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To: Members of the FOMC

From: J. Kalchbrenner

Attached are comments by Governor Wallich and Gary Fromm of the NBER concerning materials that are part of the research done in Stage II of the Subcommittee on the Directive. These comments were presented at the December 1975 meetings of the American Economic Association at the session concerning optimal control applications to economic stabilization problems. The comments pertain to three of the papers cited in the Interim Staff Report: Stage II for the Subcommittee on the Directive (nos. 1, 6, 15 in the bibliography).

Attachments

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OPTIMAL CONTROL AND THE POLICYMAKER

Remarks by

Henry C. Wallich
Member, Board of Governors of the Federal Reserve System

at the Session

Application of Optimal Control to Problems of Economic Stabilization

at the Annual Meeting of the

American Economic Association

in

Dallas, Texas

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The use of optimal control techniques in planning for economic stabilization is approaching the policy stage. At the present time, as the papers before us show, the principal application of these techniques has been the examination of models and of past policies. Its use for effective policy advice still seems some distance away. But initial efforts to build an optimal control approach into Federal Reserve policymaking are underway. I believe that there is potential for progress at both the technical and the policy levels. It is important, therefore, for the producers and the

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Note: The views expressed herein are my own and do not necessarily reflect those of the Board of Governors or the Board's staff.

potential users of this technique to become better acquainted. Model builders and policymakers must explore one another's needs and capabilities.

I appear here, of course, as a potential user, with no pretense to technical expertise. In this capacity, I would like to comment on a number of points raised by the papers of Ando-Palash, Chow, and Kalchbrenner-Tinsley. My remarks will be addressed mainly to the loss function, the departures from present practices implied, some features of the models employed, and the relation between uncertainty and the scale of policy action.

Policymakers, I believe, regard their role as somewhat more modest than that with which the terminology of the loss function sometimes endows them. The Federal Reserve, to be specific, is responsible for only one phase of the nation's economic policy -- the handling of monetary policy. The overall objectives, moreover, are given by the Employment Act. Most of the economic policies that influence the rate of growth, employment, and the degree of price stability, are handled elsewhere in the government. Particularly when several objectives are involved, which obviously cannot all be attained with one instrument, it seems somewhat presumptuous to state one's preferences in the form of "targets."

The monetary policy official naturally has ideas also about desirable fiscal policy, and about many other policies that influence economic development. Only in the very short run can he make fairly

firm assumptions as to what these policies will be. For the longer run, a not unreasonable attitude for him may be to think of monetary policy as helping to create the environment in which other public policies, as well as decisions made in the private sector, will become effective. Any given monetary policy may be consistent with alternative combinations of growth, unemployment, and inflation. The monetary policymaker will adjust his action to what he sees happening in these other spheres. But he should not overestimate his ability to influence the outcome.

The time horizon over which target values are to be set likewise presents difficult problems. One may believe that a lower rate of economic expansion in the immediate future will lead to more sustainable growth and lower ultimate unemployment and inflation than would a more aggressive policy. But unless such preferences are built into a loss function, and a long time horizon is allowed for, rather extreme policy proposals may follow from optimal control techniques applied to econometric models with long lag structures, as some of the papers at this meeting indicate.

The policymaker may also be troubled by an appearance of misplaced concreteness. He may be accustomed to thinking in directional terms -- up or down -- or in terms of rates of change -- faster or slower. He may want to reserve judgment as to precise targets for unemployment and inflation until the economy is a little

closer to what he might consider optimal. And if in addition he were asked whether he has a quadratic loss function, i.e., whether he is indifferent to an equal degree of over- and undershooting of his targets, he might be tempted to think the whole thing a spherical nuisance, i.e., a nuisance from every angle.

Monetary policy in the United States, moreover, is made by a group, the Federal Open Market Committee. The loss functions of all its members are unlikely to be identical. Perhaps one could think of a consensus loss function, or at least of one that would be Pareto optimal as between two disagreeing groups. But as a practical matter, it is probably easier for such a group to arrive at agreement on something on which they are compelled to take immediate action such as bank reserves, or the money supply, or the Federal funds rate, than about desirable conditions in the economy over which they have no immediate control. This leads me to my next topic, the role of intermediate targets.

