect to x—are subject to considerable uncertainty.

2. *Lags:* Any reasonable macroeconometric model will have a complex lag structure that is ignored by (1). This is not much of a problem in principle because, as all graduate students learn, this complication can be accommodated formally simply by appending further equations for lagged variables (see Chow 1975). However, in practice it creates serious difficulties that bedevil policymakers.

3. *Need for forecasts:* Because of lags, execution of the Tinbergen-Theil framework requires forecasts of the future paths of the exogenous variables—in principle, the entire z vector, which may be quite long. Such forecasts are neither easy to generate nor particularly accurate.

4. *Choice of instrument:* The Tinbergen-Theil framework takes as given that some variables are endogenous and others are policy instruments. In most cases, however, the central bank has at least some latitude, and maybe quite a lot, in choosing its instrument(s). One way of thinking about this is that some of the *xs* and *ys* can trade places at the discretion of the central bank. For example, the short-term interest rate can be the policy instrument and bank reserves an endogenous variable; or the central bank can do things the other way around. Some economists take this idea a little too far and write models in which central bankers can control, say, nominal GDP, the inflation rate, or the unemployment rate on a period-by-period basis. Believe me, they cannot. If they could, monetary policy would be a good deal simpler than it is

5. *The objective function:* The next problem can be framed as a question: Who supplies the objective function? The answer, typically, is: no one. The political authorities, who after all should decide such things, rarely if ever give such explicit instructions to their central banks. So central bankers must— in a figurative, not literal, sense—create their own social welfare function based on their legal mandate, their own value judgments, and perhaps their readings of the political will. This last thought brings up the independence of the central bank—a subject I will take up in depth in the third lecture.

Adding it all up, a curmudgeon could summarize the problems with applying the Tinbergen-Theil program as follows: We do not know the model, and we do not know the objective function, so we cannot compute the optimal policy rule. To some critics of "impractical" or "theoretical" economics, including some central bankers, this criticism is a show-stopper. But, speaking now as a former central banker, I think such know-nothingism is not a very useful attitude. In fact, in my view, we must use the Tinbergen-Theil approach—with as many of the complications as we can handle even if in a quite informal way. An analogy will explain why.

Consider your role as the owner of an automobile. You have various objectives toward which the use of your car contributes, such as getting to work, shopping, and going on pleasure trips. You do not literally "know" the utility function that weighs these objectives, but you presumably wish to maximize it nonetheless. The care and feeding of your car entails considerable expense, and you have great uncertainty about the "model" that maps inputs like gasoline, oil, and tires into outputs like safe, uneventful trips. Furthermore, there are substantial, stochastic lags between maintenance expenditures (e.g., frequent oil changes) and their payoff (e.g., greater engine longevity).

What do you do? One alternative is the "putting out fires" strategy: Do nothing for your car until it breaks down, then fix whatever is broken and continue driving until something else breaks down. I submit that few of us follow this strategy because we know it will produce poor results.[2] Instead, we all follow something that approximates—philosophically, if not mathematically—the Tinbergen-Theil framework. Central banks do, too. Or at least they should, for they will surely fail in their stabilization-policy mission if they simply "put out fires" as they observe them. Let me review briefly how the Tinbergen-Theil framework is used in practice.

To begin with, there must be a macro model. It need not be a system of several hundred stochastic difference equations, though that is not a bad place to start. In fact, no central bank that I know of, and certainly not the Federal Reserve, is wed to a single econometric model of its economy. Some banks have such models, and some do not. But, even if they do not, or do not use it, *some* kind of a model—however informal—is necessary to do policy, for otherwise how can you even begin to estimate the effects of changes in policy instruments?

Some central bankers scoff at large-scale macroeconometric models, as do some academic economists. And their reasons are not all that dissimilar. Many point, for example, to the likelihood of structural change in any economy over a period of several decades, which casts doubt on the stationarity assumptions that underlie standard econometric procedures and thus on the bedrock notion that the past is a guide to the future. Others express skepticism that something as complex as an entire economy can be captured in any set of equations. Still other critics emphasize a host of technical problems in time series econometrics that cast doubt on any set of estimated coefficients. Finally, some central bankers simply do not understand these ungainly creatures at all and doubt that they should be expected to.

Leaving aside the last, there is truth in each of these criticisms. Every model is an oversimplification. Economies do change over time. Econometric equations often fail subsample stability tests. Econometric problems like simultaneity, common trends, and omitted variables are ubiquitous in nonexperimental data. The Lucas critique warns us that some parameters may change when policy does.[3] Yet what are we to do about these problems? Be skeptical? Of course. Use several methods and models instead of just one? Certainly. But abandon all

[2] In the engineering literature on control of nonlinear systems in which the model is only an approximation to reality, smoothing of control instruments is often recommended because sudden, large reversals of instrument settings may set off unstable oscillations. A related problem in the economics literature is instrument instability (Holbrook 1972).

[3] See Lucas (1976). While the Lucas critique has generated a huge amount of academic interest, it seems hardly to concern practical central bankers. Perhaps this is because they do not believe in regime changes. Or perhaps it is because they do not believe in econometric models. Probably both.

econometric modeling? I think not. The criticisms of macroeconometrics are not wrong, but their importance is often exaggerated and their implications misunderstood. These criticisms should be taken as warnings—as calls for caution, humility, and flexibility of mind—not as excuses to retreat into econometric nihilism. It is foolish to make the best the enemy of the moderately useful.

Indeed, I would go further. I don't see that central bankers even have the luxury of ignoring econometric estimates. Monetary policymaking requires more than just the qualitative information that theory provides—e.g., that if short-term interest rates rise, real GDP growth will subsequently fall. They must have quantitative information about magnitudes and lags, even if that information is imperfect.

I often put the choice this way: You can get your information about the economy from admittedly fallible statistical relationships, or you can ask your uncle. I, for one, have never hesitated over this choice. But I fear there may be altogether too much uncle-asking in government circles in general, and in central banking circles in particular. For example, a long line of politicians will assure you that a lower capital gains tax rate will spur investment. The only trouble is that no evidence supports this contention. Similarly, central bankers often take it as axiomatic that long-term interest rates are good forecasters of either (a) inflation or (b) future short-term interest rates. Unfortunately, the data refute both claims.

3 Uncertainties: Models and Forecasts

Let me now turn to the first of three important amendments to the Tinbergen-Theil framework, beginning with the obvious fact that no one knows the "true model." It would hardly have been news to Tinbergen and Theil that both models and forecasts of exogenous variables are subject to considerable uncertainties. And subsequent developments by economists have provided ways of handling or finessing hese gaps in our knowledge.[4] Let us consider, very briefly, three types of uncertainty.

Uncertainty about forecasts: In the linear-quadratic case, uncertainty about the values of future exogenous variables is no problem *in principle;* you need only replace unknown future variables with their expected values (the "certainty equivalence" principle). But here is one case in which the gap between theory and practice is huge because the task of generating unbiased forecasts of dozens or even hundreds of exogenous variables is a titanic practical problem. It is, for example, a major reason why large-scale econometric models are not terribly useful as forecasting tools.[5]

Skeptics often object to certainty equivalence on the grounds that (a) the economy is nonlinear and (b) there is no particular reason to think that the objective function is quadratic. Both

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[4] In Knight's (1921) terminology, these methods apply to cases of "risk" rather than "uncertainty." Risk arises when a random variable has a known probability distribution; uncertainty arises when the distribution is unknown. In the real world we are normally dealing with uncertainty rather than risk. And here, almost by definition, formal modeling gives us little guidance.

[5] I should clarify what I mean. Used mechanically, the large models are not very good at forecasting "headline" variables like GDP and inflation—which is why virtually no model proprietors use them this way. But econometric models are an essential tool in enforcing the consistency you need to forecast the hundreds of variables in a typical macro model.

objections are undoubtedly true and, if taken literally, invalidate the certainty-equivalence principle. But I think the importance of this point is often exaggerated by those who would denigrate the usefulness—and thereby escape the discipline—of formal econometric models. Policymakers almost always will be contemplating changes in policy instruments that can be expected to lead to small changes in macroeconomic variables. For such changes, any model of an economy is approximately linear and any convex objective function is approximately quadratic.[6] So this problem of principle is, in my view, of great practical importance only on those rare occasions when large changes in policy are contemplated.

