

## **ECONOMETRICS FOR POLICY ANALYSIS: PROGRESS AND REGRESS**

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### 1. ORIGINS OF THIS TALK

- (I) What I wrote a few years ago (1996) in “Macroeconomics and Methodology” — I don’t want to rehash that.
- (II) Time I spent last year visiting central banks and interviewing people there about what econometric models they use and how they use them.
- (III) Recent technical developments that have converted theoretical advantages of Bayesian over classical approaches to inference into practical reality in some applied areas. Associated applied work and methodological commentary emerging in the literature.
- (IV) Haavelmo’s 1944 paper/monograph “The Probability Approach in Econometrics”, and some related previous literature.

We are going to begin by discussing (IV), using it as a kind of table of contents for aspects of (II) and (III).

### 2. THE ROAD AHEAD, AS OF 1939

- The fault line separating “numbers men” from theorists.
- Model flaws
  - Minor, or “picky” (but still with us)
  - Major, still, and perhaps always, unresolved
  - Major, very substantial progress since 1939
  - Back to square one

### 3. THE FAULT LINE

To undertake what Tinbergen did in the 30’s, constructing a quantitative model of the economy based on tools of statistics then available, required audacity, energy, and a certain personality type. This personality type was not that of Keynes. He closes his review of the first volume of Tinbergen’s League of Nations modeling effort with

*I hope I have not done injustice to a brave pioneer effort. The labour it involved must have been enormous. The book is full of intelligence, ingenuity, and candour; I leave it with sentiments of respect for the author.*

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But it has been a nightmare to live with, and I fancy that other readers will find the same. I have a feeling that Prof. Tinbergen may agree with much of my comment, but that his reaction will be to engage another ten computers and drown his sorrows in arithmetic.

#### 4. THE MINOR ISSUES

Keynes weakened his critique by treating as equally weighty problems that we now agree were serious problems in principle with Tinbergen's approach and problems that were minor, or at least a matter of degree than of principle. For example, he complained at length about

- The linearity assumption.
- What he saw as the requirement that *all* true causes of the left-hand-side variable appear on the right in a regression, and that there be no double-counting.

Tinbergen answered these components of Keynes's criticisms effectively. On the latter point, an uncharitable reading of the Keynes review might suggest he did not understand how multiple regression works.

Nonetheless, a similar pattern of mixing minor issues — even these same minor issues — with more fundamental arguments about applied econometric work, are observed even today, for example in some critiques of the structural VAR literature. When a certain approach inspires revulsion in its critics, it may tend to subvert their rhetorical effectiveness by making them unresistant to the temptation to pile trivia on top of substance in their critiques.

#### 5. THE MAJOR ISSUES STILL WITH US

- The problem of “bad data” or of “castles in the air theory”. The variables in formal theories are almost never a perfect match for data series with the same name. There are always possibly important causal mechanisms on which we have no data. Keynes and Tinbergen both saw this, Haavelmo articulated the point at length, and the situation has not changed much.
- How can we ever trust conclusions from a model whose explicit accompanying behavioral stories are known not to be literally true? Keynes knew there was no behavioral story he could believe that would precisely justify Tinbergen's specification. Tinbergen didn't think this a decisive objection. RBC modelers know that their general equilibrium models with small numbers of agent types are not literally true. They don't think this a decisive objection.

#### 6. THE MAJOR ISSUES ON WHICH THERE HAS BEEN GREAT PROGRESS

- Expectations
- Simultaneity
- The probability approach

## 7. EXPECTATIONS

After listing variables that Tinbergen takes to explain investment, <sup>?</sup>, f.n.1,p.563 Keynes observes, "I should like to have said 'the *expected* rate of profit'. But there is no room for expectations, so far as I can discover, in the theory of investment with which the economists have supplied Professor Tinbergen."

Haavelmo in his <sup>?</sup> presidential address to the Econometric Society, argues that econometricians have a responsibility to take a stronger role in the direction of economic theory. One example he gives is expectations; he appears to believe that econometricians, working with data, are strongly aware of the importance of expectations, and that (in 1958) they should push theorists to consider theories in which expectations appear explicitly, as well as push statistical agencies to collect better data on expectations.

(His other example might be read as a suggestion in 1958 to better develop the field of behavioral economics.)

## 8. EXPECTATIONS: PROGRESS

Whatever our current problems in macroeconomic theory, the lack of theories that give explicit treatment to expectations is not one of them. And the development of these theories, and the beginnings of matching them to data, are a major achievement, clearly only dimly foreseen even as late as the 50's.