Intermediate targets and other recent developments in monetary policy

In an optimal control framework, it is argued, there is no need for intermediate targets. It is the ultimate goals that go into the loss function. The pursuit of these ultimate targets by means of intermediate targets such as money supply or interest rates, it is argued, is in theory suboptimal. Their function is to serve as

information variables from which insights into developments in the real sector can be gathered. The central bank should look at them as it should look at other readily observable data -- everything should be looked at as a source of information about real developments that are not directly or not frequently observable.

This line of argument is in conflict, of course, with some of the main developments in monetary policymaking in the United States during the last 10 or 15 years. There was a time when the Federal Reserve indeed did "look at everything." It was not a technique lending itself to much precision, and the development of intermediate targets was generally regarded as a step forward.

The advantages of intermediate targets have often been described as those of better observability and better control. I regard particularly the latter as significant. Moving directly from money market conditions or bank reserves, which the central bank controls, to the ultimate targets of growth, employment and price stability conveys very little of a sense of the quantitative impact of monetary policy action. At best, one can have a sense of the direction of policy, and even there, as we have learned, mistakes are possible if the central bank does not distinguish between endogenous and exogenous movements in its policy variables. This uncertainty is enhanced by long lags of policy action. By focusing on the money supply or on interest rates, a better intuitive sense of the thrust of policy is

likely to be achieved. The sense of having some protection against the extreme kind of error that might manifest it by extreme values of these variables, can be helpful.

Policymakers may be reluctant to surrender this means of obtaining some direct feel for the economic meaning of their actions and to replace it by a system that tells them to move some variable like unborrowed reserves, or the Federal funds rate, perhaps drastically, in order to achieve some particular results in the real sector. It might come close to flying with an automatic pilot. Manual control, I believe, would instill greater confidence.

Concern over possibly extreme values of policy variables is likely to be another obstacle to the greater use of optimal control that will have to be dealt with. The experience of the postwar period seems to show that extreme settings of policy variables, even for relatively short periods, can be destabilizing. Frequent variation in instrument settings likewise may add to instability. Given the lags and the uncertainties, moderate instrument settings and a degree of steadiness seems preferable most of the time, quite aside from possible side effects of wide policy gyrations on the functioning of financial markets. Policymakers who do have such preferences for moderation and steadiness can, of course, put them into their loss function, as the paper by Kalchbrenner and Tinsley does. But that is only partial protection, unless the penalties assigned to wide deviations in the paths of instrument variables are very high and

hence perhaps unreasonable. Moreover, one may remain suspicious about the properties of a feedback process that needs to be disciplined in this artificial way.

Models

The policy evaluation -- or advice -- derived from an optimal control system presumably is no better than the model through which the feedback flows. Policymakers are likely to be interested in several aspects.

First, while models have reached a certain degree of proficiency in short-run predictions under ordinary circumstances, and in that sense agree with each other, there nevertheless seem to be important differences. Policy multipliers seem to vary importantly among models. Even within particular models, these multipliers seem to be sensitive to small changes in the specification of particular equations, or to the choice among alternative equations of seemingly equal theoretical and empirical plausibility. Chow's paper suggests a minimax strategy, choosing among the models on the basis of which minimizes the worst case. That procedure would provide some insurance, but otherwise seems to adopt a rather pessimistic slant. Alternative procedures might be to examine models for robustness of their policy advice under varying assumptions, or perhaps to look for a policy that is robust with respect to switches among models. Clearly one of the precautions policymakers would want to apply is to use a variety of models. But when there are significant differences, it is not easy to work up much confidence.

Second, there is likely to be concern about the possibility of bias in particular models or specifications of loss functions. Ando-Palash point out that in a quadratic loss function, if the target values for unemployment and inflation are set low, such as at zero, the unemployment variable will obtain an unintendedly high weight. My own concern is that on the contrary an inflation bias may enter the process, via the structure and the typical use made of most models. They seem to underestimate inflation because of a questionable process of forming price expectations, which relies on distributed lags of past experience instead of on rational expectations based on observed government policy. The paper by Kalchbrenner-Tinsley makes reference to the severe underestimation of inflation.