Uncertainty about parameters: Uncertainty about parameters, and hence about policy multipliers, is much more difficult to handle, even at the conceptual level. Certainty equivalence certainly does not apply. While there are some fairly sophisticated techniques for dealing with parameter uncertainty in optimal control models with learning, those methods have not attracted the attention of either macroeconomists or policymakers. There is good reason for this inattention, I think: You don't conduct experiments on a real economy solely to sharpen your econometric estimates.

There is, however, one oft-forgotten principle that I suspect practical central bankers can—and in a rough way do—rely on. Many years ago, William Brainard (1967) demonstrated that, under certain conditions,[7] uncertainty about policy multipliers should make policymakers *conservative* in the following specific sense: They should compute the direction and magnitude of their optimal policy move in the way prescribed by Tinbergen-Theil and then do less.

Here is a trivial adaptation of Brainard's simple example. Simplify equation (2) to:

$$y = Gx + z + e, \qquad (2')$$

and suppose that G and z are independent random variables with means g and $\bar{z}$ respectively. The policymaker wishes to minimize $E(y - y^*)^2$. Interpret $z + e$ as the value of y in the absence of any further policy move ($x = 0$) and x as the contemplated change in policy.[8] If G is nonrandom, the optimal policy adjustment is certainty equivalence:

$$x = (y^* - \bar{z})/G,$$

that is, fully closing the expected gap between y^* and $\bar{z}$. But if G is random with mean g and standard deviation σ, the loss function is minimized by setting:

$$x = \frac{y^* - \bar{z}}{g + \frac{\sigma^2}{g}},$$

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[6] This statement seems to be a straightforward application of Samuelson's (1970) analogous proposition in the context of portfolio theory.

[7] One very important condition is that covariances are small enough to be ignored. With sizable covariances, anything goes.

[8] In the case of monetary policy, defining "no change" ($x = 0$) is actually a nontrivial problem. I will discuss where to locate the "zero" point on the monetary-policy scale in the next lecture.

which means that policy aims to fill only part of the gap.

My intuition tells me that this finding is more general—or at least more wise—in the real world than the mathematics will support.[9] And I certainly hope it is, for I can tell you that it was never far from my mind when I occupied the Vice Chairman's office at the Federal Reserve. In my view as both a citizen and a policymaker, a little stodginess at the central bank is entirely appropriate,

Uncertainty over model selection: Parameter uncertainty, while difficult, is at least a relatively well defined problem. Selecting the right model from among as variety of non-nested alternatives is another matter entirely. While there is some formal literature on this problem,[10] I think it is safe to say that central bankers neither know nor care much about this literature. I leave it as an open question whether they are missing much.

My approach to this problem while on the Federal Reserve Board was relatively simple: Use a wide variety of models and don't ever trust any one of them to much. So, for example, when the Federal Reserve staff explored policy alternatives, I always insisted on seeing results from (a) our own quarterly econometric model, (B) several alternative econometric models, and (c) a variety of vector autoregressions (VARs) that I developed for this purpose. My usual procedure was to simulate a policy on as many of these models as possible, throw out the outlier(s), and average the rest to get a point estimate of a dynamic multiplier path.

This can be viewed as a rough—make that very rough—approximation to optimal information processing.[11] As they say: Good enough for government work!

4 Lags in Monetary Policy

It is a commonplace that monetary policy operates on the economy with "long and variable lags." As I noted previously, the formalism of the Tinbergen-Theil framework can readily accommodate distributed lags. The costs are two-fold. First, the dimensionality of the problem increases; but with modern computing power this is not much of a problem. Second, the optimization problem changes from one of calculus to one of dynamic programming.[12] This latter point is significant in practice and, I think, inadequately appreciated by practitioners.

A dynamic programming problem is typically "solved backward," that is, if *T* is the final period and *x* is the policy instrument, you first solve a one-period optimization problem for period *T*, thereby deriving ${}_{t}x_{T}$ conditional on a past history. (The postscript denotes calendar time and the

[9] With many random variables and nonzero covariances, the mathematics does not "prove" that conservatism is optimal. In some cases, parameter uncertainty will actually produce greater activism.
[10] One strand, derived from the optimal control literature, deals with choosing among rival models. Another strand, due to Hendry and his collaborators, focuses on encompassing tests. See, for example, Hendry and Mizon (1993).
[11] True optimal information processing would require weighting by a variance-covariance matrix.
[12] Kydland and Prescott (1977) showed that it is an error to pursue dynamic programming mechanically if private agents base decisions on expectations about future policy. In that case, expectational reactions to policy must be taken into account. I take up Kydland and Prescott's critique in the next lecture; here I use the term "dynamic programming" generically, intending to include such reactions of expectations.

prescript denotes the date at which the expectation is taken.) Then, given your solution for ${}_tx_T$ which most likely depends *inter alia* on ${}_tx_{T-1}$, you solve a two-period problem for ${}_tx_T$and ${}_tx_{T-1}$ jointly. Proceeding similarly, by a process of backward induction you derive an entire *solution path:*

$$x_t, {}_tx_{t+1}, {}_tx_{t+2}, \ldots, {}_tx_T.$$

Don't get me wrong. I do not believe it is important for central bankers to acquire any deep understanding of Bellman's principle, still less of the computational techniques used to implement it. What really matters for sound decisionmaking is the way dynamic programming teaches us to think about intertemporal optimization problems—and the discipline it imposes. It is essential, in my view, for central bankers to realize that, in an dynamic economy with long lags in monetary policy, today's monetary policy decision must be thought of as the first step along a path. The reason is simple: Unless you have thought through your expected future actions, it is impossible to make today's decision rationally. For example, when a central bank begins a cycle of either tightening or easing, it should have some idea about where it is going before it takes the first step.

Of course, by the time period $t + 1$ rolls around, the policymaker will have new information and may wish to change his or her mind about the earlier tentative decision ${}_tx_{t+1}$. That is fine. In fact, given the information then available, the policymaker will want to plan an entirely new path:

$$x_{t+1}, {}_{t+1}x_{t+2}, {}_{t+1}x_{t+3}, \ldots, {}_{t+1}x_T$$

But that realization in no way obviates the need to think ahead in order to make today's decision—which is the important lesson of dynamic programming. It is an intensely practical lesson and, I believe, one that is inadequately understood.[13]

Too often decisions on monetary policy—and, indeed, on other policies—are taken "one step at a time" without any clear notion of what the next several steps are likely to be. In central banking circles, it is often claimed that such one-step-at-a-time decisionmaking is wise because it maintains "flexibility" and guards against getting "locked in" to decisions the central bank will later regret. I often heard sentiments like this expressed both at FOMC meetings and at international meetings of central bankers.

But this attitude reflects a fundamental misunderstanding of the way dynamic programming teaches us to think. It is absolutely correct that flexibility should be maintained and that locking yourself in should be avoided. But both of these notions are inherent in dynamic programming. If there are any surprises at all, the decisions that you actually carry out in the future will differ from the ones you originally planned. That's flexibility. Ignoring your own likely future actions is myopia.

[13] Beginning in June 1997, the Reserve Bank of New Zealand began publishing a three-year projection for monetary policy, stating clearly that only the next quarter's monetary policy is "desired"—the others are merely "projected" and subject to change. This closely approximates the dynamic programming approach. I am extremely grateful to the Bank's Governor, Donald Brash, for calling this innovation to my attention.

These matters are really quite intuitive. Despite their lack of understanding of the fine points of the calculus of variations, ordinary rational people do not deem it wise to ignore the admittedly unknown future in order to "maintain flexibility." Think, for example, about students formulating educational and career plans. In choosing a major, and sometimes even in choosing a college, many undergraduates are looking ahead to their ultimate career objectives. They know their crystal ball is cloudy, and they realize that they may have many reasons to change their minds along the way. But they nonetheless find it rational to plan ahead when making the initial decision. And they are right.

