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Could Learn from Academics--and Vice Versa**

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Distinguished Lecture on Economics in Government

What Central Bankers Could Learn from Academics—and Vice Versa

Alan S. Blinder

Academic economists have been popping in and out of high government posts for long enough, and in sufficient numbers, that it appears safe to assume that this lend-lease program has met the market test: it must be creating value added for both the government and the academics. What is the source and nature of this value added? Rather than treat this subject in the abstract, I thought I might best illuminate it by preparing a kind of “case study” of economics at the Federal Reserve and other central banks. Specifically, this lecture will focus on two questions: 1) Is the training we give and the research we do in the academy useful in actual monetary policymaking? 2) Could it be *more* useful if academics and/or central bankers would change their ways?

As you will see, my answer to both questions is “yes, but . . .” In the process of answering these questions, I will call attention to what I perceive as problems with both academic research and Federal Open Market Committee (FOMC) procedures.

My framework for exploring these issues is the old-fashioned Tinbergen-Theil targets-and-instruments approach, but augmented to include lags. I think of this as dynamic programming or optimal control.¹ This framework imagines that the central bank controls certain instruments and seeks to minimize the expected present discounted value of the social losses from unemployment and inflation (the “targets”). The loss function is assumed to be quadratic, and the model is assumed to be linear, so that certainty equivalence holds.

¹ The original sources are Tinbergen (1952) and Theil (1961). A good source for a more modern treatment is Chow (1975).

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The approach instructs monetary policymakers to specify: a) the goals of policy; b) the instruments; c) their model of the macroeconomy; and d) forecasts of exogenous variables. Now, a curmudgeon could object that the Tinbergen-Theil program cannot be implemented in practice because no real central banker knows either the objective function or the true model of the economy. Taken literally, this objection is true. But in my brief career as a central banker, I found the framework enormously useful nonetheless. Let's start with the targets.

The Goals of Monetary Policy

Most academic economists begin and end their formal thinking about the goals of monetary policy by positing a periodic loss function that weights the squared deviations of unemployment and inflation from their target values, like

$$L = (u - u^*)^2 + \alpha(\pi - \pi^*)^2.$$

In the academic literature, u^* , π^* , and α are taken to be known (though perhaps time-varying) parameters, and the analysis proceeds from there. But in the real world, choosing these numbers is an immensely important practical problem. The central bank's legal mandate typically provides only vague guidance, if that. In the case of the Federal Reserve, Congress has directed monetary policymakers to pursue "maximum employment" and "stable prices." But what do these terms mean?

The phrase "maximum employment" is conceptually awkward. In the presence of a price stability objective, it cannot possibly mean the largest number of jobs that the economy can generate. One reasonable interpretation would set u^* equal to the natural rate of unemployment, the only unemployment rate consistent with stable inflation, and interpret the goal as stabilizing unemployment around its natural rate.

The natural rate concept itself is of limited use in Europe, where few if any countries have reliable statistical Phillips curves. But it is quite operational in the United States, where a stable Phillips curve with the natural rate property fits the data amazingly well.² Indeed, my experience at the Federal Reserve led me to believe that many members of the Federal Open Market Committee (FOMC) interpret the "maximum employment" mandate in precisely this way. But the FOMC has never officially adopted the natural rate Phillips curve as part of its intellectual framework, much less agreed upon any numerical value for the natural rate.

"Stable prices" is, if anything, an even more slippery concept. Alan Greenspan has suggested that price stability obtains when household and business decision making ceases to take inflation into account, and I have offered a similar definition: prices are stable when ordinary people, in their ordinary course of business, stop

² See, for example, Gordon (1997) in the symposium in the Winter 1997 issue of this journal.

talking about inflation. But these verbal definitions hardly yield a numerical inflation target. Indeed, one might argue that the contemporary United States, with its 2.5–3.0 percent measured inflation rate, is already at functional price stability.

Another possible definition would set the target for *measured* inflation at the estimated “bias” in the CPI (Consumer Price Index) inflation rate, thereby defining the target as zero “true” inflation rather than zero “measured” inflation. With recent CPI inflation running in the 2.5–3.0 percent range and popular estimates of the bias in the 1.0–1.5 percent range (Shapiro and Wilcox, 1996)—with high-end estimates even larger (Nakamura, 1997)³—it is now critical for the FOMC to decide on a concrete definition of price stability.

