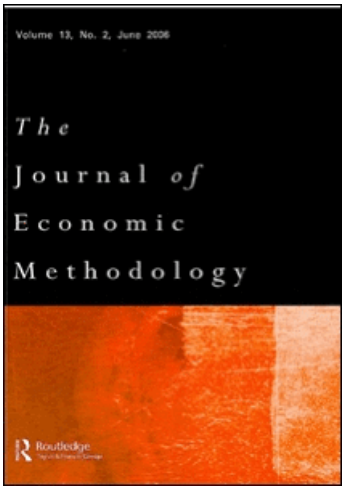

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When is a model like a thermometer?

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BOOK REVIEWS

When is a model like a thermometer?

How economists model the world into numbers, by Marcel Boumans, London and New York: Routledge, 2005, xiv + 206 pp., £90 (hardback), ISBN 0-415-34621-5

Measurement – the assignment of numbers to phenomena – is a characteristic, if not the most characteristic, activity of the physical and life sciences: what is Planck's constant? What is the tension in the San Andreas fault? How far away is the Andromeda galaxy? How much did the mean temperature of the earth rise in the twentieth century? The importance of measurement is reflected in such facts as the US National Science Foundation giving grants specifically targeted to instrumentation. Marcel Boumans' *How Economists Model the World into Numbers* is a provocative and original investigation of the analogous activities in economics.

Some time ago, economic methodology, like the philosophy of science more generally, took a naturalistic turn and began to pay greater attention to the actual practices of economics – what economists do and why they do it according to their own lights rather than what, on the basis of detached speculation, methodologists think they ought to do. Boumans' book to a large extent represents an extension of naturalism to the most basic areas of applied economics. Such an investigation requires deep immersion in practice, but it is treacherous to attempt to master current practice and simultaneously take a detached view of its methodology. Boumans elects instead to pursue a series of historical case studies – the history of science itself has taken a naturalistic turn, away from the succession of doctrine (think of Schumpeter's *History of Economic Analysis* or Blaug's *Economic Theory in Retrospect*) toward a history of practice. Boumans' cases cover key episodes in the history of the practice of empirical macroeconomics in the twentieth century – in chronological, albeit not his, order of presentation: Fisher's construction of index numbers for the study of monetary economics; Tinbergen's business cycle models of the 1930s; the development of the structural equations approach to econometrics in the 1940s; the debates over the assessment of its applications to macroeconometric modeling in the 1950s; the introduction of rational expectations business cycle models and the related real business cycle models in the 1970s and 1980s.

There are advantages to such an historical approach to methodology. Boumans has written a rich, deeply informed and insightful book. History provides access to the practice of economics that may be hard to come by otherwise. It displays greater variety and more contrasting possibilities than are easily seen at the cutting edge of contemporary economics. Yet, there is some loss from not focusing on current problems. And the book falls short of being a systematic treatise on measurement in economics. The history frequently dominates the methodology, and it is the nature of history that its lessons are not always unequivocal.

The thermometer serves as Bouman's paradigm of a measuring instrument. In a well-known paper, Hasok Chang (2001) describes a central problem in the history of metrology – establishing the standard thermometer. We sense roughly that materials

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expand when they become hotter. To quantify that relationship, we need temperature measurements, but those measurements rely on the very relationship that we wish to quantify. The expansion of their primary media (mercury, alcohol or a gas) is how thermometers measure temperature, and the differential between the expansion of the measuring medium and its containing materials (e.g. glass) affects the accuracy of the measurement. There is a nest of circularity problems. We require some material to expand linearly; yet we cannot prove the linearity of our target material without *assuming* the linearity of some other material. We cannot judge the inaccuracy induced by the expansion of the containing material without a standard of measurement; but any practical standard is itself subject to inaccuracy induced by its containing material.

The gas thermometer was ultimately adopted as the standard. The solution relied on the pragmatic judgment that gas expands with rising temperature many times more than glass, so that its expansion could be neglected. Circularity cannot be avoided, so in place of a natural standard, metrologists focused on consistency as the primary virtue: different tokens of the same thermometer type should report the same temperature at calibration points (for example, at the freezing and boiling points of water in standard conditions). Linearity of the thermometer is acknowledged as a convention, whose primary virtue is the simplification of laws in which temperature plays a part. A consistency standard does not eliminate circularity, but it tightens the circle substantially.