Applying this abstract discussion to a concrete problem in monetary policy may help resolve a long-standing issue in central banking. Policymakers in the United States and elsewhere have often been accused of making a particular type of systematic error in the timing of policy changes. Specifically, it is alleged that they overstay their policy stance—whether it is tightening or loosening—thereby causing overshoots in both directions.[14] I believe this criticism may be correct, although I know of no systematic study that demonstrates it. I furthermore believe that the error, if it exists, may be due to following a strategy I call "looking out the window."

The error is well illustrated by what I call the parable of the thermostat. The following has probably happened to you; it has certainly happened to me. You arrive at night in an unfamiliar hotel and find the room temperature too cold. So you turn up the heat and take a shower. Emerging 10 minutes later, you still find the room too cold. So you turn the heat up another notch and go to sleep. At about 2 a.m. you wake up in a pool of sweat in a room that is oppressively hot.

By analogy, a central bank following the "looking out the window" strategy proceeds as follows. For concreteness, suppose it is in the process of tightening. At each decision making juncture, the bank takes the economy's temperature and, if it is still too hot, tightens monetary conditions another notch. Given the long lags in monetary policy, you can easily see how such a strategy can keep the central bank tightening for too long.

Now compare "looking out the window" to proper dynamic optimization. Under dynamic programming, at each stage the bank would project an entire path of future monetary policy actions, with associated paths of key economic variables. It would, of course, act only on today's decision. Then, if things evolved as expected, it would keep following its projected path, which would be likely (given the lags in monetary policy) to tell it to stop tightening while the economy was still "hot." Of course, economies rarely evolve as expected. Surprises are the norm, not the exception, and they would induce the central bank to alter its expected path in obvious ways. If the economy steamed ahead faster than expected, the bank would tighten more. If the economy slowed down sooner than expected, the bank would tighten less or even reverse its stance.

Do central banks actually behave this way? Yes and no. Like a skilled billiards player who does not understand the laws of physics, a skilled practitioner of monetary policy may follow a dynamic-programming-type strategy intuitively and informally. In the last few years, for example, the notion that it is wise to pursue a strategy of "preemptive strikes" against inflation seems to have caught on among central bankers. The main impetus for this change in fashion

[14] See, for example, Meltzer (1991).

was, I believe, the leadership and perceived success of the Federal Reserve in first tightening monetary policy "preemptively" in early 1994 and then achieving the fabled "soft landing." By now, a variety of other central banks are talking the same talk. But the very fact that this style of decisionmaking was perceived to be a great advance suggests that the dynamic programming way of thinking has not yet permeated central banking circles.

A preemptive strategy implies a certain amount of confidence in both your forecast and your model of how monetary policy affects the economy, both of which are hazardous. But preemption does not require too much confidence. Remember the flexibility principle of dynamic programming and the Brainard conservatism principle. Taken together, they lead to the following sort of strategy:[15]

Step 1. Estimate how much you need to tighten or loosen monetary policy to "get it right." Then do less.

Step 2. Watch developments.

Step 3a. If things work out about as expected, increase your tightening or loosening toward where you thought it should be in the first place.

Step 3b. If the economy seems to be evolving differently from what you expected, adjust policy accordingly.

Two final points about preemptive monetary policy are worth making. First, a successful stabilization policy based on preemptive strikes will appear to be misguided and may therefore leave the central bank open to severe criticism. The reason is simple. If the monetary authority tightens so early that inflation never rises, the preemptive strike is a resounding success, but critics of the central bank will wonder—out loud, no doubt—why the bank decided to tighten when the inflationary dragon was nowhere to be seen. Similarly, a successful preemptive strike against economic slack will prevent unemployment from rising and leave critics complaining that the authorities were hallucinating about rising unemployment. Precisely these criticisms of the Fed's tightening in 1994-1995 and subsequent easing in 1995-1996 were heard in the United States in recent years.

Second, the logic behind the preemptive strike strategy is symmetric. The same reasoning that tells a central bank to get a head start against inflation says it should also strike preemptively against rising unemployment. That is why Chairman Alan Greenspan told Congress in February 1995, just after the Fed had completed a year-long tightening cycle that raised short-term interest rates 300 basis points, that: "There may come a time when we hold our policy stance unchanged, or even ease, despite adverse price data, should we see signs that underlying forces are acting ultimately to reduce inflationary pressures."[16] In fact, the statement itself amounted to a

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[15] This strategy has a temporal aspect not found in Brainard's analysis, and hence may embody a leap of faith. But Aoki (1967) offered a dynamic generalization of Brainard's result. Nonetheless, Aoki's result, like Brainard's, is fragile and may not survive, e.g., nonnegligible covariances.

[16] From testimony given to committees of both the House and Senate on February 22 and 23, 1995, printed in *Federal Reserve Bulletin*, April 1995, p. 348.

monetary easing, since it fueled a bond-market rally well before the Fed started cutting interest rates (which did not occur until July 1995). Notably, both Greenspan's statement and the Fed's interest-rate cut in July 1995 came while the unemployment rate was below contemporary estimates of the natural rate.

Under what circumstances might the preemptive strike strategy apply more to fighting inflation than to fighting unemployment?

First, if the short-run Phillips curve is distinctly nonlinear in the way Phillips originally drew it, so that low unemployment raises inflation more than high unemployment lowers it. But, with due apologies to those notable curve-fitting exercises done at the London School of Economics in the 1950s, the U.S. evidence is decidedly against this hypothesis. A linear Phillips curve fits the data extremely well,[17] and tests for nonlinearity suggest, if anything, a concave (to the origin) Phillips curve rather than with a convex one.[18]

Second, the central bank's loss function could attach much more weight to inflation than to unemployment—as some observers of central banking have suggested, and as some central bank charters (but not the Fed's) mandate.

Third, lags in monetary policy could be longer for inflation fighting than for unemployment fighting, calling for earlier preemption in the former case. This last circumstance appears to obtain, and may be the main justification for acting more preemptively against inflation than against unemployment.

Note, however, that political considerations most likely point in the opposite direction. In most situations, the central bank will take far more political heat when it tightens pre-emptively to avoid higher inflation than when it eases preemptively to avoid higher unemployment.

5 Central Banking by Committee

So far, I have offered one explanation for the alleged tendency of central banks to overstay their stance—remaining tight for too long, thereby causing recessions, and remaining easy for too long, thereby allowing inflation to take root: a failure to internalize the dynamic-programming way of thinking. But a prominent institutional feature of some central banks (including the Federal Reserve) may also contribute to this problem. Specifically, in many countries monetary policy is made not by a single individual but by a *committee.*

While serving on the FOMC, I was vividly reminded of a few things all of us probably know about committees: that they laboriously aggregate individual preferences; that they need to be led; that they tend to adopt compromise positions on difficult questions; and—perhaps because of all of the above—that they tend to be inertial. Had Newton served on more faculty committees at Cambridge, his first law of motion might have read: A decisionmaking body at rest or in

[17] See Gordon (1997).
[18] See Eisner (1996)

motion tends to stay at rest or in motion in the same direction unless acted upon by an outside force.

Inertial behavior has its virtues, as I will explain shortly. But it also has some vices. In particular, decisionmaking by committee may contribute to the systematic policy errors I have mentioned already by inducing the central bank to maintain its policy stance too long.

While the Federal Open Market Committee has not been immune to this ailment over the years, there is at least one tradition at the Federal Reserve that tends to minimize it: that of the powerful chairman. The law says that each of the 12 voting members of the FOMC has one vote. But no one has ever doubted that Alan Greenspan, or Paul Volcker, or Arthur Burns were "more equal" than the others. The Chairman of the Federal Reserve Board is virtually never on the losing side of a monetary policy vote. So, to a significant extent, FOMC decisions are *his* decisions, as tempered by the opinions of the other members. Nonetheless, a chairman who needs to build consensus may have to move more slowly than if he were acting alone.