Indeed, during my time on the committee, I viewed the lack of consensus on the ultimate targets for unemployment and inflation as a severe handicap to rational policymaking. How can you know what to do if you do not even know where you want to go? Here is one case, I believe, in which greater internalization of the Tinbergen-Theil way of thinking could improve policymaking.

In fact, I would take the point further. Policymaking in the FOMC tends to be far too situational. Consensus is reached on a meeting-by-meeting basis, based on painstaking analysis of the current macroeconomic situation and near-term outlook. But rarely is any attempt made to reach agreement on the basic conceptual framework for monetary policy—including the ultimate targets and the relative weights attached to each. I believe the discipline of academic-style (but certainly not terribly abstract) thinking could have a salutary impact in the practical arena. But resistance at the Fed is strong.

Having criticized FOMC procedures, let me now turn the tables and bring up an aspect of the objective function where I think the FOMC debate—while quite informal—may be running well ahead of academic thinking. Suppose inflation is above its ultimate target.⁴ One absolutely critical issue for practical central bankers is the speed of disinflation—the desired path toward the ultimate inflation objective.

The linear-quadratic framework offers a clear answer. As long as inflation is above its target value, society suffers a loss proportional to the squared difference between actual and target inflation. So the “tightness” of monetary policy should be apportioned to this gap.

But is this answer correct? Over the years, several members of the FOMC have suggested a different strategy, which has come to be called *opportunistic disinflation*, to distinguish it from the conventional *deliberate disinflation* strategy just outlined.⁵ The idea is this. Suppose inflation is running above its long-run target, but not very much above.⁶ Under certain circumstances, the optimal disinflation strategy is

³ However, the Bureau of Labor Statistics disputes these estimates and argues that the bias is much smaller.

⁴ The analysis applies equally well if inflation is below its long-run target. Just reverse all the signs.

⁵ For details on both the FOMC antecedents and the analysis, see Orphanides and Wilcox (1996).

⁶ If inflation is far above target, the opportunistic and deliberate strategies are nearly identical.

asymmetric in the following specific way: you guard vigorously against any rise in inflation, but wait patiently for the next favorable inflation shock to bring inflation down. The opportunistic strategy makes the time needed to approach the ultimate inflation target a random variable. When I was Vice Chairman of the Fed, I often put it this way: the United States is “one recession away” from price stability.

Opportunistic disinflation is optimal behavior if the objective function differs from the quadratic in a particular way. Specifically, suppose we generalize the previous linear quadratic equation to read

$$L = [(u - u^*)^2]^\beta + \alpha(\pi - \pi^*)^2,$$

and allow the parameter β to be less than unity. This means that small deviations of u from u^* are penalized more heavily than are small deviations of π from π^* . Specifically, when both unemployment and inflation are near their targets, the central bank will—at the margin—be more concerned with unemployment than with inflation.

This formulation looks unconventional and ad hoc. But academic macroeconomists tend to use quadratic loss functions for reasons of mathematical convenience, without thinking much about their substantive implications. The assumption is not innocuous.⁷ Might an exponent less than 2—meaning that the social costs of unemployment rise more slowly than the square of $u - u^*$ —make sense? The FOMC has debated this issue at length, though not in this formal way, and without, to my knowledge, reaching a conclusion. I believe that both practical central bankers and academics would benefit from more serious thinking about the functional form of the loss function.

If the social costs of inflation derive from triangles under money demand functions, which represent deadweight losses from the “inflation tax,” or if they stem from the distortions in capital taxation emphasized by Feldstein (1996),⁸ then a quadratic in $\pi - \pi^*$ may be a reasonable approximation. But Harberger triangles may not be the appropriate way to think about the losses from unemployment if, say, 2 percentage points of unemployment above the natural rate means that 2 percent of workers are fully unemployed, rather than all workers being 2 percent unemployed. In that case, losses that are linear in the unemployment rate may be a better approximation. But my purpose here is not to resolve the issue, just to broach it. It is worth studying and has implications that transcend the present context.

Another, and very different, rationale for opportunistic disinflation lies in the political economy of monetary policy. Even a rather independent central bank can

⁷ See Orphanides and Wilcox (1996), which deals with the special case $\beta = 1/2$. This makes the first term in the previous equation the absolute value of $u - u^*$.

