

Central Banking in Theory and Practice

Lecture I: Targets, Instruments, and Stabilization

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1. Introduction

I realize that these are the Marshall lectures, not the Ricardo lectures. But please pardon a momentary digression on comparative advantage, for I have long believed that the 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 have long harbored doubts about my economist friends who mow their own lawns rather than hiring a gardener. While they rationalize their eccentric behavior by claiming that they actually enjoy cutting grass, such a claim is suspect on its face. More to the point, a true believer in comparative advantage should be constitutionally incapable of enjoying activities that run so deeply against the Ricardian grain.

Being a devotee of comparative advantage, once I agreed to give the Marshall lectures, the topic virtually chose itself. Greater economic theorists and more skilled econometricians than I have delivered these lectures in the past and doubtless will in the future. But there must be relatively few people on earth who have been so thoroughly steeped in the academic literature on monetary policy and then found themselves actually living in the world they used to theorize about. Therein, I presume, lies my comparative advantage. So that is the topic of these two lectures: the theory and practice of central banking.

To keep things manageable, I have pared the topic beyond what 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 lefthand than righthand variables.

Somewhat arbitrarily, I have divided this subject matter into two parts: old-fashioned and new-fangled. Today's lecture covers the old-fashioned parts. What I have to say today probably could have been written 15 or 20 years ago, if I had had the relevant practical experience then, which I did not. In particular, today's lecture mostly ignores the expectational and game theoretic issues that have been central to much of the modern academic literature on central banking. I do this not out of a fondness for sounding archaic, but to remind this sophisticated audience that some of the lessons of the past are still important and very much central to monetary policymaking in the practical world. Tomorrow, I will turn to some of the topics that have occupied the attention of modern academic theorists of central banking--like credibility, dynamic consistency, and central bank independence.

Let me give away the main theme right off the bat. It comes in three parts. First, central banking looks rather different in practice than it does in theory. Second, both theory and practice could benefit from greater contact with and deeper understanding

of the other. Neither of these will surprise you, but the third one might: It is in the old-fashioned realm, I believe, that practical central bankers have the most to learn from the "theorists,"¹ while theorists could and should pay more attention to practitioners in the new-fangled realm.

2. Targets and Instruments: The Rudiments

In their role as monetary policymakers, central banks 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,² the central bank is forced to strike a balance among competing objectives, that is, to face up to various tradeoffs. Unless your education in economics is very thin, 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:

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

and in reduced form as:

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

Here y is the vector of endogenous variables (a few of which are

¹I mean, of course, theorists armed with appropriate econometric evidence.

²One example is a central bank that must fix the exchange rate. A number of people have suggested that central banks should pursue zero inflation to the exclusion of all other objectives.

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 $F(\cdot)$ is linear and the policymaker's objective function,

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

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

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

All very simple.

What's wrong with this simple framework? Both nothing and everything. Starting with "nothing," I do believe that--once you have added a host of complications, several of which I will speak about today--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, central bankers do think about policy this way.

But, as is well-known, there are many complications. Let me just list a few, some of which I will dwell upon at length in the balance of today's lecture:

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 $G(\cdot)$ 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 by the formalism simply by appending further equations for lagged variables (cf. Chow (1975)). However, in practice it creates serious difficulties that bedevil policymakers.

3. Need for forecasts: With 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. Needless to say, 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 x s and y s 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 the central bank can directly control, say, nominal GDP, the

inflation rate, or the unemployment rate on a period-by-period basis. Believe me, we cannot.

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 $W(.)$ 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, to which I will return tomorrow.

Summing up, if I wanted to be curmudgeonly, I could summarize the problems with applying the Tinbergen-Theil program as follows: We do not know the model (1), and we do not know the objective function (3), so we cannot compute the optimal policy rule (4). To some critics of "impractical" or "theoretical" economics, including some central bankers, this criticism is a show-stopper. But, speaking now as a practical 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 which 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 virtually none of us follows this strategy because we know it will produce poor results.³ 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

³In 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)).

that is not a bad place to start. In fact, no central bank that I know of, and certainly not the Federal Reserve, is literally 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 macroeconomic 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. 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 econometric modelling? 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 nihilism. It is foolish to make the best the enemy of the moderately useful.

Indeed, I would go further. I don't see that we 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. (And who said the theory always gets the sign right, anyway?) We 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, do not hesitate over this choice. But I fear there may be too much uncle-asking in government circles in general, and in central banking circles in particular.

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 these 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

⁴In Knight's 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.