Now for the positive side. America is the land of checks and balances. Our political traditions harbor great fear of unbridled, centralized power. It is an anti-government form of government—the little government that couldn't because it was too tied up in knots. Yet the Federal Open Market Committee has virtually total freedom to do as it pleases with monetary policy—without asking permission from any other branch of government and with little fear of being countermanded. So long as FOMC decisions are done by the book and remain within the Fed's legal authority, the committee is neither checked nor balanced—at least not externally.

But the group nature of FOMC decisions creates what amounts to an *internal* system of checks and balances. No chairman can deviate too far from the view that prevails in his committee. Decisionmaking by committee, especially when there is a strong tradition of consensus, makes it very difficult for idiosyncratic views to prevail.[19] So monetary policy decisions tend to regress toward the mean and to be inertial—and hence biased in just the same way that adaptive expectations are biased relative to rational expectations. But errors like that, while systematic, will generally be small and will tend to shrink over time. And, in return, the system builds in natural safeguards against truly horrendous mistakes.

I leave it to some clever theorist to prove that the FOMC is an example of optimal institutional design. My own hunch is that, on balance, the additional monetary policy inertia imparted by group decisionmaking provides a net benefit to society. It does, at least, provide something of a check against an overzealous Fed chairman. But my main point is simpler: My experience as a member of the FOMC left me with a strong feeling that the theoretical fiction that monetary policy is made by a single individual maximizing a well-defined preference function misses something important. In my view, monetary theorists should start paying some attention to the nature of decisionmaking by committee, which is rarely mentioned in the academic literature.[20]

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[19] Curiously, there is no such tradition of consensus decisionmaking on the U.S. Supreme Court, where 5-4 votes occur about 20% of the time. But over the last 20 years there has been only one 7-5 vote and one 6-4 vote on the FOMC, and there have been only seven other votes with four dissents.

[20] An interesting exception is Faust (1996).

6 Conclusion

Overall, however, the message of this lecture is rather cheerful. Working in their cloistered universities, Tinbergen, Theil, Brainard, and others taught valuable abstract lessons that turned out to be of direct practical use in central banking. So did other scholars who developed their ideas further, pointed out additional complexities, and brought more powerful technical tools to bear—such as macroeconometric models and dynamic programming. Their ideas do not provide pat answers for central bankers, and their techniques cannot be applied mechanically. The real world is much too complicated for that. So there must be both art and science in central banking. Nonetheless, the science is still useful; at least I found it so while on the Federal Reserve Board.

In fact, I think central bankers could learn a good deal more from the academics. For example, I emphasized that the dynamic programming way of thinking is not sufficiently ingrained into the habits of monetary policymakers, who too often just "look out the window" and base policy judgments on present circumstances. I believe this is a fundamental mistake and is one reason why central banks often overstay their policy stance.

There also seems to be much too much reliance on "uncle asking," relative to econometric evidence, among practical central bankers. Skepticism about econometric estimates is one thing, and is highly appropriate. But healthy skepticism should not be allowed to devolve into econometric nihilism, which is too often an excuse for wishful thinking and an escape from the discipline of the data.

But please don't think I believe that all wisdom resides in universities, from where it flows, somewhat impeded, to central banks. The next lecture should dispel that notion completely. After dealing briefly with one more issue where academic thinking was both correct and triumphant—the choice of monetary instrument—I will turn in detail to the rules versus discretion debate, where I will argue that much recent academic research has been barking up the wrong—or, rather, nonexistent—trees. On that issue, I believe, the academics must learn from the central bankers, and the sooner the better.

2 Choosing and Using a Monetary Policy Instrument

1 Introduction

In dealing with the pervasive uncertainties that surround monetary policy in the first lecture, I swept one very basic issue under the rug: What is the policy lever? What instrument does the central bank actually control?

The Tinbergen-Theil framework elides one of the most enduring controversies in monetary theory simply by labeling some variables as "targets" and others as "instruments," as if that were their birthright. Plainly, it is not. Central bankers all over the world must choose their policy instruments, so I begin this second lecture with a few thoughts on that choice. Although the ground is mostly well ploughed, I will introduce some new thoughts by proposing an answer to a question that has long bedeviled both practitioners and students of monetary policy: How do we define a "neutral" monetary policy?

The greater part of this lecture, however, will be devoted to an even more basic and contentious issue: whether it makes sense to attempt discretionary monetary policy at all, rather than rely on a simple mechanical rule—the age-old debate over rules versus discretion. Here, as I suggested at the end of the first lecture, I will be sharply critical of a large body of recent adacemic research which has, in my view, made insufficient contact with reality.

2 The Choice of Monetary Instrument

In simple models, beginning with Poole (1970), the choice of monetary instrument is often posed as a contest between the rate of interest, *r*, and the money supply, *M*. In one case, *r* is the instrument, and *M* is an endogenous variable. In the other case, the roles are reversed. This dichotomy, of course, is both too confining and too simple. In reality, there are many more choices—including various definitions of *M*, several possible choices for *r*, bank reserves, and the exchange rate. Furthermore, it is doubtful that any interesting definition of *M* or any interest rate beyond the overnight bank rate can be controlled tightly over very short periods of time like a day or a week. In the United States, the federal funds rate and bank reserves are probably the only viable options. But other variables like the *Ms* become candidates if the control period is longer—say, a quarter—and the control tolerances are wider.

The intellectual problem is straightforward in principle. For any choice of instrument, you can write down and solve an appropriately complex dynamic optimization problem, compute the minimized value of the loss function, and then select the *minimum minimorum* to determine the optimal policy instrument. In practice, this is a prodigious technical feat that is rarely carried out.[21] And I am pretty sure that no central bank has ever selected its instrument this way. But, then again, billiards players may practice physics only intuitively.

Returning to Poole's dichotomy, let me remind you of his basic conclusion: that large LM shocks militate in favor of targeting interest rates while large IS shocks militate in favor of targeting the money supply.[22] After Poole's seminal paper, monetary theorists devoted much attention to the question he posed and tackled it in a variety of ways. One such contribution by Sargent and Wallace (1975), in fact, turned out to be among the opening salvos in the rational expectations debate.

Much of the scholarly literature was worthwhile and intellectually fascinating. But in the end, real-world events, not theory, decided the issue. Ferocious instabilities in estimated LM curves in the United States, United Kingdom, and many other countries, beginning in the 1970s and continuing to the present day, led economists and policymakers alike to conclude that money-supply targeting is simply not a viable option. Some facts about the U.S. monetary aggregates illustrate just how strong this evidence is.

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[21] A few papers in this spirit are Tinsley and von zur Muehlen (1981), Brayton and Tinsley (1994), and Bryant, Hooper, and Mann (1993).

[22] Covariances and slopes of the IS and LM curves also matter. I ignore them here.

Surely the weakest version of monetarism must be the notion that money and nominal income are cointegrated, for with no such long-run relationship why would anyone care about the behavior of the *Ms?* Yet a series of cointegration tests for M1 and nominal GDP in the United States, using rolling samples of quarterly data beginning in 1948:1 and ending at various dates, fail to reject the hypothesis of no cointegration as soon as the endpoint of the sample extends into the late 1970s.[23] [3] That is, the natural logs of M1 and nominal GDP are cointegrated only for sample periods like 1948-1975, not since then.

Apparent cointegration between either M2 or M3 on the one hand and nominal GDP on the other lasts longer. But it, too, disappears into a black hole in the 1990s. In fact, this statement is actually far too kind to M2 or M3 monetarism, since data limited to periods like 1948-1980 fail to indicate cointegration. A cointegrating vector appears only when the sample is extended well into the 1980s, but then it disappears again as data from the 1990s are appended. In a word, no sturdy long-run statistical relationship exists between nominal GDP and *any* of the Federal Reserve's three official definitions of M for *any* sample that includes the 1990s.