Additionally, an inflation bias may appear if model simulations are kept too short, since price effects typically lag volume effects. Long model simulations into the future are not popular, owing to the difficulty of estimating exogenous variables and perhaps also because of the longer run instability of some models. Nevertheless, by limiting a simulation or forecast to only a few quarters ahead, the long-run price effects may be cut off. These effects then will carry less weight in the optimal control simulation while volume effects, which occur with less of a lag, dominate.

Third, still another question about the performance of present-day models relates to their ability to deal with severe exogenous shocks such as the devaluation of the dollar, or the rise in oil prices. Such

effects, as Kalchbrenner-Tinsley put it, simply "had no place to go in traditional econometric models." Now that the high rates of unemployment and inflation that resulted have become part of the data, users of the models' output are likely to be concerned about possible distortions from these outliers.

Uncertainty

Policymakers may be concerned about the kind of advice they are likely to get when the outlook appears more uncertain than usual. At such times, anyone with a firm opinion is likely to carry disproportionate weight, but in the case of advice from a model that is part of the risk to be guarded against. The natural tendency of policymakers, under such conditions, will be in the direction of greater conservatism, i.e., to do "less" than they otherwise would. The meaning of "less" may not be the same for everyone, although technically it would seem to imply that policy action should then be so designed as not to add to the variance of the loss. In practice, it may just mean to keep doing whatever was being done before.

Among technicians, views do not seem to be unified concerning the implication of varying uncertainty. A well known theorem by William Brainard states that, under specified conditions, uncertainty reduces the scale of action. Kalchbrenner-Tinsley, in an earlier paper, seem to be of the same opinion. I have heard others quoted to the effect that uncertainty probably but not necessarily implies greater

conservatism. In any event, the users of optimal control probably would like to know whether they run the risk, under particularly ticklish conditions, of being confronted with extreme advice from this source as they often are from other sources as well.

The outcome of the policy simulations

Ando-Palash and Kalchbrenner-Tinsley have very properly indicated to what extent their findings point to alternative ex ante policies that in the light of contemporaneous information could have been adopted to produce better results. Ex post simulations, employing information that policymakers did not have at the time, may provide valuable lessons for the future but do not constitute a valid criticism of past policies. If I understand the two pairs of authors correctly, they both claim that, with the benefit of hindsight, policy could have been significantly improved. Kalchbrenner-Tinsley also seem to find that, on an ex ante basis, optimal control would not have done better. Feedbacking, in other words, is not enough to produce better policies, according to their findings. I might add that in a set of papers by Hyman-Shapiro and Hirsch that will be discussed this afternoon, evaluating recent policies with the aid of eight alternative models in an optimal control framework, the conclusion was reached that even with the benefit of hindsight the inflation and recession of the last few years could not have been avoided, although policy could have been improved upon.

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DISCUSSION, Gary Fromm, National Bureau of Economic Research

The papers presented at this and related sessions on optimal control are among the most important being given at the 1975 annual meetings. While none of them is pathbreaking, each contributes to a growing literature in a relatively new field in economics, one that should expand in signififance as knowledge and techniques develop over a span of years. At this stage it is easy to find flaws in the methodology and its application for the design of optimal economic policies. But, especially on the eve of the 1976 Bicentennial and Adam Smith's Wealth of Nations, it should be recognized that perfection in theory or practice is not to be attained at the outset but only after substantial development and experience.

To some economists, especially policymakers, the thought of using formal models and control theory for the design of optimal stabilization and growth policies seems, on its face, as nonsensical. Yet these same skeptics speak approvingly of automatic stabilizers and formula flexibility, which are highly related to types of strategies that would result from applying control theory methodologies. It should be recognized that at one limit, control theory produces simple strategies like Milton Freidman's prescription of constant money supply growth. But, even if simple rules only are desired, it seems likely that somewhat less rigid control mechanisms could yield improved economic performance. For instance, a slightly less restrictive economic strategy would permit seasonally adjusted money growth rates to vary systematically within prescribed bands inversely to a leading indicator index.

