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# REFLECTIONS ON MACROECONOMIC MODELLING; CONFESSIONS OF A DRI ADDICT

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Like the others on this program, I am here mainly to remind everyone that Otto Eckstein was one of the good guys. There is a sense in which my knowledge of Otto extended over a period longer than his professional life. During the academic year 1949-50, my wife and I were in New York. I was writing my thesis on an SSRC dissertation fellowship, and studying mathematical statistics at Columbia. My wife was working at the Federal Reserve Bank of New York. I think it was her boss Henry Wallich who brought back the news from Princeton that there was this absolutely brilliant undergraduate burning up the economics department. It was Otto. The story was quite true. (My wife does not remember it exactly that way. If the story isn't true, it might as well be.)

As some of you know, Otto's first work, beginning with his Ph.D. thesis on water resources, was about project evaluation, especially the difficult problems induced by the need to take into account long streams of benefits and costs whose private and social values were likely to diverge. I don't know what chain of thoughts and events brought him to macroeconomics. Maybe it was sheer omnivorousness; maybe his experience with the large Joint Economic Committee study of output, productivity and the price level had something to do with it. By whatever route, Otto became the natural successor to Jan Tinbergen and Lawrence Klein in the illustrious family tree that forms the ancestry of the large-scale complete macroeconomic model of today. That is the context of my reflections here.

Ten or twelve years ago the thought occurred to me that we -- the economics profession -- were dissipating our store of empirical knowledge foolishly. Every so often someone estimates a demand function for automobiles or washing machines or onions, writes an article in Econometrica or the AER or elsewhere, and that is usually the end of it. Five or ten years afterwards, nobody knows whether that particular demand function is still any good, i.e. fits the recent data as well as it had done in and near its sample period. (I don't mean to single out demand functions; I could have used any econometric relationship as an example.) Nor is it a simple job to find out. As everyone knows who has ever done that sort of work, it is always necessary to fiddle the raw data one way or another: deflate by a specifically-constructed price index, correct for changes in coverage, match up a price index with a quantity index, that sort of thing. It seemed to me that it would be worthwhile to set up a continuing organization to do just that. We would at least know how reliable our basic empirical constructs really are. I reckoned it might even be a useful educational exercise for students to retrace the original author's steps, reconsider the estimation methods, figure out what might have gone wrong if the fit has indeed deteriorated, and so on.

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When I mentioned the idea to one of my colleagues, a knowledgeable and distinguished econometrician he pooh-poohed it for an interesting reason. He argued that pretty soon all such empirical work in economics would be carried on in conjunction with one of the large macroeconomic models. That was not only the efficient way to check and preserve econometric work; it was already being done in such organizations as DRI. Since at that time I always believe everything my colleagues told me, I abandoned the project at once. I don't suppose everyone in the business would have agreed with that view even then. But it is interesting that a leading practicing econometrician believed only a dozen or so years ago that the ongoing large macroeconometric model would be the dominant format for empirical research in the future. The point of the story is that I think it sounds less plausible now than it did then.

I have no idea what Otto's advice would have been, had I asked him. He would probably have wanted to let a hundred flowers bloom rather than bring almost all empirical work under the aegis of the big models. As for me, I have always been ambivalent about big econometric models, and I want to explain my ambivalence today. I used to talk about this sort of thing with Otto occasionally. He did not share my doubts about macroeconometrics. But that didn't keep us from being friends and allies. I think it is a fitting tribute to his memory to continue the dialogue with you all.

I propose to describe the details of my ambivalence, not because it is mine but because I think that many economists actually feel the same way. They are ashamed to say so, probably because ambivalence is not fashionable among economists, who seem to be only too willing to speak with assurance on subjects that call for more modesty and a degree of uncertainty.

The large econometric model responds to a frame of mind that thinks the real economic world is not fundamentally very noisy. In this view a good model would explain nearly everything that happens. If one did not have that feeling it would not make so much sense to build a very inclusive model with many endogenous variables, and then fit and re-fit it, and re-fit it again every year or so. If the time series y and x are very nearly exactly linearly related, it will do no harm to mine the data. There will be no temptation to try a very complicated regression, a high-degree polynomial say; and if you do try it, the results will soon tell you that the relation is linear, and will get it right.