Because of stark facts like these, interest rate targeting won by default in the United States and elsewhere. As Gerry Bouey, a former governor of the Bank of Canada, put it, "We didn't abandon the monetary aggregates, they abandoned us." I often put the issue this way to economists with monetarist leanings (a vanishing breed, to be sure): If you want the Fed to target the growth rate of M, you must first answer two questions: which definition of M, and how fast should it grow? In recent years, these questions have proven to be show-stoppers—for no one has coherent answers.

The death of monetarism does not make it impossible to pursue a monetary policy based on rules. But it does mean that the rule cannot be a money-growth rule. I will return to the broader rules-versus-discretion debate shortly.

Was the theoretical literature on the choice of monetary instrument therefore useless to practitioners? Absolutely not. In fact, it is hard to think of an aspect of monetary policy in which theory and practice have interacted more fruitfully. Poole's conclusion in theory was that instability in the LM curve should push central banks toward targeting short-term interest rates. In practice, LM curves became extremely unstable and one central bank after another abandoned any attempt to target monetary aggregates.

In the case of the Federal Reserve, the brief and tumultuous experiment with monetarism between 1979 and 1982 was probably more a marriage of convenience than infatuation. Monetarist rhetoric provided the Fed with a political heat shield as it raised interest rates to excruciating heights. In any case, the Fed began the gradual process of backing away from M targets in 1982. The target growth range for M1 was formally dropped in 1987, but growth targets for M3 and, especially, M2 retained some subsidiary role in monetary policy formulation into 1992—at least putatively. Finally, in February 1993, Fed Chairman Alan Greenspan announced with magnificent understatement that the Fed was giving "less weight to monetary

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[23] These tests allow for time trends in velocity, that is, they do not assume proportionality between nominal GDP and money.

aggregates as guides to policy."[24] Less? How about zero? Greenspan's proclamation was greeted with yawns in both academia and the financial markets because it was considered old news.

As usual, however, laws lag far behind both academic knowledge and central bank practice. The Humphrey-Hawkins Act, a 1978 law which is still on the books, requires the Federal Reserve to report its target ranges for money growth to Congress twice a year. This the Fed dutifully does. But it is an empty ritual. The relevance to policy eludes all concerned.

3 Real Interest Rates and "Neutral" Monetary Policy

So interest rates won by default. But what interest rate should the monetary authority try to control? And can it succeed?

Most empirically-oriented economists would agree with the following proposition, which seems to pose a major dilemma for monetary policy: The interest-sensitive components of aggregate demand react mainly to the real long rate while the central bank controls only the nominal short rate. In other words, the interest rate that the central bank can control doesn't matter (much), and the rates that really matter cannot be controlled. On the surface, this seems a devastating conundrum. But things are not quite as bad as they appear.

Note that two separate distinctions are being made here: nominal interest rates are not real rates, and short rates—are not long rates. In this lecture, I would like to focus on the real-nominal distinction. So I hope you will grant me the liberty to proceed under the assumption that the expectations theory of the term structure appropriately links long rates to short rates—a convenient fiction that I will debunk in the following lecture.

In the contemporary United States, virtually all academic and market observers agree that the Federal funds rate—the overnight rate in the interbank market for reserves—is the Federal Reserve's central policy instrument.[25] And so does the Fed. But the Federal funds rate is, of course, a nominal rate, which means that the FOMC must act nominal while thinking real. Fortunately, this bit of mental gymnastics is not too hard to perform in the short run because inflationary expectations are, under normal circumstances, quite sluggish. So the Fed can be reasonably confident that short-run changes in the nominal Fed funds rate signify changes in the real Fed funds rate.

In the long run, however, telling the two apart is not so simple; and mistakes, if uncorrected, can be terribly damaging. The reason has been well known for years. Suppose that, in choosing a nominal interest rate, the central bank mistakenly sets the real interest rate too high. Such an error will restrict aggregate demand, eventually open up a GDP gap, and, with a lag, start to bring inflation down. If the central bank fails to adjust its nominal interest rate downward as inflation falls, the real rate will grow even larger. This spells trouble. The GDP gap expands,

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[24] Statement to the Committee on Banking, Housing, and Urban Affairs, U.S. Senate, February 19, 1993, printed in the *Federal Reserve Bulletin*, April 23, 1993, p. 298.

[25] Bernanke and Blinder (1992) argued several years ago that the Fed has long used the Federal funds rate as its policy instrument. Most research since then has confirmed this finding, though perhaps with some modifications. (See Bernanke and Mihov [1995].)

inflation falls faster, and real rates rise even more. The economy is put into a disinflationary tailspin.

The opposite happens if the nominal interest rate is accidentally pegged at a level that makes the real interest rate too low. In that case, loose monetary policy eventually leads to an overshoot of potential GDP and, thereby, to higher inflation. If the central bank holds the nominal interest rate fixed as inflation rises, the real rate falls even lower, aggregate demand gets stimulated even more, and the economy is off to the inflationary races.

The moral of the story is simple: Stubbornly pegging the nominal interest rate while inflation is changing (in either direction) is likely to be hazardous to your economy's health. Before too long, the central bank must adjust its nominal rate so as to guide the real rate back toward its *neutral* setting.

What was that word again? Neutral? I must pause a moment to examine a concept that is prominent in the financial press these days, even though it has no agreed-upon definition: the *neutral real interest rate.* Let me first propose a definition, and then defend it.[26]

At any point in time, given all the standard determinants of aggregate demand—including fiscal policy, the exchange rate, and the spending propensities of consumers and investors—the economy has some *steady-state IS curve.* By this I mean the IS curve that will prevail once all the lags have worked themselves out, and provided all random shocks are set to zero. Specifically, if the normal IS curve is written:

$$y = f(y_{-1}, r, x, G, \ldots) + e,$$

the steady-state IS curve is:

$$y = f(y, r, x, G, \ldots).$$

It is labelled "IS" in figure 2.1.

I propose to define the neutral real interest rate, *r**, as the interest rate that equates GDP along this steady-state IS curve to potential GDP, y*; implicitly:

$$y^* = f(y^*, r^*, x, G, \ldots).$$

Graphically, it is defined by the point in the graph at which the steady-state IS curve intersects the vertical line at potential GDP. Notice several critical features of this definition.

First, if the real interest rate is below the neutral rate, aggregate demand will eventually exceed potential GDP, leading to higher inflation. Conversely, a real interest rate above neutral will ultimately be disinflationary. Thus the proposed definition of neutrality is oriented entirely

[26] The basic idea, of course, dates back to Wicksell (1898).

toward the control of inflation, as seems appropriate given that price stability is the primary long-run responsibility of any central bank. According to my proposed definition, "neutral" monetary policy is consistent with constant inflation in the medium run. Any higher real interest rate constitutes "tight money" and will eventually imply falling inflation; and any lower real rate is "easy money" and signals eventually rising inflation.

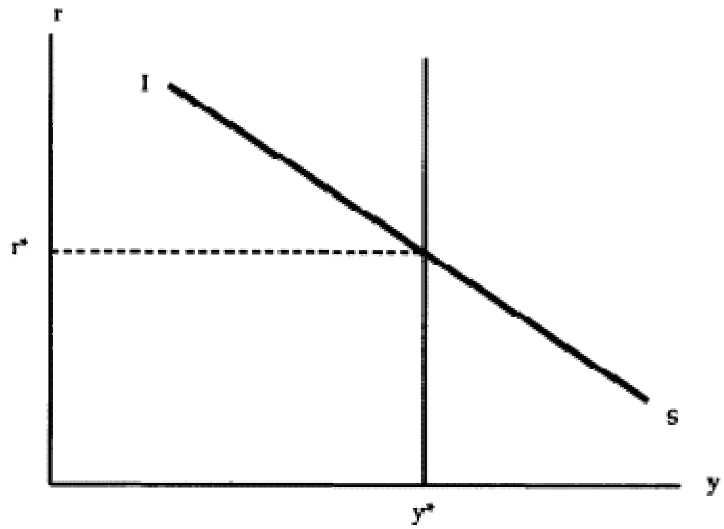

Figure 2.1
The neutral real interest rate

Second, the neutral real interest rate is not a fixed number. It depends, among other things, on fiscal policy and the exchange rate; and it is sensitive to other permanent (though not temporary) IS shocks. I use the steady-state IS curve to define the neutral real interest rate precisely to filter out transitory fluctuations in demand and focus on longer-run factors. But durable IS shocks do change the neutral rate.

