

# MACROECONOMICS *and the* REAL WORLD

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# 10A Models all the way down: comments on Smith and Juselius

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## 1. Econometrics in different keys

Before the Bergamo Conference, I had never met Katarina Juselius or Ron Smith. So, when I was reading their papers for this volume, I amused myself trying to guess from the tone and style of their papers something about their ages, temperaments, and life experiences. For though their papers are in many ways similar and derive from what I imagine to be a common intellectual background, their tone and ambitions are different. I found much to admire in both papers and, indeed, much to agree with.

Both papers are too rich for me to react to all that I found notable. Smith is to be congratulated for the way in which he lays bare the connections in the development of unit roots, vector autoregressions (VARs), Granger-causality, and cointegration. His account, as he notes, is ahistorical—but no less useful for that. Equally, I admire the intricately detailed account that Juselius gives of the formulation and interpretation of cointegrating VARs. It is a model of thoughtful conceptual preparation for empirical investigations that could be profitably studied by any applied macroeconomist.

The central message of Smith's paper is that there is a mismatch between statistics and economic theory. The result is a glib appropriation of statistical results to support economic conclusions, even when the statistical tests do not adequately map onto the economic notions. Smith rightly stresses the importance of developing sensible economic interpretations of statistics and of using a variety of sources of information (qualitative, historical, institutional), while noting that the more qualitative sources may suffer from parallel interpretive problems. But, aside from the passing suggestion that we might all be better off to think hard about the structural econometrics of Haavelmo and the Cowles Commission, he offers little practical guidance on how to get the interpretive project going. While he says that he is optimistic, there is a note of world-weariness in Smith's tale.

Juselius's tone is the opposite of Smith's. She too sees the gap between theory and statistics and argues that statistics must enter in at the ground level and be developed into a set of constraints on theorizing. She then proceeds to give detailed examples of what she has in mind. There is a cheerful optimism in her tale.

## 2. The theory–data gap

One common feature of their papers is the focus on the gap between theory and data. The problem is that econometrics packages and describes data, but descriptions, in

themselves, are not useful. Economics needs and seeks—directly or surreptitiously—structure, where *structure* can be defined broadly as those enduring features of the economy that stay put in the face of interventions, manipulations, shocks, and so forth. It is the yearning for structure that recommends to Smith the econometrics of the 1940s (Haavelmo and the Cowles Commission). As Mary Morgan's (1990) history of econometrics or David Hendry and Morgan's (1995) more recent anthology of essential original works in econometrics (mostly from before 1950) demonstrate, these issues have an even longer history in econometrics than Smith suggests.

Juselius despairs of defining structure adequately and retreats to the claim that, even taken descriptively, a statistical analysis of common trends is useful. Similarly, Smith states that unidentified VARs are good enough for unconditional forecasts. This does not seem right in either case. The only use these descriptions could have is based on the notion that the future will be like the past. Even if our descriptions do not permit the articulation of structures, we must appeal to the notion of structure to warrant any *use* of the statistics economically. Take a trivial example. We can always generate a descriptive statistic such as the mean age of the people in a conference room. But to what use can we put that statistic if we are unable to relate the occupants of the room to larger groups of people. If the people are all economists and, we believe, randomly chosen, then we might extrapolate to the mean age of all economists. If they are all adult men and randomly chosen, then we might extrapolate to the mean age of all adult men. It is the extra-statistical information that makes the statistic interpretable and useful. Without it, it is just a number.

### 3. Structure and the problem of induction

When stripped down to its barest form, the problem has a much older history, having been formulated at the dawn of modern economics by David Hume. It is important to recall that Hume was both an economist and a philosopher (as well as a historian and general man of letters). Both his philosophy and his economic essays ought to be read (Hume 1739, 1742, 1777). The economic essays, indeed, would help to temper the more radical readings of Hume as a sceptical philosopher (see Hoover 2001: ch. 1).

Hume took data (i.e. sense data) to be fundamental (the foundation of knowledge). As actors, however, we seek necessary connections, causes, and effects—that is to say, *structure*. Hume is famous for his attack on induction. In this context, his attack can be reformulated simply: data do not wear their structure on their faces. In one sense, this is the end of the story: Hume is right, data alone do not reveal the economic structures that generated them. His own solution was to locate necessary connection in the customary regard that we feel for sequences that are repeated frequently enough. We expect that the glass *must* fall to the floor when tipped off the table because it has done so every time we can recall in the past. Hume has no doubt that we reason this way. But he believes that it is irrational and that the 'must' indicates nothing more than our familiarity with glasses and other objects falling in particular circumstances.

Hume's problem is still with us. And in economics we have adopted something like Hume's solution to it. In order to estimate and test structural models we make a priori identifying assumptions, which are grounded in a kind of professional custom about what is reasonable or what is derivable from theory. That custom itself has a customary basis. Is this a solution? Or is it standing naked whistling in the dark? Christopher Sims underlined the problem in his 'Macroeconomics and Reality' (1980): the identifying assumptions typically employed are literally incredible.