⁵I 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. (Almost all hand-adjust both equations and exogenous variables.) But other forecasting techniques--including pure judgment--also produce modest records. So perhaps big models should not be dismissed so readily. Furthermore, econometric models are an essential tool in enforcing the consistency you need to forecast the hundreds of variables in a typical macro model.

particular reason to think that the objective function is quadratic. Both 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, not terribly important in practice--except on those rare occasions when large changes in policy are being considered.

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, and perhaps for good reason.

There is, however, one oft-forgotten principle that I suspect practical central bankers can--and in a rough way do--rely upon. Many years ago, William Brainard (1967) demonstrated

⁶Samuelson (1970) proves an analogous proposition in the context of portfolio theory.

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 Tvinger-Theil way and then do less.

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

$$(2') \quad y = Gx + Z + e,$$

and suppose that G and Z are independent random variables with means g and z respectively, and 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. If G is nonrandom, the optimal policy adjustment is certainty equivalence:

$$x = (y^* - z)/g,$$

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

$$x = (y^* - z)/(g + s^2/g),$$

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

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

support.⁸ And I certainly hope it is, for I can tell you that it is never far from my mind when I sit in my 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 a variety of non-nested alternatives is another matter entirely. While there is some formal literature on this problem,⁹ I think it 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 is relatively simple: Use a wide variety of models and don't ever trust any one of them too much. So, for example, when the Federal Reserve staff explores policy alternatives, I always insist 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 have developed for this purpose. My usual procedure is 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

⁸With 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.

⁹One strand, derived from the optimal control literature, deals with choosing among rival models. Another strand, due to Hendry and his collaborators, focusses on encompassing tests. See, for example, Hendry and Mizon (1993).

of a dynamic multiplier path. I would be very grateful if some brilliant young econometric theorist would prove that this constitutes optimal information processing!

4. Lags in Monetary Policy

It is by now 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. 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.¹⁰ 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 x_T conditional on a past history. (The postscript denotes calendar time and the prescript denotes the date at which the expectation is taken.) Then, given your solution for x_T , which most likely depends inter alia on x_{T-1} , you solve a two-period problem for x_T and x_{T-1} jointly. Proceeding

¹⁰Kydland 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 use the term "dynamic programming" generically, intending to include such reactions of expectations.

similarly, by a process of backward induction you derive an entire solution path:

$$x_t, {}_t x_{t+1}, {}_t x_{t+2}, \dots, {}_t x_T.$$

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 a 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 decides to begin a cycle of tightening, 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 its mind about its earlier tentative decision ${}_t x_{t+1}$. That is fine. In fact, given the information then available, it will want to plan an entirely new path:

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

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,

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. Some people claim 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. But that is a grave misunderstanding of the way dynamic-programming teaches us to think. It is absolutely correct that flexibility should be maintained and 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.

Let me now make this abstract discussion more concrete. Central banks, both in the U.S. 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--be it tightening or loosening, thereby causing overshoots in both directions.¹¹ 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

¹¹See, for example, Meltzer (1991).

exists, may be due to following a strategy I call "looking out the window."

A central bank following the "look out the window" strategy proceeds as follows. Suppose, just for concreteness, that it is in the process of tightening. At each decisionmaking juncture, it 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. Lately, for example, the notion that it is wise to pursue a strategy of "preemptive strikes" against inflation seems to have caught on among central banks. The Federal Reserve, I am proud to say, seems to have started this trend,¹² followed by, e.g., the Reserve Bank of Australia and the Bank of England.¹³

Such a strategy implies a certain amount of confidence in both your forecast and your model of how monetary policy affects the economy. But not too much. Remember the flexibility principle of dynamic programming and the Brainard conservatism principle. Taken together, they lead to the following sort of strategy:¹⁴

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.

¹²This is not self-praise. I was not on the Federal Open Market Committee when it began to tighten monetary policy in February 1994.

¹³The RBA began tightening in August 1994 and the BOE a month later. Neither economy had yet reached full capacity, nor was either yet experiencing an upsurge of inflation.

¹⁴This strategy has a temporal aspect not found in Brainard's analysis, and hence may embody a big 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.

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

Two final points about preemptive strikes are worth making. First, a successful stabilization policy based on preemptive strikes will appear to be misguided and may leave the central bank open to vociferous criticism. The reason is simple. If the monetary authority tightens so early that inflation doesn't rise, 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 unemployment.