Third, and implicit in what I just said, the neutral real rate of interest is difficult to estimate and impossible to know with precision. It is therefore most usefully thought of as a concept rather than as a number, as a way of thinking about monetary policy rather than as the basis for a mechanical rule.

But it is an operational concept. There are two main ways to get an estimate. One is to solve a complete macroeconometric explicitly for its neutral rate. Different econometric models will, of course, produce different numerical estimates; so alternative models should be examined. Bomfim (1997) has used the MPS model, the model formerly used at the Federal Reserve Board, to compute a quarterly time series on the neutral real federal funds rate based on a definition similar to mine. The key difference is that Bomfim uses the equation residuals to estimate the random shocks buffeting the economy and allows them to move the neutral rate up or down, whereas my definition sets the shocks to zero. The result is a time series that is quite volatile from quarter to quarter. According to his estimates, the neutral real funds rate has averaged about 2.8% during the 1990s.[27] It was typically much higher during the 1980s.

The other method is to compute the average *ex post* real rate over a long historical period—the idea being that lags work themselves out, transitory phenomena fade, and random shocks average to zero over long periods of time. Care must be taken, however, to avoid short periods that might be unrepresentative—as during the 1970s, when real interest rates were often negative, or the 1980s, when they were extraordinarily high. I prefer to use 30-50 years in computing such historical averages. This method tends to produce estimates of the neutral real federal funds rate in the 1.75-2.25% range, depending on the precise period chosen and the measure of inflation used to convert the nominal rate into a real rate.

My suggestion, then, is that central banks estimate the neutral real interest rate on a regular basis (a range may make more sense than a point estimate), and use that estimate as the "zero point" on their monetary policy scales. Any higher interest rate constitutes "tight money"; any lower rate constitutes "easy money." Neutrality is the only viable policy setting for the long run.

To tie these ideas to real events, consider Federal Reserve policy since the 1990-1991 recession. Gradually and, some might say, grudgingly, the Fed lowered the federal funds rate to 3%—which was about zero in real terms—in a lengthy series of steps culminating in the fall of 1992. As zero is well below the neutral rate by anyone's reckoning, monetary policy was clearly very stimulative. With a lag, the U.S. economy responded. Then, in February 1994, the Fed started moving the funds rate back toward neutral—explicitly calling attention to that concept as part of its justification. The nominal funds rate eventually peaked at 6%—which translated into a real rate slightly above 3%—in February 1995. That would be on the "tight" side of neutral by most—though not necessarily all—estimates. And the Fed held that rate until July 1995, when it began a three-step easing that brought Federal funds down to 5.25% on the last day of January 1996. With inflation running in the 2.5%-3% range, that meant a real rate between 2.25% and 2.75%— which was probably either neutral or just slightly on the tight side. Chairman Alan Greenspan explicitly acknowledged this in Congressional testimony in February 1997, when he said: "The real funds rate might be at a level that will promote continued non-inflationary growth."[28] But a month later he hedged his bet by nudging the Federal funds rate up another 25 basis points, where it remained as this book went to press.

4 The Rules versus Discretion Debate: Then and Now

Now that we have settled on a monetary policy instrument—the estimated real short-term rate of interest, it is time to face up to the really big question: Should the central bank use that instrument actively to try to stabilize the macroeconomy? Or should it rely passively on a rule?

Academic economists have long argued about whether central bankers practicing discretionary monetary policy should be replaced by a computer programmed to follow a mechanical rule. To

[27] Bomfim's (1997) series is nominal and ends in 1994:3. In computing the real rate, I used the four-quarter trailing rate of change of the deflator for personal consumption expenditures.
[28] Testimony of Alan Greenspan to the Senate Banking Committee, February 26, 1997.

my knowledge, practical central bankers have not joined this debate—presumably because they think they know the answer. As a former central banker, I naturally take the side of the establishment. But it is documented that I held this view even before I was admitted to the temple and learned the secret handshake.[29]

Before proceeding further, I must clarify what I mean by a *monetary policy rule,* to at least make clear what we are arguing about.

What Is a Rule?

The Tinbergen-Theil program that was discussed in the previous lecture will, if carried out, lead to a policy *reaction function* relating the central bank's instrument to a variety of independent variables—most prominently, the deviations of target variables from their desired levels. For example, the equation might have the overnight bank rate on the left and such items as inflation, unemployment, and the exchange rate or current account deficit on the right.

That is *not* what I mean by a rule. Rather, such an equation is, to me, a mathematical—and somewhat allegorical—representation of discretionary policy. It is the way an economist theorizing in the Tinbergen-Theil tradition imagines monetary policy to be made. To qualify as a rule, in my parlance, the "equation" for monetary policy must be simple and non—reactive, or nearly so. Friedman's famous k-percent rule is the best-known example, although holding money growth to a constant rate is no mean trick in the short run. Pegging the exchange rate is another realistic example. Holding the real short-term interest rate at its neutral level, no matter what, would be a third.

There is, however, another case that has gained prominence in the theoretical literature: assigning the central bank a rule based on *outcomes,* rather than one (like Friedman's) based on *instruments.* The two most obvious such rules are targeting inflation and targeting nominal GDP growth.

There is indeed an intellectual case for a rule of this sort. In fact, such rules come fairly close to—and in some cases duplicate—the legal mandates of central banks. The problem is that they are not really rules at all, but rather, objectives that may require a great deal of discretion to achieve. A government that wants to, say, stabilize the inflation rate at 2% cannot replace its central bank by a computer and throw away the key. Reaching that target and staying there is sure to require human judgment and adaptation to changing circumstances—to wit, discretion. The harsh but simple fact is that no central bank directly controls inflation, unemployment, or nominal GDP—much as economic theorists would like to pretend otherwise.

So, to me, the operational question is: Would it be better to replace central bank discretion with a simple rule based on instruments the bank can actually control, not on outcomes that it cannot control? Two very different lines of reasoning have been used to answer this question in the affirmative.

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[29] See, for example, Blinder (1987).

The Old Debate and the New Debate

The old-fashioned approach is intimately linked to the name Milton Friedman. Friedman and others argued that the automatic servo-mechanism of an unregulated economy would produce tolerably good, though certainly not perfect, results. While activist stabilization policy might be able to improve upon these results in principle, they doubt it will prove efficacious in practice because policymakers lack the knowledge, competence, and perhaps even the fortitude necessary to carry out the task. Faced with a choice between an imperfect economy and an imperfect government, Friedman and his followers opt without hesitation for the former. They share Lord Acton's concerns about power more than Lord Keynes's concerns about unemployment.

The arguments on each side of this old debate have been hashed over numerous times, so I will not repeat them here. Suffice it to say that, while I find the Friedmanite arguments for rules less than persuasive, they cannot be summarily dismissed. Our knowledge is indeed not quite up to snuff, and many monetary authorities have failed to acquit themselves with distinction. In all honesty, we must admit that there is at least an outside chance that Friedman could be right. However, I mention this older debate not to take sides but rather to contrast it with the newer version of the rules-versus-discretion debate.

The new arguments for rules take an entirely different tack. They are based neither on the ignorance nor the knavery of public officials and, in fact, assume that everyone knows how the economy operates—even the government! Moreover, the government's objectives are assumed to coincide with the people's objectives, and everyone has rational expectations. Despite these seemingly ideal circumstances, modern critics argue that a central bank left with discretion will err systematically in the direction of excessive inflation. To remedy this distortion, they advocate a fixed rule.