What are the instruments of economics, equivalent to thermometers in physical sciences? Boumans' most striking assertion is that they are mathematical models. The idea of the mathematical model is a relatively new one in any science. As Boumans points out, mathematical models until the second third of the twentieth century mainly referred to physical representations of mathematical structures, such as sets of wooden blocks that illustrated different spatial curvatures (see Figure 6.1, p. 154).[1] Economics was a pioneer in using mathematical models to represent the world.

Boumans' understanding of the role of models is deeply indebted to Morrison and Morgan's (1999) notion of *models as mediators* between data and theory. They stress that models have a life of their own: models are substantial (as opposed to formal) analogies of real phenomena, but they are not necessarily a representation of a specific theory, and they are constrained by features that may have no analogue in the target phenomenon at all. A good example is the Phillips machine, a physical model of the British economy, constructed by A.W. Phillips in the late 1940s out of plastic tubing, tanks, pumps and colored water. The model was informed by Keynesian principles, but constrained by facts about the economy and by principles of hydraulics unrelated either to the economy or to economics. Purely mathematical models are similarly constrained by functional forms, tractability, computational power, and other considerations unrelated to economic phenomena.

Morrison and Morgan's general view seems about right. Models are often instrumental to relating theory and data. Boumans, however, notes that models serve a variety of functions, and he advocates a broader conception in which models serve as a forum in which theoretical notions, mathematical concepts and techniques, stylized facts, policy views, analogies and metaphors, as well as empirical data are brought together. His view acknowledges the breadth of factors at play in modeling, while making their instrumentality somewhat murky. In his initial discussion in Chapter 1, Boumans shifts the focus to how the model is built rather than on how it functions as a measuring instrument. We do get to see models at work later, but the lessons are less systematically drawn.

The distinction between theory and model is a fraught one. It has been drawn in so many different ways that one doubts that there will ever be an agreed, consistent usage. Boumans treats theory as unquantified and even informal insights, which when made more

formal and quantified are embedded in models. Thus, much of what runs under the color of 'economic theory' in the journals would constitute models for Boumans. And apparently, any of them that are quantified with reference to actual economies would constitute measurement. The Phillips machine was used to illustrate economic concepts and, apparently, as an analogue computer to calculate counterfactual policy experiments. As such it needed to be adjusted to reflect measured properties of the British economy. But was it a measuring instrument itself? That seems doubtful. Where did the parameter values come from that were used to calibrate the Phillips machine? We seem to need a prior account of economic measurement.

The best example of the required notion of economic measurement is Boumans' account of index numbers in the last substantive chapter. Index numbers clearly parallel his discussion of the thermometer. Boumans rejects axiomatic index-number theory. Index numbers achieve formal consistency only when they are too denatured to do real work for empirical economics. Instead, Boumans advocates Irving Fisher's compromise among inconsistent criteria in which the pragmatic requirements of empirical research dictate the particular compromise, which may in fact be different in different cases. Boumans clearly admires Fisher's horses-for-courses, pragmatic methodology, which stresses empirical relevance over *formal* rigor. (He defends the notion that there is also a sound concept of empirical rigor.) The dominance of empirical relevance is a deep theme that is equally reflected in his admiration of Tinbergen.

Index numbers do not offer much support for Boumans' view of models as measuring instruments. Models play little role in index numbers. And where they do, as with cost-of-living indices based in utility theory, Boumans is skeptical. Like thermometers, index numbers show considerable independence from particular theories or models in which they might be employed – post Keynesians and new classicals may both use the same consumer price index in their starkly different models, grounded in contradictory theoretical understandings. Thermometers and index numbers rest on a few (and even vague) theoretical notions involving short inferential chains. Their value is in their consistent relation to the world (e.g. the calibration points of the thermometer), their service in rationalizing (a variety of) theoretical notions, and the fact that the information that they generate can usefully be carried from context to context. In this way, index numbers are like other types of data processing: the use of filters (which Boumans discusses in some detail in Chapter 5), averaging, interpolation, seasonal adjustment and so forth.