⁸ These distortions arise from an unindexed tax system. For example, depreciation allowances defined in nominal terms are eroded by inflation. On the other hand, businesses can deduct nominal—not just real—interest expenses.

be vulnerable to political attack—which can ultimately threaten its independence—if its actions are seen as deliberately causing recessions. Sometimes this may be unavoidable—as, for example, in the Volcker disinflation. But when inflation is already low and stable, the central bank may prefer to wait patiently rather than shoulder the blame for engineering economic slack.

The Instruments of Monetary Policy

I will be much briefer regarding the instruments of monetary policy, an area marked by a long and intense controversy between advocates of monetary aggregates (M) and advocates of interest rates (r), because this controversy is now history.

Virtually all major central banks nowadays use the overnight interbank rate (in the United States, the federal funds rate) as their central policy instrument. The resolution of this long-running controversy illustrates the interaction between theory and practice at its best. Scholarly literature, beginning with Poole (1970), clarified the *theoretical* conditions under which an interest-rate policy is preferred to a money-supply policy. Briefly, that happens when shocks to money demand (“LM shocks”) dominate shocks to spending (“IS shocks”).⁹ These conditions then arose in practice, and one central bank after another abandoned M targets in favor of r targets. Would that things always worked out so nicely.

As a former central banker, I do, however, have one pet peeve to share with you. Theoretical models of monetary policy often treat either the unemployment rate or the inflation rate as the central bank’s policy instrument. To a theorist, it may seem innocuous to pretend that monetary policy can control either u or π *perfectly* on a *period-by-period* basis. But to a practical central banker, it seems downright silly, for it assumes away most of the uncertainties that define everyday life. When the Federal Reserve contemplates changing the federal funds rate, it faces at least four different elements of uncertainty about how the economy will react:

1) *Market reactions*: How will the proximate transmission variables (such as long-term interest rates, stock prices, exchange rates, and credit) react to changes in the funds rate?

2) *The IS curve*: How will real aggregate demand react to changes in these transmission variables?

3) *Okun’s Law*: How will the unemployment rate react to changes in real GDP?

4) *The Phillips curve*: How will inflation react to changes in unemployment?

From my office at the Federal Reserve, the first two sets of uncertainties—those that link the federal funds rate to aggregate demand—always looked *much* greater than the uncertainties surrounding Okun’s Law or the Phillips curve. If the FOMC

⁹ That is not the whole story. Even in Poole’s simple analysis, the slopes of the IS and LM curves and the covariance between the IS shock and the LM shock also matter. And more complicated models bring in yet more factors.

literally controlled either unemployment or inflation, monetary policy would have been far simpler. Theoretical models that said we had such control were of little interest.

Even from a purely theoretical perspective, the assumption can be quite misleading. Consider, for example, Barro's (1986) model of reputational equilibrium. The public is assumed to use observed inflation outcomes to judge whether its central bank is tough or soft—which is certainly reasonable. But in that model, a single observation of high inflation is enough to make the public conclude—with certainty!—that its central bank is soft. In a world with a myriad of shocks and pervasive uncertainties, such a deduction would be ludicrous; and intelligent people do not make it. For example, few people deduced that the Bundesbank had gone weak in the knees when it allowed German inflation to rise for a while after reunification.

The Model of the Macroeconomy

Obviously, no one knows the “true model” of the economy. The Fed's own model is not just a trade secret; in a very real sense, it does not even exist. The board's staff uses its own giant econometric model to derive policy multipliers, but not to generate a baseline forecast. The 12 Reserve Banks, whose presidents participate in every FOMC meeting, each have different analytical tools that they use in different ways. No effort is ever made to reach consensus on the model of the economy. That may be a strength rather than a weakness, given all the uncertainties. But I do not believe that the Fed or any other central bank comes close to the technological frontier in dealing with model uncertainty.

While at the Fed, I used to say that there are two basic ways to obtain quantitative information about the economy: you can study econometric evidence, or you can ask your uncle. To me, the choice was easy despite all the well-known pitfalls of time series econometrics. But I believe there is far too much uncle-asking in government circles in general and in central banking circles in particular. Academics are trained to be skeptics, and thus are naturally and appropriately wary of empirical evidence that may stand on shaky statistical ground. (And what finding based on nonexperimental data does not?) That is all well and good. But we should be careful not to give aid and comfort to the supporters of uncle-asking, which is really a subterfuge for escaping the discipline of the data and allowing your priors to run rampant.

But the main aspect of the macroeconomic model that I want to deal with here is the lags in monetary policy. These lags tend to be trivialized or ignored in academia; in theoretical expositions, they can be handled by a simple change of notation (Chow, 1975). But they pose a huge practical problem for policymakers. Failure to take proper account of lags is, I believe, one of the main sources of central bank error.