Kydland and Prescott (1977) initiated this new round of discourse by observing that the expectational Phillips curve poses a temptation to the monetary authorities. Specifically, by stimulating aggregate demand and surprising the private sector with unanticipated inflation, the central bank can reduce unemployment temporarily. Lower unemployment is prized by both the public and the central bank. The problem is that you can go to this well only so often and, under rational expectations, not very often at all.

If expectations are rational, people understand the central bank's behavior pattern, and monetary policy cannot produce systematic gaps between actual and expected inflation. So a central bank that regularly reaches for short-term gains will, on average, produce more inflation but no more employment than a central bank that is more resolute. But any central bank that makes monetary policy on a period-by-period discretionary basis will constantly face, and presumably succumb to, the temptation to reach for short-term gains. Kydland and Prescott dubbed this (an example of) the problem of *time inconsistency* and suggested that the way to solve it is to tie the central bankers' hands with a rule.

Barro and Gordon (1983a, 1983b) and Barro (1986) clarified this message and extended it in a variety of ways, noting among other things that the rule could be reactive and exploring the role of reputation as a way to produce less inflationary policies in repeated games. Their analyses spawned a small growth industry that spins theories of central bank behavior and offers remedies for the alleged inflationary bias of discretionary monetary policy. As an academic, I found this analysis unpersuasive. And what I learned as a central banker strongly reinforced this view. Let me explain why.

Three Major Objections

First, a historic point is worth making. With some variations in timing, the period from the mid-1960s until about 1980 was one of accelerating inflation in the industrial countries. Barro and Gordon ignored the obvious *practical* explanations for the observed upsurge in inflation—the Vietnam War, the end of the Bretton-Woods system, two OPEC shocks, and so on—and sought instead a *theoretical* explanation for what they believed to be a systematic inflationary bias in the behavior of central banks.[30] They found it in Kydland and Prescott's analysis.

But that was then and this is now. Recent history has not been kind to the view that central banks have an inflationary bias. In fact, the history of much of the industrial world since roughly 1980 has been one of disinflation—sometimes sharp disinflation, and sometimes at high social cost. Furthermore, the monetary authorities of many countries, especially in Europe, have displayed a willingness to maintain their tough anti-inflation stances to this very day, despite low inflation and persistently high unemployment. Whether or not you applaud these policies, they hardly look like grabbing for short-term employment gains at the expense of inflation.

How are we to reconcile the disinflation history of 19801997 with a theory that says that central banks systematically produce too much inflation? My answer is simple: We cannot. Nor can we dismiss the 1980-1997 period as a brief interlude of history, insufficiently long to belie the Barro-Gordon analysis, for the 1965-1980 period they used as "evidence" of inflationary bias was even shorter. Furthermore, few theorists seem to have noticed the following embarrassing fact: If the key parameters of the model are constant, the theory predicts stable inflation that is too high, not accelerating inflation. So it doesn't even explain the history of 1965-1980. The real question about that period is why inflation rose in so many countries. What changed?

I am tempted to conclude that Barro and Gordon and their followers were theorizing—and incorrectly at that—about the last war just as real-world central bankers were fighting the next one. In addition, it is worth noting that the real-world cure to the alleged "inflation bias" problem did not come from adopting rigid precommitment ("rules") or other institutional changes,[31] as Kydland-Prescott and Barro-Gordon suggested. It came from determined but discretionary application of tight money. Rather than seeking short-term gains, central banks paid the price to disinflate. As in the Nike commercial, they just did it.

My second objection is simple and practical: Most of the literature presumes that the central bank controls either the inflation rate or the unemployment rate perfectly on a period-by-period

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[30] Of course, some might interpret the fact that central banks allowed these shocks to pass through into higher inflation as evidence for inflationary bias.

[31] In the case of some European countries, it can be argued that the European Exchange Rate Mechanism (ERM) was such an institutional change; it effectively tied the country's monetary policy to that of Germany. But, for example, the United Kingdom and Italy broke out of the ERM, but still brought inflation down.

basis. Obviously, this is not so in reality. Now, a theorist may argue that this is an inessential point; after all, no theory is meant to be literally true. But I think that retort dismisses the objection too cavalierly. When the literature comes to discussing solutions to the inflationary-bias problem, as I will shortly, the arguments for simple rules based on outcomes (like "keep inflation at zero") or for certain incentive-based contracts seem to hinge sensitively on the notion that either the central bank controls inflation perfectly or that shocks are perfectly verifiable *ex post.* Trust me; the real world is not that simple. When the inflation rate changes, the public cannot be sure that the central bank did it. Come to think of it, neither can the bank!

My third objection appears to be a narrow technical detail, but it is not. The literature derived from Barro and Gordon (1983a) posits a loss function in inflation and unemployment that looks something like the following:

$$L = (u_t - ku^*)^2 + \alpha\pi^2_t,$$

where π_t is the inflation rate, *u* is the unemployment rate, *u** is the natural rate, α is a "taste" (or inflation-aversion) parameter, and *k* is a constant less than one indicating that the optimal unemployment rate is below the natural rate.[32] This last parameter turns out to be essential to the argument for inflationary bias. In fact, the inflationary bias of discretionary policy disappears in most models if $k = 1$.

I can assure you that it would not surprise my central banker friends to learn that economic theories that model them as seeking to drive unemployment below the natural rate imply that their policies are too inflationary. They would no doubt reply, "Of course that would be inflationary. That's why we don't do it." That reply points to a disarmingly simple solution to the Kydland-Prescott problem: *Direct* the central bank to aim for *u** rather than *ku**. That is exactly what I felt duty-bound to do while I was Vice Chairman of the Fed. And my attitude was hardly unique on the FOMC, where members were always concerned about the potential inflationary consequences of pushing unemployment below the natural rate.

Three Proposed Solutions

Let me now examine three proposed "solutions" to the inflationary-bias problem found in the theoretical literature. My purpose in each case is to compare theory to reality.

1. Reputation: The first solution hinges on notions of reputation—a concept, that is near and dear to the hearts of real central bankers. Here theorists have been barking up the right tree. Nonetheless, theoretical models of reputation have some peculiar features.

Consider, for example, Barro's (1986) model, in which the central banker is either a "tough guy," who will always opt for low inflation, or a "wet," who is willing to deviate in order to boost employment. The public does not know which kind of central banker it has, and is therefore forced into statistical inference. If the central bank keeps inflation low, its reputation—technically, the subjective probability that it is tough—will rise. This part rings true. For

[32] To be clear, *ku** is the optimal unemployment rate *if there were no worry about inflation.* So it is reasonable to assume $k < 1$ in the loss function.

example, the Federal Reserve probably had relatively little anti-inflation credibility in the late 1970s but has quite a lot now. In Europe, I believe the monetary authorities of both the United Kingdom and France have built up substantial anti-inflation capital during the 1990s.

But in the model, as soon as the bank allows high inflation, even once, the public concludes—with certainty—that it is a hopeless "wet." This is the feature of the model that strikes me as eccentric, if not downright silly. In reality, there are many types of central banker, not just two, and random shocks cloud the mapping from outcomes back to types. For these and other reasons, reputation is not like pregnancy: You *can* have either a little or a lot. For example, the Bundesbank's entire reputation as an enemy of inflation did not collapse when German inflation rose from about zero in 1986 to about 4% in 1992. Nor should it have.

In central banking circles, it is viewed as obvious that the accumulation and destruction of reputational capital more closely resembles adaptive than rational expectations—it lags behind reality. Here, I think, the central bankers are closer to the truth than the economic theorists.

2. Principal-agent contracts: A second proposed cure for the alleged inflationary bias of monetary policy that has attracted the recent attention of theorists is drawing up a contract between the central bank as agent and the political authorities (which I shall parochially call "Congress") as principal. The genesis of the idea is simple. The Kydland-Prescott analysis suggests that the incentives of decisionmakers are distorted toward excessive inflation. Say the word "distortion" and economists reflexively think of taxes and subsidies. So Walsh (1995) and Persson and Tabellini (1993) have proposed making the central banker's salary decline in proportion to inflation.[33] They show that this particular incentive scheme induces the central banker to behave optimally in the context of a model like that of Barro and Gordon (1983a).