## 9. SIMULTANEITY

Both Keynes and Tinbergen recognized the problem of simultaneity. Keynes included it under the heading "spurious correlation" in his critique, and thereby mixed it up with other, less fundamental, problems.

Of course Haavelmo in his <sup>?</sup> *Econometrica* paper showed the path toward resolving this problem, and he and the Cowles Foundation worked it out in subsequent years. This was again a major advance over what either Tinbergen or Keynes understood in 1939.

## 10. THE PROBABILITY APPROACH

Haavelmo wrote his 118 page 1944 on this topic in an atmosphere where the application of probability-based statistical methods to economic models was a controversial idea. His argument, briefly:

- We use models in economics that do not make precise predictions that allow rejection of the theory based on precise measurements.
- If our models are to represent science, they must be confronted with data.
- We need therefore to provide commonly accepted principles by which we weight errors, treating some as unimportant or expected, while others are treated as casting strong doubt on the model. He argues that this is always implicitly a matter of putting probability distributions on the errors, and ought to be explicitly so.

### 11. THE PROBABILITY APPROACH AS PROGRESS

- Keynes concluded that Tinbergen's modeling effort could *at best* merely give "quantitative precision to what, in qualitative terms, we know already as the result of a complete theoretical analysis." He concluded therefore that "the method is one neither of discovery nor criticism."
- Keynes could not see how fitting a model could ever result in producing evidence about the validity of a theory!
- Tinbergen knew this was not correct, but did not have a formal, articulated, counterargument.
- Haavelmo embraced Neyman-Pearson theory in part because it so clearly answered this objection of Keynes (and similar objections by others). Econometric analysis of time series data could result in formal tests of theories.

### 12. REGRESS

- Expectations: not a complete collapse
- Simultaneity: almost a complete collapse
- The probability approach: under siege

### 13. SIMULTANEITY

- None of the four central banks I visited (US FRB, ECB, Bank of England, Swedish Riksbank) use as the primary model one in which estimation takes account of simultaneity. The same is true of the major commercial macro models.
- The topic occupies a much smaller part of the econometrics curriculum in most graduate programs than it did 30 years ago, and many economics graduate students have a weak understanding of the subject — the general principles, not just the technical details. That is, they avoid research topics or theories that seem to lead to the need to analyze and fit a model that has multiple equations and shows simultaneity.

### 14. EXPECTATIONS

- Graduate students in macro these days certainly learn how to handle several classes of models (though often only small ones) that include expectations.
- Only two of the four banks I studied have models that claim to allow for assuming "model consistent expectations". But these models include expectations in ways that few academic macro theorists would defend. Because they are based almost entirely on single-equation inference, they ignore from the start one of the first-noted implications of the rational expectations approach: RE implies cross-equation restrictions and precludes equation-by-equation approaches to estimation and equation specification.

### 15. THE PROBABILITY APPROACH

- Much of what Haavelmo wrote on this could be lifted from his 1944 paper and applied directly to the dispute today between econometricians and “extreme calibrationists”. By the latter I mean economists who would claim that calibration, i.e. inference without formal appeal to probability-based statistical methods, is not just an occasionally, arguably, necessary expedient when probability-based inference is too complicated, but instead an improved replacement for probability-based inference.
- In other words, there is serious dispute today about Haavelmo’s argument. The dispute is no doubt greater today than it was in, say 1965.
- The fact that the models in use at central banks use single-equation estimation methods reflects a flaw at a deeper level: They are not formulated as, and never tested as, models for the joint time series behavior of the data used to estimate them.
- In other words, we have gone from a day when policy-oriented macroeconomic modelers tried to apply the probability approach, as in the 60’s, to a day when the most important policy macro models in actual use have completely forsworn it. They have gone back in many respects to Tinbergen’s single-equation modeling approach.

### 16. HOW DID WE REACH THIS POSITION?

- Some of the blame has to go to the Luddites of the Rational Expectations Revolution. Use of quantitative models as a guide to real-time policy advice was cast into such deep disrepute that academic research on the topic nearly completely ceased. Real-time policy advice was still needed, and policy makers still wanted staff to help them understand what recent data implied about their policy options, so quantitative macro modeling continued, always in the time-pressured atmosphere of actual rounds of policy advice and with little cross-fertilization from academia.
- Some of the blame must also be placed at the doorstep of Neyman-Pearson theory and Haavelmo’s wholesale swallowing of it.