One reason is simple and could, perhaps, be helped by more applied research. All central bankers understand that there are long lags in monetary policy. But

when policy is being either tightened or eased, policymakers typically have no usable quantitative estimate of what are often called “pipeline effects,” that is, the lagged effects of previous monetary policy actions that have not yet shown through in the data. Deriving such a measure may seem a prosaic task.¹⁰ But any contribution academic economists could make toward developing an accepted methodology for estimating such pipeline effects would be extremely useful in the practical world.

The second problem with lags runs much deeper and is, at least in part, psychological. Put plainly, human beings have a hard time doing what *homo economicus* does so easily: waiting patiently for the lagged effects of past actions to be felt. I have often illustrated this problem with the parable of the thermostat. The following has probably happened to each of you; it has certainly happened to me. You check in to a hotel where you are unfamiliar with the room thermostat. The room is much too hot, so you turn down the thermostat and take a shower. Emerging 15 minutes later, you find the room still too hot. So you turn the thermostat down another notch, remove the wool blanket, and go to sleep. At about 3 a.m., you awake shivering in a room that is freezing cold.

The corresponding error in monetary policy leads to a strategy that I call “looking out the window.” At each decision point, the central bank takes the economy’s temperature and, if it is still too hot (or too cold), proceeds to tighten (or to ease) monetary policy another notch. With long lags, you can easily see how such myopic decision making can lead a central bank to overstay its policy stance, that is, to continue tightening or easing for too long.

One way to think about this problem is that practical central bankers have not internalized the dynamic programming way of thinking. It is not the mechanics that matter, and certainly it is not the computational techniques. But dynamic programming does teach us a systematic, step-by-step way of thinking about stabilizing a system subject to uncertainty and lags. First, you must plan an entire hypothetical path for your policy instrument, from now until the end of the planning horizon, even though you know you will activate only the first step of the plan. It is simply illogical to make your current decision in splendid isolation from what you expect to do in subsequent periods. Second, when next period actually comes, you must appraise the new information that has arrived and make an entirely new multi-period plan. If the surprises were trivial, that is, if the stochastic errors were approximately zero, step one of your new plan will mimic the hypothetical step two of your old plan. But if significant new information has arrived, the new plan will differ notably from the old one. Third, you must repeat this reappraisal process each and every period.

Many central bankers reflexively reject the dynamic programming way of thinking because they think it both foolish and dangerous to make a multiperiod plan when you do not know what lies ahead. On this view, it is folly even to *try* to decide

¹⁰ Twenty years ago, Stephen Goldfeld and I developed several such measures using the MPS model (Blinder and Goldfeld, 1976). Clearly, this activity did not become a growth industry!

today, however tentatively, what your future actions might be. Instead, maintaining “flexibility” is viewed as the height of wisdom. And getting “locked in” to a strategy you may regret is the cardinal sin that is to be avoided at all cost. I cannot tell you how many times, both at the Federal Reserve and at meetings with foreign central bankers, discussions of future policy were cut short with phrases like “let’s see what happens” or “we’ll have to wait until next month (or next meeting).”

Unfortunately, this bit of received central banking wisdom is not at all wise. In fact, it reflects a complete misunderstanding of the dynamic programming way of thinking. Dynamic programming is precisely a way to *maintain* flexibility and to *avoid* getting locked in to an inappropriate strategy. It calls for constant reappraisals and updates of the monetary policy plan as new information arrives. But it also calls for thinking ahead, *not* for myopic decision making.

Forecasting

In the Tinbergen-Theil framework, once you have targets, instruments and a model, you need forecasts. Forecasts can be generated without formal models of the economy, and frequently they are. I think it is safe to say that few, if any, central banks rely heavily on macroeconomic models to do forecasting—which may well be an appropriate division of labor.

I think it is also safe to say that relatively few academic economists are deeply involved in the forecasting business these days, having long ago ceded the field to government and business economists. This may also be an appropriate division of labor. Nonetheless, forecasting is terribly important to practical central bankers. At the Federal Reserve, in particular, enormous resources are devoted to this activity and, for the most part, it is done well.¹¹

I have one criticism, however, which is related to my previous point about lags and myopia. The forecast presented and discussed at each FOMC meeting generally covers only the next six to eight quarters. Relative to standard estimates of the lags in the effects of monetary policy on real output and, especially, inflation, that is much too short a time horizon. Nothing the Fed does today can be expected to have much effect on inflation six quarters from now, and the effects on both inflation and unemployment stretch out over several years. Policymakers must think in a much longer time frame. Ironically, the much-maligned Humphrey-Hawkins Act, for all its flaws, at least forces the FOMC to do this twice a year.