However, even with application of more complex optimal control strategies, complete stability and growth objectives are likely not to be achievable because of lack of knowledge of the structure of an evolving economic system, imperfect and missing data, modeling deficiencies of misspecifications, incompleteness, and aggregation, and stochastic shocks. But, I am certain

that the results reported at this session and those of comparable papers are valid - use of control theory techniques can reduce output fluctuations and inflation while raising the realization of economic potential.

With that introduction let me now turn to the present papers. Rather than picking on details of each, I will concentrate my remarks on issues common to all. One of the key aspects is the choice of an objective function. Often, as is the case in several papers here, a quadratic loss function is chosen such that deviations from targets are penalized at nonlinear rates. Such a loss function may, perhaps, correspond to actual preferences. However, analysts frequently choose targets arbitrarily and it is not realized that non-optimal solutions may be derived as a consequence. This can arise for two reasons. First, with a multiple argument objective function, if all targets are not achievable, points within the possibility frontier are as likely to be selected as solutions as points on the frontier. Second, unless the targets are on the frontier at the bliss point, target achievement does not lead to maximization of social utility (or, the converse, mimimization of disutility) nor to optimum policy. That is, a target loss function is appropriate only if targets are at the bliss point. Determination of the latter requires knowledge of both the structural constaints of the system (the frontier) and the positive utility function. But, if the positive utility function is known (or assumed), then it no longer is necessary to ascertain the bliss point in advance, and direct solution for optimal policy can be undertaken without specifying any targets or goals.

Clearly, the economics profession must do considerably more work on the determination of social utility functions before strong reliance can be placed on optimal control policy prescriptions. Again, despite the fears of some skeptics, this is not as hopeless as it seems at first blush. Both direct

survey and indirect revealed preference methods already have shown some promise in ascertaining social tradeoffs or preferences for output, inflation, income transfers, and related variables. It should be remembered, too, that even if social preference functions are not explicitly identified, they are implicit in economic policy choices made by the President and the Congress. The key question is can we do better by consideration of explicit tradeoff functions than by leaving them hidden and implicit. The answer, I believe is clear. As in other areas of decision analysis, formalization of implicit assumptions and structural characteristics (the tradeoff functions and constraints) should lead to more informed and intelligent choices which are more consistent with welfare maximization than those which would occur without such explicit identification.

In specifying or estimating these welfare or tradeoff functions, analysts should beware of strong simplifying assumptions such as ignoring time dimensions and preferences, complementarities in preferences among arguments, tradeoffs in amounts and variations in arguments, and, in general, the impact of uncertainty and stochastic influences. Too often, for the sake of expediency in the ease or cost of computations, extremely simplified functions have been assumed. There is need to perform sensitivity analyses with different forms, arguments, and parameters of such functions because policies and their ranking can change dramatically as these are altered. Fromm and Taubman found, for instance, that choices between policies shifted as the elasticity of substitution in a CES utility function was varied within plausible limits.

The same violence of simplications has been true with regard to structural constraints. If the world truly were linear and recursive, optimal control solutions could easily be calculated. But most models of the economy reveal significant nonlinearities and, in selected blocks, strong simultaneity. My

own experience with the Brookings and Fed-MIT models has shown that linearizing such systems, even on a piecewise basis, drastically alters structural characteristics and multipliers on policy instruments. Therefore, extreme caution is advised in interperting optimal control results that depend heavily on linear approximations.

The handling of uncertainty is another problem area. Again, based on simulations with the Brookings model and employing a utility function that incorporates risk preferences, it would appear that strong simplifications, such as certainty equivalence, are dangerous and lead to non-optimal policy prescriptions. One clear need in this area is to define some terminology. For instance, we need definitions that differentiate between stochastic terms in estimation and model solution within sample periods, beyond sample periods, and for residuals of ex ante and ex post predictions with and without introduction of prior information such as constant and parameter adjustments or add factors.

In the short time allotted, it has been possible only to touch on a few general issues and not grapple with details and some of the admittedly strange results of applying control techniques in the Ando-Palash and Kalchbrenner-Tinsley papers. But these authors and Gregory Chow, are to be congratulated for fine efforts which should lay a basis for much future work in this important field.