The first thought twenty years ago was that we could get everything we needed out of the unrestricted VAR. But it was soon shown (as both Juselius and Smith recount) that anything useful in a VAR required, at a minimum, an identifying assumption for the Wold-causal ordering of the contemporaneous variables. The data really do not wear their own structure. But which causal order? There were endless debates. Another possibility is discussed by both Juselius and Smith—namely, that cointegration relations provide identifying assumptions. Even though zero or other exact restrictions on parameters are unlikely to be credible, coarser restrictions might be plausible. Again, which can be taken as acceptable a priori is disputed. Juselius implicitly endorses the typical assumption that shocks to nominal things do not have long-run effects, although they may have short-run effects, on real things. (This is another assumption shared with Hume, a long-run quantity theorist who recognized short-run exceptions.) Juselius renders this assumption in the form of independence of aggregate-supply shocks from aggregate-demand shocks. Smith, however, attacks this independence assumption as implausible. The emperor has no clothes. Do we have any to give to him?

#### 4. The role of models

Both Juselius and Smith highlight a gap between data and theory. The central methodological problems that they pose are why that gap exists? And what would one do to fill it? The answer, I suggest, to both questions is, *models*. There is a gap because we deal not with the structure of reality but with models of it, and the only way to fill it is to take modelling seriously.

Morgan's (1990) *History of Econometric Ideas* identifies two distinct streams of development. The business-cycle stream started with the business-cycle barometers and other atheoretic descriptions of macroeconomic aggregates and developed over the century into time-series analysis in the Box-Jenkins or unrestricted-VAR tradition. The demand-analysis stream started with a maintained demand theory and attempted to measure the actual demand elasticities. These streams partly merged in the 1930s and 1940s in the work of Frisch, Tinbergen, Haavelmo, and the Cowles Commission, and redivided to form various schools of econometric practice. One of these streams is the standard textbook view in which theory proposes and estimation tests and disposes. This apriorist approach has not been a great success since the theories are rarely rich enough to do justice to the complexities of the data.

I once suggested that we should oppose this 'econometrics-as-measurement approach' to an 'econometrics-as-observation approach' in which econometrics would

be regarded as generating observations, through its various ways of filtering the data, that it was the job of theory to explain. Just as with photographs through telescopes, some observations would be informative, others less so. Juselius's paper convinces me that econometrics too deals exclusively in models. Perhaps this should not have been a revelation, since the phrase 'econometric model' just trips off the tongue, but her paper brings out clearly how much even characterizing the data depends upon untested and untestable assumptions.

I am reminded of the famous tale of the cosmologist who is challenged by an old woman. She pooh-poohs his explanation of the structure of the universe. Everyone knows, she says, that the world sits on the back of giant turtle. The scientist replies, 'But, Madam, on what does the turtle stand?' To which the old lady says, 'Young man, you think that you are very clever, but the turtle stands upon the back of another turtle. It's turtles all the way down.' So too in empirical macroeconomics. It is not just that we have theoretical models; it's models all the way down.

Even the 'raw data' themselves are models in the sense that substantive choices are made that are not inherent in the data. For example, Juselius cites the well-known fact that velocity is  $I(1)$ ; but in the USA this appears to be true for M1-velocity, while M2-velocity appears to be  $I(0)$ . The choice is part of the econometric model of money.

Where does our confidence in models come from? This is a vexed question. Smith laments the use of statistics, say, Granger-causality tests even when the needed sense of causality is not Granger's sense of incremental predictability. I suggest that the practice of using them this way is in fact a disguised appeal to authority—a fallacy, but one with strong psychological appeal to those whom doubt might otherwise immobilize. The difficulty goes right back to Hume and needs to be nipped in the bud. Hume is wrong to regard sense data as foundational. The eyes do not see and ears do not hear and then interpret the data; they see and hear interpreted data. We can be brought to doubt the evidence of our own eyes and ears, to doubt the natural interpretations that thrust themselves on us, but it is self-defeating to start with the premiss that they are misleading.

En route to the Bergamo Conference, I saw 'The Truman Show' in which Jim Carrey's character is brought up without his knowledge in a completely staged environment, complete with artificial weather and actors for friends and parents. Though he comes to doubt the reality of his world, it would not have been sensible for him to have doubted such a perfect fraud from the beginning. He discovers the truth by noticing incongruities and attempting to bring a reasonable order to his model of how the world works. So too in economics. There are always assumptions that we do not really doubt and that others share. We start from them and use them to place limits on the possible classes of acceptable economic models and acceptable statistical models of the data. There is an interplay on these margins, and it may force us sometimes to abandon a class of models. Smith credits Nelson and Plosser's time-series models and unit-root tests of US macroeconomic data as having forced macroeconomists to abandon the Lucas surprise-only aggregate supply function. (I do not think that Nelson and Plosser did this single-handedly. It does not, however, change the story to recognize that the cumulative weight of incongruities isolated by Barro and others, as

well as Nelson and Plosser, finally led the mainstream opinion in macroeconomics to drift away from, rather than decisively reject, the Lucas supply function.)

The process through which statistical evidence affects theoretical commitments does not look much like the textbook pattern: deduce testable conclusions from a theory and then test them. That notion is not so much wrong as limited, itself requiring a large number of unsupported assumptions to make it work in special cases. The model is not so much testing and disposal as it is one of relentless criticism and mutual adjustment. And to demonstrate that these are not merely empty slogans, one need only look at Juselius's careful attempts to work out the relationships between non-stationary characterizations of data and the possible theoretical models that might be consistent with them.

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