I know that Fed staff would object that anyone who claims to be able to generate a detailed quarterly forecast stretching more than two years into the future is perpetrating a hoax. And I agree. Indeed, I might go even further and argue that the data tea leaves enable us to peer no more than one or two *quarters* into the future.

¹¹ Romer and Romer (1996) find the Fed’s internal forecasts to be clearly more accurate than private-sector forecasts.

But monetary policymakers do not really need quarterly forecasts of dozens of variables. What real difference does it make if the growth rate forecast for business fixed investment in quarter $t + 5$ is 3.8 percent or 5.6 percent? In my view, policymakers need to pay more attention to the basic forces that are moving the economy in the medium-term—such factors as past and future monetary policy, fiscal policy, foreign demand, and so on. The requisite attention span is measured in years, not in quarters.

Policy Multipliers

While forecasts can be generated without using a formal econometric model, policy multipliers cannot be. Regardless of what you think about the Lucas critique, it is simply impossible to estimate how a policy move at time t will affect, say, inflation and unemployment at time $t + j$ without using *some* quantitative model of the economy. The model need not be a large-scale macroeconomic model; it can, for example, be a small vector autoregression that eschews structural identifying assumptions. But some kind of empirical model must underlie any dynamic multiplier path.

What model should the Fed use? Obviously, there is no right answer to this question. While on the Federal Reserve Board, I always insisted on seeing multiplier results from several different econometric models and several different vector autoregressions. This exercise held a pleasant surprise for me: it turns out that most empirical models of the U.S. economy these days generate rather similar dynamic multiplier paths for monetary policy—which was certainly not true years ago. To be sure, the various estimated paths are not identical. But they are sufficiently alike to hold similar implications for policy. Consensus is not quite the same as truth. But to a harried central banker, it is comforting.

Nonetheless, there is clearly much uncertainty over policy multipliers. How should a policymaker take this uncertainty into account? Many years ago, Brainard (1967) gave an answer that seems largely to have been forgotten. He argued that, under certain conditions, multiplier uncertainty makes optimal policy more *conservative* in the following specific sense: policy instruments should be moved in the same directions as under certainty equivalence, but by smaller magnitudes. I have dubbed this the *Brainard conservatism principle*.

Unfortunately, Brainard's result is not very robust in the sense that theorists use the term. It can be overturned, for example, by sizable covariances among the various shocks.¹² Furthermore, making it operational for monetary policy requires

¹² Here is a simple example that illustrates the intuition. (It is *not* a proof.) Let y be the variable that is to be stabilized, x be the policy instrument, m be the multiplier, and e be an additive error. Thus $y = mx + e$, with variance $\text{var}(y) = x^2\text{var}(m) + \text{var}(e) + 2x\text{cov}(m, e)$. If the covariance is zero, the effect of a change in x on $\text{var}(y)$ is clearly greater when $\text{var}(m)$ is larger. Hence x should be used less aggressively when m has greater variance. But if the covariance is negative enough, $\text{var}(y)$ might actually fall as x rises, which will encourage more aggressive use of the policy instrument.

a concrete definition of “doing nothing”—which has proven elusive to students of monetary policy over the years.¹³ Nonetheless, I wish more academics would train their high-powered tools on this question, for I can tell you that, as a Federal Reserve governor, I always viewed the Brainard conservatism principle as extremely wise.

“Thou Shalt Not Fine-Tune”

It has long been conventional wisdom in academia that policymakers cannot “fine-tune” the economy. Furthermore, they should not try, because their knowledge base is insufficient, their instruments are not that finely calibrated, and the economy responds with long and variable lags. I believed this myself. But after a stint as a central banker, I wonder whether the abjuration of fine-tuning has any operational meaning at all. I now suspect that the entire concept is epistemologically empty.

Part of the hostility toward fine-tuning is surely the notion that policymakers should not set their sights too high and expect to eliminate either the variance of real output around trend or the variance of inflation around target.¹⁴ I certainly agree. But so what? Does this imply that central banks should therefore not try to reduce these variances? Doesn’t even a poor archer aim for the bull’s-eye, even though he doesn’t expect to hit it?