Boumans draws a distinction between observation and measurement and cites favorably the apparently closely related distinction of Bogen and Woodward (1988) between data and phenomena. These distinctions highlight an ambiguity in the book. Boumans often talks as if measurement is about assigning numbers to phenomena – the object of theory being to characterize phenomena conceptually. Yet some of the deepest inquiries in the book concern the observational devices used to gather data. Boumans, for instance, provides useful discussions of observational distortions in maps, lenses, index numbers and filters. He is less successful in the discussion of the mapping between observational data and theoretical phenomena, which was the original target of the notion of models as mediators.

The best example of a model as a measuring instrument is presented in – unaccountably – an appendix to Chapter 5. Boumans recounts how Lucas (1973) sought to test his monetary-surprise, rational-expectations model of the macroeconomy through its implication that the trade-off between output and inflation should be negatively correlated with the variance of aggregate demand. Lucas's test operated under strong assumptions, which restricted the range of model outcomes far more than theory suggested they might be restricted in the world – a good illustration of the intrusion of model-specific

constraints – and which were not supported by the data. The literature ultimately relaxed so many of these assumptions that Lucas's model was no longer testable. Yet, if one accepted the weaker assumptions, the slope of trade-off in the revised model could be used to estimate the otherwise unobservable slope of the aggregate supply function. Lucas's model was transformed into a measuring instrument.

What is measured in the end is a phenomenon in Bogen and Woodward's sense. Yet, where as the rate of price inflation is measured directly, like the air temperature, the slope of the aggregate-supply function is measured very indirectly. Although the ultimate model vastly weakened Lucas's initial assumptions, it still rested on assumptions that reasonable economists might reject and reasonable models might violate. The 'measurement' cannot, then, be carried from model to model or context to context in the manner of a temperature reading or a rate of price inflation – rather it is model-specific. Models, like thermometers, face circularity issues. The Lucas example illustrates that, while the circle is tight and benign for a thermometer, it is large and contentious for many economic models.

Boumans is, of course, right that models and measurement interact in subtle and important ways. He is illuminating about many of them. Still, models and measurement are not identical. They are like characters in a story with tightly intertwined, though separate, lives. The differences might have been clarified had Boumans worked the data/phenomena distinction somewhat harder. Measurement is involved with both – more directly with data, more indirectly with phenomena, as his case studies of index numbers and the Lucas model amply illustrate.

Although hard to relate to thermometers, Bouman's investigation of the nature of empirical models is historically and methodologically rich. In his account of Tinbergen's business cycle models of the 1930s, Bouman's emphasizes what he calls mathematical molding – the repeated checking and reshaping of the model in light of the data, using whatever mathematical tools seem to work. Where later econometricians might have called this data mining and condemned it as unscientific, Boumans sees it as positively virtuous. No sculptor or architectural modeler would think of systematically avoiding interplay between the model and the modeled subject.

Methodological commitments, like politics, sometimes make strange bedfellows. On the philosophical side, Boumans relates mathematically molded models to Nancy Cartwright's (1983) simulacrum account of models, which itself has deep affinities with Lucas's notion that we understand the operation of the economy when we can produce a model the simulated output of which cannot be distinguished from the actual economy. Lucas operationalizes this notion, advocating the test of the Adelmans: give a business cycle analyst actual and simulated data; if he cannot tell them apart, the model adequately explains the business cycle. As Boumans notes, the Adelmans' test is closely analogous to the Turing test for machine intelligence.

Lucas's views on models are, in turn, related to Herbert Simon's account of the architecture of the artificial system and of his analysis of near decomposability. Hierarchical systems typically consist of elements that are tightly connected to some elements and weakly connected to others. A network of tightly connected elements may often be analyzed in relative isolation, neglecting the weak connections, and at other times treated as a black box, a distinct element of a larger system, whose internal workings can be neglected so long as one gets the right relationship between inputs to and outputs from the black box. Boumans is insightful in relating Simon's systems analysis to both Lucas's (and, subsequently to Kydland and Prescott and the calibration tradition's) simulacrum view of explanation and to Milton Friedman's Marshallianism.

Friedman is often viewed as advocating models with truly false assumptions, so long as the data act *as if* they were true. Boumans shows that this is more nearly the position of Lucas and his allies, who focus on the black box. The question of when the black box can be left alone and when one must look inside is related to Lucas's famous 'critique' of econometric policy evaluation.