### 17. HOW DID NEYMAN-PEARSON THEORY CONTRIBUTE?

- It envisions science as a process of sifting through theories, rejecting one false one after another until the truth crops up (after which only one out of 20 published papers rejects it).
- It therefore is silent when many theories are “unrejectable”, as well as when all the interesting theories (say DSGE models) are rejected in favor of an uninteresting (say reduced form VAR) model.
- In pursuit of an “objective” theory, it insists on never putting probabilities on what is labeled as the “parameter space”, or on competing models.

- It has no mechanism for introducing prior probability beliefs, even when these are of a standardized form, “objective” in the sense that they downweight portions of the parameter space that nearly everyone agrees are uninteresting.

#### 18. SOURCES OF DECLINE FOR THE PROBABILITY APPROACH

- It was obvious to RBC modelers as they started their enterprise that the notion that theorists hand econometricians candidates for “the true theory”, and that the econometricians then have the job of returning the message “accept” or “reject” at the 5% level, made no sense for what they were doing.
- Decision makers naturally use the language of probability to characterize and discuss major sources of uncertainty. They want to discuss the weight of evidence across models or across values of key parameters, not which of the models or which sets of parameter values are “accepted at the 5% level”. [Productivity growth example.] More than one central bank staffer, at more than one central bank, gave me almost exactly this argument for why “econometrics” could not be brought to bear in actual discussions with policy makers.
- Simultaneous equation theory, when applied to large macro models, seemed to say “apply 2SLS or 3SLS”, or “apply FIML or LIML”. If all instruments suggested by the model structure were used, 2SLS and 3SLS tended to be identical to, or nearly identical to, OLS. In models this big, FIML and LIML tended to be “ill-behaved”. Though there were ad hoc resolutions of these difficulties, academic econometric research paid them little attention, and results of careful attention to simultaneity with existing tools tended to be close to OLS, but at much greater computational and staff time expense.

#### 19. HOW BAYESIAN THOUGHT PATTERNS CAN HELP

- In the many-instruments, few degrees of freedom environment, use of weak priors that rule out bizarre corners of the parameter space can regularize the problem.
- Integrating the posterior, as suggested by a Bayesian approach, is likely to avoid some of the difficulties created for maximum likelihood by sharp, isolated peaks in the likelihood.
- Of course Bayesian approaches lead directly to analysis of how data and prior affect posterior odds across models and posterior distributions of parameters. This fits with the natural language and thought patterns of both decision makers and econometricians. Indeed here is Tinbergen, trying to explain to Keynes what can be learned from Tinbergen’s model fitting:

certain details on the probability distribution of the [explanatory variables] ‘influences’ can be given. These details are the central (most probable) values and the standard deviations of the regression coefficients, measuring the ‘influences’.

## 20. BAYESIAN DSGE MODELING?

- Bayesian thinking does not help very much if all the interesting (DSGE) models available have very low posterior odds, while reduced form VAR's have high posterior odds.
- Contrary to assertions one sometimes hears, a Bayesian viewpoint does not comfortably handle choice among models *known* to be false. This misconception arises because a Bayesian viewpoint does help guide analysis and decision making when there is uncertainty, given the data, about *which one* of several models is correct.
- My own view is that when it is clear that all the interesting models are false, it should nearly always be possible to diagnose their faults and get them to fit better. But if there really is no possibility of doing this, it may still be possible to do something useful in analyzing fit. Schorfheide (1998) has interesting analysis of these issues.

## 21. RECENT REASONS FOR OPTIMISM

- Work by Chib, Geweke and others on importing into econometrics the Markov Chain Monte Carlo methods applied successfully in other fields has made possible complete Bayesian analyses of models for which that would have been impossible a few years ago.
- This approach relaxes the constraint on model complexity somewhat, so that DSGE models that tell appealing stories about behavior can at the same time be complex enough to fit the data about as well as the best Bayesian reduced form VAR's. Smets and Wouters (2002) have given us an example of exactly this situation.
- While I disagree with some of its specific arguments, recent work on the relation of econometric modeling and model choice to policy analysis, for example that by Brock, Durlauf, and West (2003) and Leeper and Zha (2001), as well as by Schorfheide, suggests that the Neyman-Pearson framework may be loosening its grip.

## 22. WHAT DISTINGUISHES "BAYESIAN THINKING" ABOUT INFERENCE?

- Recognition that the aim of analysis of data is to guide decision making, and that this will implicitly or explicitly involve putting probability distributions across models and parameters.
- An emphasis on characterizing the shape of the likelihood function in scientific reporting of data analysis, with standardized priors used where they help in doing so.
- Not a failure to distinguish "subjective" from "objective" probabilities.
- Not an insistence on producing numerical conclusions, even when "reserving judgment" might be more reasonable.

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