One possible, though extreme, interpretation of the injunction against fine-tuning is the normative statement 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 interpretation 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 forsaking the whole messy business. But once you leave the realm of nonreactive rules and opt for some tuning, I fail to see any bright line—and maybe not even a dim one—between coarse-tuning, which is what central bankers are supposed to do, and fine-tuning, which is what they are supposed to avoid. Don’t you always do the best you can, mindful of all the uncertainties and aware that perfection will not be achieved?

Another possible interpretation of the abjuration of fine-tuning is an affirmation of Brainard’s conservatism principle: estimate what you should do, and then do less. If so, I have great sympathy. But I doubt that this is what anti-fine-tuners have in mind, for the strategy calls for constant adjustments of policy, even small ones, as new information is received. This sounds a bit like fine-, albeit cautious,

¹³ In Blinder (1997, ch. 2), I propose a concrete definition of the “neutral” level of the real federal funds rate, and I suggest setting the federal funds at this level as the zero point for monetary policy.

¹⁴ Herschel Grossman has suggested to me that “no fine-tuning” is really a code word for the belief that monetary policy should concentrate only on price stability and ignore output stabilization. If so, this is an abuse of language.

tuning to me. Indeed, yet another interpretation of fine-tuning is excessive fiddling with the policy controls—which, it is argued, creates unnecessary uncertainties in markets.

To make this discussion concrete, consider a situation the FOMC faced while I was on it. In the closing months of 1994, the U.S. economy attained a macroeconomic position that, if not quite ideal, was at least excellent: the lowest unemployment in years plus the lowest inflation rate in a generation. Improvement seemed unlikely.¹⁵ So a central bank that eschewed fine-tuning would certainly have been content to leave well enough alone and not twiddled the dials further. But what did that actually mean in practice? Holding the nominal federal funds rate constant even while inflation, long-term interest rates, stock market values and the dollar's exchange rate moved? Or holding the real rate constant? And should we have ignored forecasts that rising inflation was likely under unchanged policy?

My point is that monetary policymakers 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 its November 1994 meeting, held rates steady at the December meeting, raised rates by another 50 basis points in February 1995 and then stopped. 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.

Time Inconsistency and the Problem of Inflationary Bias

My next topic is one place where, in my opinion, academic economists have been barking loudly up the wrong tree and could learn a great deal from listening to practitioners. Starting with Kydland and Prescott's (1977) seminal paper, many theorists have fretted over the following time-inconsistency problem that allegedly bedevils monetary policy. Because the Phillips curve embodies a tradeoff between unemployment and *unanticipated* inflation, well-meaning central bankers are constantly tempted to reach for short-term employment gains by engineering inflation surprises. But you can't go to this well too often and under rational expectations, not very often at all. In the long run, discretionary monetary policy therefore produces higher average inflation than a mechanical rule, but not lower average unemployment.

Let me begin with a nonconfession: during my brief career as a central banker, I never once witnessed nor experienced this temptation. Nor do I believe my colleagues did. I firmly believe that this theoretical problem is a nonproblem in the real world because central bankers have found simple, practical ways to solve it. Furthermore, these solutions have nothing to do with the academic debate ignited by Barro and Gordon (1983), except for the insightful paper by Rogoff (1985). In particular,

¹⁵ In fact, two years later the unemployment rate was even lower and the inflation rate was no higher!

governments have not adopted precommitment strategies (“rules”) to help them resist temptation; nor have they created incentive-compatible compensation schemes for their central bankers. Instead, they brought inflation down dramatically by purely discretionary policy decisions. As in the Nike commercial, they just did it.

In terms of the theory, I believe that two closely related solutions were adopted. Models in the tradition of Barro and Gordon (1983) generally posit a loss function that looks like

$$L = (u - ku^*)^2 + \alpha(\pi - \pi^*)^2.$$

Here u^* is the natural rate, $k < 1$ is a parameter indicating the optimal level of unemployment in the absence of concerns about inflation, and α is, as before, an inflation-aversion parameter. Rogoff (1985) observed that one way to offset the inflation bias is to install a quite “conservative” person to lead the central bank—someone whose inflation-aversion parameter, α , exceeds that of society as a whole. This, I believe, is common practice around the world. Indeed, the noun “central banker” practically cries out for the adjective “conservative.”