What's wrong with this idea? Well, to start with, a small decrease in salary is probably not much of a motivator for central bankers who are already voluntarily giving up a large portion of their potential earnings to do public service. Let me put it bluntly and personally. When I was at the Fed, I had (more or less) a Walsh-type contract: Since my nominal salary was fixed by Congress and extremely unlikely to be raised, I suffered a 1% real wage loss for each point of U.S. inflation. But that meagre $1231 never once entered my thinking about monetary policy. It was dwarfed by my other financial losses. And I was just a fugitive from academia! Imagine the financial losses of a banker or successful business person.

Second, we must face up to the embarrassing fact that virtually no central bank explicitly ties its salaries to economic performance—not even New Zealand, where there really is a formal contract between the governor of the Reserve Bank and the minister of finance. The governor may be dismissed (and thus suffer a huge pay increase!) if inflation comes in too high. But he does not have his salary docked.

Third, and finally, there is a severe problem with the party on the other side of the contract.[34] In practice, "the public" cannot serve as the principal in the contemplated contract, so Congress

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[33] Actually, many people had proposed such a scheme before. Walsh and Persson and Tabellini proved its optimality in formal models.
[34] This problem has also been pointed out by McCallum (1996).

must play this role as surrogate. But Congress is really an *agent,* not a principal. And members of Congress—who must stand for reelection—face even stronger incentives to reach for short-term gains than do central bankers. So why would Congress propose a contract with the central bank that would eliminate the inflationary bias? And, more important, why would it want to enforce such a contract if the central bank deviated and thereby caused a little boom?

Critics of government everywhere complain that elected officials focus myopically on the next election rather than on the best long-run interests of the nation. Indeed, that is probably the principal rationale for making the central bank independent from politics, as I will note in the next lecture. The vision of highly disciplined and farsighted politicians curing the wayward ways of profligate and myopic central bankers seems a strange role reversal. In the real world, it is independent central bankers who prevent politicians from succumbing to the Kydland-Prescott temptation.

3. Conservative central bankers: This brings me to the third proposed theoretical solution to the conundrum posed by Barro and Gordon—the one with the most practical appeal. Rogoff (1985) cleverly suggested that, if there is an inflationary bias in monetary policy, the cure may lie in the appointment of more "conservative" central bankers. Now that really does have the ring of truth! Indeed, in the real world the noun "central banker" practically cries out for the adjective "conservative."

To Rogoff, conservatism has a very specific meaning. In the Barro-Gordon model, the taste parameter α, which indicates the relative disutilities of inflation and unemploy ment, is presumed to be common to the central bank and the public. Rogoff suggested that politicians should deliberately select central bankers who are *more inflation averse* than society as a whole. That way, one bias (the unrepresentative preferences of the central bank) can cancel out the other (dynamic inconsistency).

Rogoff's model is a splendid illustration of the humorous definition of an economist as someone who sees that something works in practice and asks whether it can also work in theory. Is there any doubt that central banks in general and successful inflation-fighting central banks in particular have been dominated by quite conservative people? Rogoff's model argues that this common practice is wise. He may be right. Nonetheless, a few points about his proposed solution are worth making.

First, the enhanced vigilance against inflation produced by conservative central bankers comes at a cost: Real output and employment are more variable than in the dynamically inconsistent solution. That is fine because it presumably moves society closer to the optimum. My point is just that the gains on the inflation front come at some cost. Appointing conservatives to the central bank board does not buy society a free lunch.[35]

Second, you can have too much of a good thing. In Rogoff's model—and, I believe, in reality—it is possible to appoint a central banker who is too conservative, that is, whose value of the

[35] This is a statement about the theory. The data on advanced countries suggest that more anti-inflation central banks do better than this. Blinder (1995), for example, found no correlation between an index of central bank independence and the variance of real GDP growth. See Eijffinger and De Haan (1996) for a comprehensive survey.

parameter a is so high that he or she does not deliver the combination of inflation and output variability that society really wants. Specifically, such a central bank will fight inflation too vigorously and be insufficiently mindful of the short-run employment costs. This too rings true, though I will refrain from naming names. It suggests that there is an optimal type of person best suited to a central bank board.

Third, Lohmann (1992) suggested an interesting amendment to the Rogoff approach which improves upon the solution—but one which must be handled with care. There may be times when it is optimal for the government to overrule the decision of the conservative central banker—for example, following a large supply shock. Lohmann suggests that such actions should be allowed, but only if the government pays a cost. In reality, the cost might be, e.g., the political heat the minister of finance would take if he or she overruled an important decision of the central bank governor. For example, the governor might resign in a huff.

Lohmann's idea is correct in both economic theory and political theory. In a democracy there should, after all, be some checks on the behavior of an over-zealous central bank. But its practical application is tricky, to say the least. No central bank can claim to be independent if its monetary policy decisions are routinely reversed. This remedy must be reserved for truly extraordinary circumstances. So any real-world government that adopts the Lohmann amendment must ensure that politicians overrule the central bank very rarely—for example, by making central bankers removable only for gross negligence. In the United States, Federal Reserve governors are removable by the president only for cause, and an act of Congress can overrule a Federal Reserve decision. But these are grave steps that have never been taken. In practice, Fed decisions are final.

The Bottom Line?

So where does this extended discussion of rules versus discretion leave us in the real, as opposed to the theoretical, world?

While Kydland and Prescott's insight points to a genuine difficulty for monetary policy, and some of the subsequent literature has been enlightening, there is less there than meets the eye. If there is strong agreement on both the positive aspects of a time-inconsistency problem (e.g., the Phillips curve) and its normative aspects (e.g., the social welfare function), as Barro and Gordon assume for the inflation problem, societies should have little difficulty in "solving" it, albeit imperfectly. For example, I just suggested one simple solution: *directing* the central bank to behave as if it prefers u^* rather than ku^*.

In fact, nations and households seem to find simple, practical ways to cope with a wide variety of potential dynamic inconsistencies—ways that bear little resemblance to the solutions suggested by theorists. Some common examples are dealing with flood plains, avoiding capital levies, punishing your children when they misbehave, and giving final examinations in courses. In each case, governments, parents, or teachers cope with a potential time-inconsistency problem, by creating—and then usually following—norms of behavior, by building reputations, and by remembering that there are many tomorrows. Rarely does society solve a time-inconsistency problem by rigid precommitment or by creating incentive-compatible compensation schemes for decisionmakers. Enlightened discretion is the rule.

Similarly, the revealed preferences of many democratic societies are to deal with the problem of dynamic inconsistency in monetary policy by legislating a long-term goal for the central bank (e.g., price stability), giving discretion to nonpolitical central bankers who have long time horizons and an aversion to inflation, and then hoping for the best. This is not obviously a bad solution.

5 Conclusion

The overall subject of these lectures is the interaction between academic theories of monetary policy and actual central bank practice. This lecture deals with three issues with very different resolutions.

In the case of choosing between interest rates and monetary aggregates as the policy instrument, the symbiosis was extremely strong. Academic research, beginning with Poole (1970), offered up sound and usable advice; and practical central bankers took it, to the benefit of all. Life imitated art.

In the case of the modern incarnation of the rules versus discretion debate, based on time inconsistency, I have argued that things are starkly different. In my view, the academic literature has focused on either the wrong problem or a nonproblem and has proposed a variety of solutions (excluding Rogoff's conservative central bankers) that make little sense in the real world. Unsurprisingly, they have had little influence on central banking practice. Here art would be well advised to imitate life a bit more.

The third issue—the choice of the zero point to define "neutral" monetary policy—is still unresolved. I have suggested using an estimated *neutral* real rate of interest, defined as the real short rate that is consistent with constant inflation, as the dividing line between "tight" and "loose" monetary policy. Neither the scholarly nor the practical jury has yet had enough time to consider and rule on this proposal. But I believe in market tests and am willing to wait for the verdict.

3 Central Bank Independence

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