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Chapter Author: Robert S. Holbrook

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Some Comments on "Comparison of Econometric Models by Optimal Control Techniques" by Gregory C. Chow

ROBERT S. HOLBROOK

DEPARTMENT OF ECONOMICS
UNIVERSITY OF MICHIGAN
ANN ARBOR, MICHIGAN

My comments are directed toward Professor Chow's paper and can be divided into two parts. The first part will focus on those aspects of the paper directly related to its title, that is, model comparison by control techniques. The second part will be devoted to a brief discussion of some peripheral issues only touched upon in the paper.

In his paper, Chow is apparently concerned with the problem of model comparison, rather than model evaluation. As I understand it, the notion of evaluation implies the existence of some type of standard, against which the model is to be measured. When we use the model in an attempt to simulate some historical period or to forecast outside of the fitting period, there are several standard criteria by which its performance can be judged. The application of control theory also produces a path, but a path which, by its nature, has never been explored, and hence cannot have a real world counterpart to be used as a standard for evaluation.

Model comparison, however, need not, and Chow's paper does not, have anything directly to do with real world standards. What he suggests here can best be viewed as an addition to, but not essentially different from, the set of dynamic multipliers and other similar statistics typically used to summarize the behavior of a model. But just as lists of the multipliers implicit in the Michigan and Wharton models, for example, would be of little use by themselves as aids in deciding which model to use as a basis for policy

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decisions, the same could be said of lists of the results of optimal control runs on the same models. They help us to describe a model in terms of its behavioral characteristics but will not tell us much with respect to whether the model is good or bad, correct or incorrect.

Within these limitations, what information can best be provided by one or more optimal control runs? For a model that is linear in all variables, lagged as well as current, and with only additive disturbances, the answer is probably very little. In this case the reduced form conveys virtually all the necessary information in as useful a format. It is when the model is nonlinear that the control approach becomes most valuable and perhaps indispensable. Chow argues here and elsewhere that optimal control is useful as a device for forcing the model to trace out its unemployment-inflation trade-off, its maximum sustainable rate of growth, and other interesting target-constrained paths. We are in full agreement on this point, and I would maintain that this is the only sensible way to proceed, unless the model is sufficiently simple to permit an analytic solution. In a complex model the alternative to the control approach is to search over a grid in some systematic way, but such a search is unlikely ever to find the best point, and it is virtually certain to be more expensive.

It is worth noting, however, that when a control algorithm is being used in this way, as a lever to force the model into a variety of positions one might not otherwise find, the loss function has become a tool, a means to an end, rather than an end in itself. Unlike its traditional role in the usual policy making or policy evaluation context, where its form and coefficients are designed to reflect some ultimate decision maker's attitudes, here the loss function is entirely arbitrary, its coefficients have no independent meaning, and it is merely a device to cause the solution program to produce a certain desired result. An implication of this fact is that although the loss function coefficients necessary, for example, to force the Michigan model to exhibit its steady-state unemployment-inflation trade-off might differ from those appropriate to the Wharton model, the particular coefficients used, and hence the values of the loss functions themselves are of no particular interest. The frontier being sought is a characteristic of the model itself and is essentially independent of the loss function.

Chow suggests other ways in which control theory might be used to compare models, but I find these far less compelling than that discussed above. In particular, he recommends that a loss function be chosen, that the values of Q, q, and $V_1(\hat{x}_1)$ be computed for each model, and that these values should be compared. But these values are so heavily dependent on the chosen loss function that I question whether such a procedure would provide us with much useful information with respect to the models. Faced with a serious policy problem as characterized by a particular loss function, one would doubtless wish to observe the optimal policy paths (and perhaps also $V_1(\hat{x}_1)$) derived from several alternative models. But it is not clear that this exercise, when performed with an arbitrary loss function, would tell us much of interest or value about the models themselves, beyond what was already known.

Moving on to the question of method, several points can be made. Chow recommends that when selecting an optimal policy one should use the closed-loop feedback control approach rather than the open-loop approach suggested by others. If one is taking account of the presence of multiplicative errors, then the closed-loop approach is the only correct one, and the open-loop approach will yield an inferior policy decision, even in the first period. period. But if one is taking account only of the additive error terms, then a succession of first-period results from an open-loop approach will be identical to those from a closed-loop solution (Holbrook & Howrey, 1978), and there is growing evidence that certain open-loop solution algorithms are faster (and hence cheaper) than those available for closed-loop control (Drud, 1978). The same applies even more strongly in the case of deterministic control, where only a single, once-and-for-all solution is necessary for the open-loop method.

A second issue has to do with the appropriate response when the optimal path derived from some model exhibits "undesirable" characteristics, such as excessive changes in government spending or negative interest rates. In some cases these are undesirable because we believe that the policy maker would not permit them to occur. In that event, they ought to have been included in the loss function in the first place, with appropriate weights. And in other cases, such as the negative interest rate case, we simply do not believe what the model is telling us. But in principle we should not have to observe such a path at all in these circumstances. Rather we should have given thought in advance to the model's limits of believability. These limits could be absolute bounds, such that we are willing to accept any value up to that limit and none beyond, or we might wish instead to attach a gradually increasing cost as the value goes outside some predetermined range. Either case can easily be dealt with within the optimal control context. But to as great an extent as possible, thought should be given in advance to both the range within which the model's results are to be believed and the policy-maker's presumed attitude towards the behavior of his instruments.

A third and last technical point is related to Chow's discussion of the use of optimal control to evaluate policy over the early 1970s. That experiment, and virtually all such experiments of which I am aware, made use of a planning horizon with a fixed terminal date, which was in this case 1975.1. But it is really inappropriate to compare the policy actually followed over that period with a policy that is "optimal" only on the assumption that the quarters subsequent to 1975.1 were never of any concern.

There are many other problems in setting up and interpreting such experiments, but this one is particularly important and casts a shadow over the results. As Chow points out, the fixed terminal date fails to allow for the delayed impact of within-period policy on post-period target values, which is always important but may be crucial in the case of inflation. This problem of "terminal hangover" should be eliminated by the use of a rolling horizon, which has a constant length but moves forward over time. In addition to eliminating terminal hangover, this approach has the added attraction of more closely approximating the behavior of an actual policy-maker.

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