The second solution to the Barro-Gordon problem is even simpler, but, as far as I know, has never appeared in the academic literature. Most models in the Barro-Gordon tradition trace the source of the inflation bias to the assumption that the parameter k is less than one. The bias disappears if $k = 1$.

Well, I can assure you that my central banker friends would not be surprised to learn that academic theories that assume that they seek to push unemployment below the natural rate then deduce that monetary policy will be too inflationary. They would doubtless reply, “Of course. That’s why we don’t do it.” Therein lies the solution: *Direct* the central bank to set $k = 1$ —that is, not to seek unemployment lower than the natural rate. In the world of practical central banking, this “solution” is, I submit, adopted as if it were second nature.

One subtle but important point needs to be made about this solution. It is not necessary to persuade the central banker that u^* is really preferred to ku^* in a social-welfare sense. Barro and Gordon (1983) and others have offered sound intellectual reasons to prefer unemployment below the natural rate in the abstract. It is only necessary to persuade or direct the central banker that his legal responsibilities require him to behave *as if* k were equal to one. *Homo economicus* may not behave this way. But responsible people, put in positions of authority, do.

In fact, this solution to the time-inconsistency problem applies just as well to the real-world version of Rogoff’s conservative central banker. It is not necessary to find a “truly conservative” central banker whose personal value of the parameter α is excessive; you can simply direct the central bank to behave *as if* α were high. In either case, central bankers set aside their own personal beliefs about what is best for society (α or k) and adopt instead parameter values that lead them to “do their duty.” You might call this simple solution “responsible behavior.” It is, I believe, the way parents, teachers, government officials and others solve time-inconsistency problems in the real world every day.

Following the Markets

Having just argued that one of the perils for monetary policy most emphasized in the academic literature is greatly overblown, let me now call your attention to what I believe is a truly serious problem that academics have ignored.¹⁶ Briefly stated, central banks can create a dog-chasing-its-tail phenomenon by following the markets too closely. Let me explain how.

Market participants are constantly evaluating the performance of the central bank and, in a sense, giving it advice in two principal ways. One is via the incessant din of market chatter about the central bank's behavior. Traders, financial industry economists and others are constantly evaluating monetary policy and predicting its likely future. Nowadays, these sentiments are carried to the desks of central bankers immediately by the wire services. The second, and more important, route involves asset prices—especially the term structure of interest rates and certain futures contracts¹⁷—which embody assumptions about the central bank's future actions.

Together, these two types of signals create a kind of biofeedback or grading system in which the markets first recommend or predict what the central bankers should or will do, and then reward them for doing it. While I never saw a single case of a central banker succumbing to the temptation that so worried Kydland and Prescott, I often witnessed central bankers sorely tempted to deliver the policy that the markets expected or demanded.

Why is this a problem? Because I believe that markets tend to get hyper-excited by almost any stimulus, sometimes succumb to fads and fancies and are often short-sighted. My experience watching markets in real time led me to formulate *Blinder's Law of Speculative Markets*: the markets normally get the sign right, but exaggerate the magnitude by a factor between three and 10. If this is true, a central banker who follows the markets too assiduously is liable to overreact to current data and tacitly adopt the markets' short time horizons as his own.

Herein lies an extreme irony. Maintaining a long time horizon is perhaps the principal *raison d'être* for central bank independence. Yet a central banker who takes his cues from the markets is likely to acquire the markets' short time horizon. That is why it is just as important for a central bank to be independent of markets as it is to be independent of politics.

Guarding against this occupational hazard is mostly a problem for practical central bankers, not for academics. But at least one aspect of the problem could benefit from some imaginative research. In talking to many market participants and market watchers, both while on the Fed and since, I have

¹⁶ However, see the recent working paper by Bernanke and Woodford (1996).

¹⁷ In the United States, the principal such contract is the federal funds futures market, which involves a pure bet on the future value of the federal funds rate.

frequently observed the following amazing oddity. Academic research showing that long-term interest rates are poor forecasters of future short rates¹⁸ is not only well known but also widely accepted in the markets. Yet everyone—and here I mean analysts, market participants and central bankers alike—continues to “read” the market’s expectations of future short rates from the yield curve, as if doing so made sense. I find it hard to explain why everyone is doing what everyone knows to be wrong. Yet it happens all the time. Perhaps the reason is that no one has offered a convincing alternative interpretation of the term structure.

