increased interest bills. Excessive reliance on short-term bank financing (as opposed to bonds and direct investment) put the third-world borrowers in a severe squeeze, which was in no way relieved by the highly inflationary policies followed by most of them. Although a temporary solution to the debt problem may be in sight, in the longer run it can only be overcome by an improvement in global economic performance. The developing countries are potentially the most dynamic element in the world economy; it would be most unfortunate if they had to curtail their growth because of merely financial difficulties.

The third item on the international agenda is the speed of protectionism, in which the U.S. is unfortunately the leader. The high value of the dollar permitted by the floating-rate regime has disrupted several of our industries. Instead of dealing with the underlying cause, the Administration and the Congress have responded to their complaints by introducing more and more trade restrictions, thus setting a bad example to other countries where protectionist pressures are also strong. Much of the progress of the last twenty years towards freer trade is in danger of being undone. A commitment to sound macro-economic policies, and perhaps to less flexible exchange rates, is needed to re-establish a climate in which international trade can flourish again.

To sum up, the world economy is not in good shape and the economic policies of the leading countries are in disarray. There will no doubt be disagreement with my tentative conclusion that the adoption of floating rates is to blame. I hope that those who disagree will show how countries can improve their policies to make better use of the opportunities created by floating rates.


REFLECTIONS ON MACROECONOMIC MODELLING; CONFESSIONS OF A DRI ADDICT

Robert M. Solow

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 summing 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 chains 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 anatomy of the large-scale complete macroeconomic model of today. That was 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 simple 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 had indeed deteriorated, and so on.

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When I mentioned the idea to one of my colleagues, a knowledgeable and distinguished economist, he pounced on it for an interesting reason. He argued that precisely all such empirical work in economics is carried on in conjunction with one of the large macroeconomic models. That was not only the efficient way to check and preserve economic work; it was already being done in such organizations as DRI. Since at that time I always believed 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 economist believed only a dozen or so years ago that the ongoing large macroeconomic 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 macroeconomics. 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 exogenous 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 model. The written documents have the air of telling us what 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 reason for my DRI addiction is quite different. One of 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, or what will happen if you change your mind about capital taxes. This 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 been achieved universal acceptance or universal rejection as a result of econometric testing. Instead, I think, the main function of econometric modelling is merely 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 y and z tend to move together or in opposite directions, net of the
influence of x. 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
prior, 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 unimportant 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 carved up in the form of the response repertoires 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 clipping to statistical significance by their fingertips, if at
all, and then only by virtue of some particular choice of functional form, sample period,
or other causal 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 confirm with a
reasonable person's understanding of the economy, and also to events that look anomalous
given one's expectations about the way the world works.

That the DRI model can be read that way reflects its eclectic character. No doubt
the model inherited its undeniably characteristic from Otto. You may remember that a
couple of years ago, Mario Foder described one well-known persuasion within your
profession as practicing "light-prior economics." He intended that as a sympathetic
characterization, though I suppose the point was that the lighter the prior, the more
imprecise it is to facts. No one could say that 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, when the hell, let's
throw 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 constraints. Mind you, I sometimes think the model builders are too good here just
as I often think the other chaps are not good 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
Measuring Business Cycles in its appeal to the comparative morphology of separate
business cycles. The model itself is used mainly 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 because 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 vision, to use evidence from my own source, even casual observation, and
not only from econometric routine. That 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 modeling.
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.