Second, the logic behind the preemptive strike strategy is symmetrical. Precisely the same reasoning that says a central bank should 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, after the Fed had raised short-term interest rates 300 basis points within 12 months, 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."¹⁵ In fact, the Fed did

¹⁵From testimony given to committees of both the House and Senate on February 22 and 23, 1995, printed in Federal Reserve

precisely that back in the summer of 1989, when it started cutting interest rates while inflation was still rising and unemployment was below its natural rate.

The preemptive strike strategy applies more to fighting inflation than to fighting unemployment only if:

1. the short-run Phillips curve is distinctly nonlinear, so that inflation rises much more in response to low unemployment than it falls in response to high unemployment. The evidence is decidedly against this hypothesis for the United States.

2. lags in monetary policy are longer for inflation fighting than for unemployment fighting, which appears to be true.

3. the central bank's loss function is notably asymmetric.

5. The "Debate" over "Fine-Tuning"

Sometime in the 1970s, or perhaps even in the late 1960s, it became the height of wisdom to declare that something called "fine tuning" is impossible because our knowledge base is insufficient and our instruments are not that finely calibrated. I agree with these criticisms wholeheartedly. In fact, so far as I can tell, everybody does. Indeed, I am not sure that anyone ever took the other side--which makes this a curious debate, rather like defending motherhood. The only trouble is that I am not convinced that the debate--or nondebate--has any operational meaning. It could be that the entire concept of fine-tuning is epistemologically empty.

Bulletin, April 1995, p. 348.

Consider some possible meanings of two common statements about fine tuning--one positive, the other normative:

I. Fine tuning is impossible.

II. No central bank should try to fine-tune its economy.

One possible meaning of statement I is that stabilization policy cannot entirely eliminate the variance of real output around trend, nor the variance of inflation around target (possibly zero), nor therefore any weighted average of the two. If that is the meaning of the phrase, it is of course indisputably true. But so what? Does it imply that central banks should therefore not try to reduce these variances?

That question brings up a possible, though extreme, interpretation of statement II: that it is unwise to attempt any stabilization policy at all. In other words, monetary policy should follow a nonreactive rule like Friedman's k-percent rule for money growth. But this definition seems to distinguish between some tuning and no tuning, not between fine tuning and coarse tuning.

There is indeed a bright line between attempting to stabilize the economy and abjuring the whole messy business. If this were the issue, I could understand the debate, bring both value judgments and technical knowledge to bear on it, and reach a conclusion--as I will in tomorrow's lecture. But once you have left the realm of nonreactive rules and opted for some tuning, I fail to see any bright line--and maybe not even a dim one--between coarse tuning, which is what we central bankers are

supposed to do, and fine tuning, which is what we are supposed to avoid. Don't you always do the best you can, mindful of a host of uncertainties?

Another possible interpretation of statement II is as an injunction to follow what I have called Brainard's conservatism principle: Estimate what you should do and then do less. If so, I have great sympathy. But I doubt very much that this is what the anti-fine-tuners have in mind, for the strategy appears to call for constant adjustments of policy, even small ones, as new information is received. This sounds a bit like fine, albeit cautious, tuning.

Another possibility is that policy changes should be infrequent; most of the time, monetary policy should be "on hold." Such behavior would resemble the (S,s) strategy of inventory management. Under an (S,s) inventory policy, a firm lets its inventory stock drift aimlessly so long as it remains below some upper limit S and above some lower limit s . But, if inventories get outside those bounds, it takes prompt action either to cut stocks down or build them up. The rationale for such behavior is that each "order" or "sale" entails a fixed cost, so that frequent, small changes are to be avoided. But what is the analogous fixed cost for monetary policy in a world in which markets change interest rates all the time, whether or not the central bank does anything?

Part of the general hostility toward fine-tuning is surely the notion that policymakers should not set their sights too high

and expect to iron every bump and wiggle out of the economy's growth and/or inflation path. Once again, I agree but wonder about the dictum's operational significance. And my brief practical experience as a central banker has only deepened my skepticism. Doesn't even a poor archer aim for the bull's eye, even though he does not expect to hit it?

To make the discussion concrete, consider the situation faced by the Federal Open Market Committee (FOMC) in recent months. Sometime in late 1994 or early 1995 (according to tastes), the U.S. economy reached a position which, if not ideal, was at least excellent: the lowest unemployment and highest capacity utilization in years plus the lowest inflation rate in a generation. So a central bank that eschewed fine tuning would certainly have been satisfied with the situation and not sought to twiddle the dials further. But what does that actually mean in practice? Hold the nominal federal funds rate constant even while inflation, long-term interest rates, stock market values, and the dollar's exchange rate moved? Or hold the real rate constant? Does either represent "constant monetary policy?" And should we, e.g., have ignored forecasts that a rise in inflation was likely under unchanged policy?