Since the central bank controls only the very short-term interest rate, while long-term interest rates are more important as determinants of aggregate demand, a better analysis of the relationship between short rates and long rates would be much more than a welcome contribution to the academic literature. It would be immensely useful to practical central bankers.

Decision Making by Committee

I conclude with one final aspect of central banking that looked important to me while I lived in that world, but which appears to have escaped the notice of researchers.¹⁹ In many countries, including the United States, monetary policy decisions are made by a committee, not by an individual with a well-defined linear quadratic preference function. This institutional “detail” may—and probably does—have important behavioral consequences.

We all know that committee discussions must aggregate preferences, seek common ground and somehow produce a group decision. We all also know that committees can be slow-moving creatures—especially when they seek near-unanimity, as the FOMC does, rather than decide issues by majority vote.²⁰ I submit as a testable hypothesis that decisions made by committees tend to be more inertial than decisions made by individuals. Had Newton served on more faculty committees at Cambridge, his first law of motion might have stated that a *committee* in motion tends to stay in motion in the same direction unless acted upon by an outside force. I believe this applies to the FOMC. The question is whether such inertia serves us well or poorly.

On the negative side, the inertia that derives from committee decision making²¹ may exacerbate a problem I mentioned earlier in discussing lags in monetary policy—the tendency for central banks to overstay their hand, pursuing either eas-

¹⁸ For a discussion of the issue in the Summer 1995 issue of this journal, see Campbell (1995).

¹⁹ A rare exception is Faust (1996).

²⁰ This is, in itself, an interesting research question: when should a committee reach decisions by majority vote (as the Supreme Court does) rather than by consensus (as the FOMC does)?

²¹ In the case of the FOMC, this inertia is probably mitigated by the fact that the chairman normally dominates the proceedings.

ing or tightening for too long. On the positive side, one could argue that a little stodginess at the central bank is entirely appropriate. In addition, the need for the Chairman of the Federal Reserve Board to bring the FOMC along serves as a check on his personal power over monetary policy, which would otherwise be virtually unlimited.²² It may, in fact, produce a kind of rough adherence to the Brainard conservatism principle, which I commended earlier.

Conclusion

Having looked at monetary policy from both sides now, I can testify that central banking in practice is as much art as science. Nonetheless, while practicing this dark art, I always found the science quite useful. And I came to believe that the Federal Reserve and other central banks could profit from more disciplined and systematic thinking.

There has long been a symbiosis between practical central banking and academic research on monetary policy. Poole's analysis of the choice of the monetary instrument is only the most outstanding example. But while this symbiotic relationship continues, I believe that large potential gains from trade in ideas between practitioners and academics remain unexploited.

As a general matter, I firmly believe that monetary policymaking in the United States and other countries could and should become far more conceptual and less situational. This sounds a bit like saying that central banking should become "more academic"; but, as you will see in a moment, that is not what I mean. My experience at the Fed convinced me that central bankers are often so absorbed by the "trees" of the current economic situation that they lose sight of the macroeconomic "forest." They need to be constantly reminded of the latter. They need to specify their ultimate goals and their long-run plans to achieve them, however tentative those plans may be. And they need to internalize better the dynamic programming way of thinking so as to avoid the pitfalls of what I earlier called "looking out the window."

For their part, academic researchers need to train their powerful tools on real-world issues instead of chasing intellectual will-o'-the-wisps. For example, I have argued that theorists have lavished vastly too much attention on a nonexistent time-inconsistency problem while ignoring a much more real problem that arises when central bankers "follow the markets" too closely. Academic economists could also be more helpful to practical policymakers if they would develop an empirically coherent analysis of the term structure of interest rates, model the central bank as a committee, investigate the robustness of Brainard's conservatism principle, and study the conditions that make either "opportunistic" or "deliberate" disinflation the preferred strategy.

²² In some other countries, such as Canada and Italy, the head of the central bank makes monetary policy decisions on his or her own.

There is little doubt that the Federal Reserve is the most analytical and economist-dominated agency in the U.S. government, maybe in any government. If the academic community is not connecting well with the Fed, there is reason for concern—and much work to be done. I believe that both the analysis and practice of monetary policy could be improved if central bankers would think more conceptually about what they are doing and if academics would plant their feet more firmly on *terra firma*. Alas, that is old news. As Alfred Marshall (1890 [1936], p. 779) noted nearly a century ago:

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.

That's a nice phrase, "trained common sense." It is, I believe, what we should all be striving for. But we are not there yet.

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