If those residual errors are large, however, the situation is different. Trying harder and harder to get an exact fit, by letting recent history dictate a change in functional form, say, or the introduction of additional variables, is more likely to lead to over-fitting, and thus to the occasional bad error, to the lame excuse, and to more over-fitting. The same thing can happen with small models, of course, and it often does. With big models, however, it is harder for anyone to see what is going on.

The habit of over-fitting is reinforced by the desire to forecast successfully. And forecasting seems to have an irresistible attraction. It is a widespread but misleading belief in our profession that the ability to predict is the only true test of the validity of a model. There is a sense in which that is harmless -- if you mean by "prediction" foretelling the outcome of a controlled experiment. That is generally what hard scientists mean by prediction. You will rarely find a chemist forecasting what collections of molecules will come rolling down the Rio Grande 365 days from today. When natural science does engage in unconditional forecasting -- as with the weather -- it is not famous for perfect accuracy. But nobody regards theories of atmospheric dynamics as false for that reason. One just acknowledges that the differential equations are hard to solve, that the data are sparse and imperfect, and that a lot of unpredictable events affect the weather. (The case of forecasting the motion of nearby heavenly objects is exactly a case where the system is not very noisy on the scale of the thing being predicted.)



Besides, forecasting is not everything, nor even the most important thing. I imagine we are all convinced that the theory of evolution by natural selection contains a large part of the truth about its subject. But I am not aware that it makes many predictions -- unless you mean that the theory predicts that a knowledgeable person will be able to give a plausible account in terms of survival value of many characteristics of species observed in the present or recorded in the past.

Anyway, partly because of the drive to forecast and partly because of the inevitable dynamics of research, large inclusive macroeconomic models seem always to push beyond the simplest and mot robust empirical relations and tackle the ones on which the facts speak softly and theory has little that is definite to say. This can only add to the instability of estimates and the temptation to overfit.

I do not wish to be misunderstood. The large macroeconometric model is an indispensable tool. Economists are distinguished from other primates because we account for indirect effects that they never see. The complete model is how we do it. That is why, despite my misgivings, I was a middle-aged DRI addict; I always looked forward to the monthly <u>Review</u>. Whenever I felt impelled to think about what is likely to happen in the near future, or what the differential effect of this or that policy change would be, I know my natural inclination was to start with the DRI forecast or the DRI estimate. I think I understand some of the reasons why that was so.

The first reason is actually a little subversive of the model itself, but only a little. DRI forecasts and DRI policy analysis had Otto Eckstein written all over them, and Otto was a very good economist. The forecasts rested on add factors, of course, and the add factors rested on Otto's intuitions, and on the closeness of his ear to the ground. (I don't mean that he did every one personally, but I bet he made damn sure that nothing he regarded as silly every came out of mechanical procedures to see the light of day.) Similarly with attempts to estimate the differential incidence of fiscal and monetary policy measures; the written documents have the air of telling it just as the equations of the model say it is, but I doubt that Otto ever released an analysis of an important policy question that didn't make sense to him. Experience had taught me that if it made sense to Otto, then eight or nine times out of ten it would make sense to me.

The second reasons for my DRI addiction is quite different. One of the the nice things about a large econometric model is that it always has an answer for everything, whether you want to know about mortgage rates, auto output, the price index for food consumed away from home, or corporate profits by two-digit industry. That is certainly very convenient, not only because you can always get a start on whatever aspect of the economy you are thinking about, but also because once the model tells you what it thinks about something you are deeply interested in, you can begin to think critically about the model. If you disagree, you naturally wonder why; and if you agree you have more confidence in the model's opinion about the next variable.

And that leads me to a third comment. For all our fancy talk about testing hypotheses and estimating structural parameters, I think that econometric modelling has actually made very little progress in doing those profound things. Very few significant hypotheses have achieved universal acceptance or universal rejection as a result of econometric testing. Instead, I think, the main function of econometric modelling is rather to provide very sophisticated descriptive statistics. A simulation run of an econometric model is a powerfully-stated opinion about the way economic variables hang together. Of course a simulation can easily give the wrong impression about partial co-movements: whether x and y tend to move together or in opposite directions, net of the influence of z. But it does summarize data; and some sort of mental summary of the economic world is what the economists needs to have. It is a major achievement.