My point is that monetary policy makers must make some decision at each moment in time. Even doing nothing--whatever that means--is a decision. In the event, the FOMC raised the federal funds rate 75 basis points at our November 1994 meeting, held rates constant at the December meeting, raised rates by 50

basis points in February 1995, and then held rates steady again at the March meeting. Did this constitute fine tuning or not? What would we have done differently if we were more devoutly opposed to fine tuning? I must admit that I don't know.

6. The Choice of Monetary Instrument

I conclude today's lecture by taking up one final old-fashioned issue: the choice of monetary instrument. By labeling some variables as targets and others as instruments, as if that was their birthright, the Tinbergen-Theil approach elides one of the most enduring controversies in monetary policy.

In simple models, beginning with Poole (1970), the issue is often posed as choosing 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 U.S., the federal funds rate and bank reserves are probably the only viable options. But other variables like the M s become candidates if the control period is thought of as, say, a quarter.

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 technical feat is rarely carried out.¹⁶ And I am pretty sure that no central bank has ever picked 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.¹⁷ Since Poole's seminal paper, monetary theorists have devoted much attention to the question he posed, and have 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 this debate was 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 the conclusion that M-targeting strategies

¹⁶A few papers in this spirit are Tinsley and von zur Muehlen (1981), Brayton and Tinsley (1994), and Bryant, Hooper, and Mann (1993).

¹⁷Covariances and slopes of IS and LM curve also matter. I ignore them here.

are simply not viable. Some facts about the U.S. monetary aggregates illustrate just how strong this evidence is.

The cornerstone of monetarism must surely be the notion that money and nominal income are cointegrated, for without such a long-run relationship why would anyone care about the behavior of the Ms? Yet a series of cointegration tests for M1 and nominal GDP, using rolling samples which begin in 1948 and end at various dates, fail to reject the hypothesis of no cointegration as soon as the endpoint of the sample extends into the late 1970s. That is, 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 also disappears into a black hole in the 1990s.¹⁸ 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 facts like these, interest rate targeting won by default. I often put the issue this way: If you want the Fed to target the growth rate of M, you must first answer two questions: What definition of M? And how fast should it grow? In recent years, these questions have become show-stoppers because no one can provide coherent answers. So, in point of fact, there are

¹⁸This statement is actually too generous to monetarism since data limited to the 1948-1980 period fail to indicate cointegration. A cointegrating vector appears only when the sample is extended well into the 1980s, but then disappears as data from the 1990s are appended.

very few M advocates left in the United States. 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 deal with the broader rules-versus-discretion debate tomorrow.

Was the theoretical literature therefore useless to practitioners? Absolutely not. In fact, it is hard to think of an aspect of monetary policy in which theory and practice 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 disengagement was gradual. After a rather exciting experiment with monetarism between 1979 and 1982, the Fed began backing away from M targets in 1982. The target growth range for M1 was dropped in 1987, but growth targets for M3 and, especially, M2 retained a serious role in monetary policy formulation through 1992. Finally, in February 1993, Chairman Greenspan announced that the Fed was giving "less weight to monetary aggregates as guides to policy."¹⁹ As usual, however, laws lag far behind both academic knowledge and central bank practice. A 1978 law which is still on the books requires

¹⁹Statement 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.

the Federal Reserve to report its target ranges for money growth to Congress twice a year. We dutifully do this. But the relevance to policy eludes most of us.

7. In Conclusion

I reach the end of this lecture with a somewhat cheerful message, one which would have made Alfred Marshall happy. Working in their cloistered universities, Tinbergen, Theil, Brainard, and Poole all taught valuable abstract lessons which 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 econometric models and optimal control. None of these ideas provide pat answers or can be applied mechanically by central bankers. The world is much too complicated for that. So there is still as much art as science in central banking. Nonetheless, the science is still useful; at least I find it so.

As Marshall wrote: "Exact scientific reasoning will seldom bring us very far on the way to the conclusion for which we are seeking, yet it would be foolish to refuse to avail ourselves of its aid, so far as it will reach:--just as foolish as would be the opposite extreme of supposing that science alone can do all the work, and that nothing will remain to be done by practical instinct and trained common sense."²⁰

²⁰Principles of Economics, p. 779.

That's a nice phrase: trained common sense. Isn't developing trained common sense what the intersection of theory and practice should be all about?