Boumans treats Friedman as taking a quite different position, though one that suits Simon's analysis. Friedman is a realist who deals with the complex world not through as-if assumptions, deliberate falsehoods, but through negligibility assumptions (neglecting weak factors when the desired accuracy does not require taking them into account) and domain assumptions (using simplified models in the domains where they can be shown to be effective, while appealing to different simplified, or more complex, models in other domains). Simon's near decomposability underwrites Friedman's 'Marshallian partitioning, not by using a sharp *ceteris paribus* razor, but a blunt knife of negligibility assumptions' (p. 88).

Tinbergen, Simon, Friedman and Lucas – whatever their methodological differences – stand on one side; Ragnar Frisch, Trygve Haavelmo, the Cowles Commission and Lawrence Klein stand on another. In a chapter that goes a long way to explaining the history of modern econometrics, Boumans describes how Frisch and Haavelmo worried about how to isolate the true structure of the economy from passively observed data. The central idea was that if the model accurately articulated the causal structure of the economy, it could be used to measure the strength of the various causes. Causal articulation on this view had to be *a priori* and finds its source in economic theory. No route is provided for feedback from the data to the structure of the model; so Boumans locates the death of mathematical molding in the ascendancy of the Cowles Commission.

The story is more complex than Boumans' account suggests. Kydland and Prescott, deeply engaged in Lucas's as-if program, are nevertheless *apriorists*. And econometricians such as David Hendry, highly sympathetic to mathematical molding, take Haavelmo as a touchstone. Boumans nevertheless successfully isolates genuine methodological faultlines, even if particular economists sometimes straddle them in awkward ways.

In the end, Boumans' real theme is anti-Cartesian: science – and economic science – starts and ends in the world; *apriorism* cannot support an empirically successful economics. We need to understand how empirical models relate to the world – both the role of the world in shaping the models, of which measurement is an essential part, and the role of the model in representing the world, which supports measurement. If Boumans has not answered every question about the relationship of models and measurement or provided a systematic methodological account, he has, at the very least, provided the richest set of historical and methodological reflections on these issues now available. His book is an essential contribution to understanding empirical economics.

Note

1. All page numbers refer to Boumans' book.

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Rational economic man revisited

Rational economic man: a philosophical critique of neo-classical economics, by Martin Hollis and Edward Nell, Cambridge: Cambridge University Press, 2007, viii + 279 pp., £29.99 (paperback), ISBN 978-0-521-03388-6

Martin Hollis and Edward Nell's *Rational Economic Man* is a classic of economic methodology. First published in 1975, it now appears in its first paperback version. As the subtitle makes clear, the authors' intention is to present 'a philosophical critique of neo-classical economics'. They argue that neoclassical economics – the mainstream price theory of the 1950s and 1960s – is unsound, by virtue of its reliance on an unsound positivist theory of knowledge. In its place, they advocate a rationalist theory of knowledge and an economics based on *a priori* knowledge of necessary truths.

This is a difficult book to review. One problem is its age. It is too recent to be treated only as a historical text, but economics and philosophy of science have both moved on since 1975. Another problem is the style in which it is written. Some passages (presumably contributed by Nell) are rather flat treatments of textbook economics, but most of the prose is clearly Hollis's work. This material is densely but informally argued, with few explicit references to the literature that is being subjected to critique. Perhaps like the genial Oxford tutors he remembers (of which more below), Hollis assumes that his readers are familiar with the classic texts of philosophy of science and can follow intricate arguments from first principles. But (unlike the student in the tutorial) the reader cannot ask for clarifications of the authors' more obscure allusions, or for explanations of difficult argumentative moves.

The reader's task is made more difficult by the possibility that the contributions of its two authors are not fully integrated. We are told that Hollis is responsible for 'the main philosophical argument', while Nell 'takes sole praise or blame' for the economics. Although Nell justifiably claims to be a philosopher-economist, Hollis flatly denies any knowledge of economics beyond 'hazy recollections of genial hours with economics tutors' as a PPE undergraduate in Oxford (p. vii). This, I think, is not just modesty or self-deprecation on Hollis's part. In the substantively economic chapters, *Rational Economic Man* argues for a Classical-Marxian form of economics. Nell has consistently espoused this approach, but it has few echoes in Hollis's later engagements with economics and rational choice theory. However, the book also contains a more philosophically framed argument for grounding economics on *a priori* principles of rationality. This does not fit easily with the Classical-Marxian approach, but prefigures ideas that Hollis developed in his later work.