I suppose a vector autoregression does much the same in a different way. I can only report my own reaction. The day after I have looked at a vector autoregression, I can no longer remember what it says. I am sure that I would improve with training and practice. But I still have the feeling that a VAR, especially a big one with many variables and high-order different equations, doesn't stick to the ribs. I suppose that must be, ultimately, because what I want a summary of the data to do is to update my priors, to tell me whether I can go on believing what the totality of my experience has led me to believe, or whether I am in trouble. The VAR, as a direct consumption good, does not describe the data in a way that helps me. It may help Christopher Sims a lot, and that would be fine with me. There can be more than one way to skin this particular cat. The important thought I want to get across is that there is nothing undignified about econometrics as descriptive statistics rather than hypothesis-testing.

Despite all the doubts I mentioned earlier, I would rather have my summary of the data served up in the form of the response repertoire of a structural model. I want to know not whether an impulse originating in M or G finds its way into Y and P through this or that chain of uninterpretable autoregression coefficients or serial covariances, but whether it does so through this explicit causal chain or that one. One of my unhappinesses with the large all-inclusive macroeconometric model is that it, too, has a way of burying the causal connections in a vast exfoliation of regression coefficients, many of which are inevitably clinging to statistical significance by their fingernails, if at all, and then only by virtue of some particular choice of functional form, sample period, or other casual decision. Nevertheless I think one of the main sources of my DRI addiction is that every month it provides an orderly description of the data, organized in such a way that one's attention is called to events that seem to conform with a reasonable person's understanding of the economy, and also to events that look anomalous given one's own expectations about the way the world works.

That the DRI model can be read that way reflects it eclectic character. No doubt the model inherited its undoctrinaire character from Otto. You may remember that a couple of years ago Melvin Reder described one well-known persuasion within your profession as practicing "tight-prior economics." He intended that as a sympathetic characterization, though I suppose the point was that the tighter the prior, the more impervious to facts. No one could say <u>that</u> of Otto or the DRI model. Everything was always open to revision in the light of the facts. Perhaps that is a natural tendency for the big-model builder. Any doctrinal difference can usually be reduced, at least crudely, to a difference of opinion about what the important right-hand-side variables should be in some structural equation. The big-model builder is tempted to say: what the hell, let's toss them all in and "let the data decide." That is in the descriptive-statistics spirit, so I ought to approve.

The benefit of this open-mindedness is tempered, however, by the likelihood that the outcome of such experiments will be anything but robust. That is why the process is so seldom decisive, and doctrinal disputes persist forever.

Perhaps I had better say explicitly that this sort of permissiveness does not mean that the models are "untheoretical". You have only to read one of Otto's full-dress descriptions of the DRI model, for instance, to see that it is full of appeals to bits of theory (and these are cogent not mere name-dropping). It is only very recently that the



opinion has sprung up among some economists that the only way an economic model can be properly theoretical is by being deducible from individual optimization of conventionally individualistic objective functions, subject only to conventional technical and legal constrains. Mind you, I sometimes think the model builders are too <u>ad hoc</u> just as I often think the other chaps are not <u>ad hoc</u> enough. But it will be a sad day when open-mindedness is a sin against theory.

In his last overview of the "The Mechanisms of the Business Cycle in the Postwar Era," written with his long-time collaborator Allen Sinai, the descriptive-statistical use of the model is perhaps dominant. The approach is reminiscent of Burns and Mitchell's <u>Measuring Business Cycles</u> in its appeal to the comparative morphology of separate "business cycles." The model itself is used mainly to try to isolate the destabilizing or stabilizing influence of particular exogenous events or endogenous loops. Unfortunately "the model" remains opaque to the reader. One can say that this is inevitable: the world is very complicated.

My own inclination is always to want to narrow the scope, to try to understand one relation at a time, to stick to the few strong stylized facts that will likely survive any change in angle of vission, to use evidence from any source, even casual observation, and not only from econometric routine. That recipe would have been far too unambitious for Otto, but we skeptical types have to live with our own ambivalences.

I have been talking entirely about Otto's influence on macroeconomic modelling. On that score, the size of our loss is unmistakable. His friends have lost even more. Otto was one of those rare people whose lives are a standing counterexample to Leo Durocher's Law that nice guys finish last. For all his success, intellectual and worldly, Otto remained the sweet, friendly, helpful, unassuming person he was from the very start. He lit up a room -- hard to do when it's full of economists -- in his own, usually half-smiling half-serious way. There is no one to take his place.