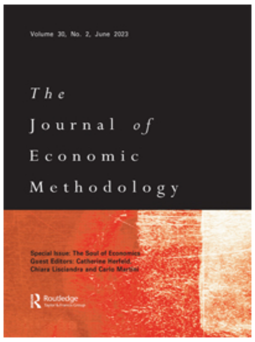

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COMMENT

The struggle for the soul of macroeconomics

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ABSTRACT
Critics argued that the 2007–09 financial crisis was failure of macroeconomics, locating its source in the dynamic, stochastic general-equilibrium model and calling for fundamental re-orientation of the field. Critics exaggerated the role of DSGE models in actual policymaking, and DSGE modelers addressed some criticisms within the DSGE framework. But DSGE modelers oversold their success and even claimed that their approach is the *sine qua non* of competent macroeconomics. The DSGE modelers and their critics renew an old debate over the relative priority of *a priori* theory and empirical data, classically exemplified in the Measurement without Theory Debate of the 1940s between the Cowles Commission and the National Bureau of Economic Research. The earlier debate is reviewed for its implications for the recent controversy. In adopting the Cowles-Commission position, some DSGE modelers would essentially straight-jacket macroeconomics and undermine economic science and the pursuit of knowledge in an open-minded, yet critical framework.

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I

The title of the conference, 'The Soul of Economics,' suggested something portentous. The conference marked the decade since the 2007–2009 financial crisis and its aftermath. In the words of Thomas Paine at the time of the American Revolution: 'these are times that try men's souls.' Very quickly in 2007, a feeling set in that something had happened more fundamental than the ebb tide of the usual economic cycle. 'What was it that had gone wrong?' and 'what would happen next?' were the immediate questions. And one repeatedly heard the hackneyed phrase: 'a crisis is a terrible thing to waste.' The crisis presented an opportunity – and to many people not simply an opportunity for reforming practical economic policy. Almost immediately pundits, politicians, ordinary people, and even some economists called for a complete rethinking of the foundations of economics – especially of macroeconomics. There was a variety of diagnoses: for some, the problem was how economics thought of human agents; for others, it was the economists' conception of equilibrium; and, for others, it was that economics failed to address real-world institutions. Proposed solutions were also various. Some called for a return to older, supposed better foundations for economics: John Maynard Keynes and his ideas on liquidity and uncertainty resurged, not just among economists, but in the popular press; and some economists began to talk of a 'Minsky Moment.' For others, economics needed to look, not to the past, but to the new ideas of behavioral or neuroeconomics, to agent-based modeling, or to other reconceptualizations. In macroeconomics, whatever their preferred direction of reform, the 'New Neoclassical Synthesis' and the Dynamic Stochastic General Equilibrium (or DSGE) model were prime targets. The criticisms carried a strong

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ideological overlay and often took the tone of Manichean struggle between the darkness of the status quo of mainstream macroeconomics and the light of some root-and-branch reforms.

II

The Manichean tone of these discussions struck me then and strikes me still as overwrought. In 2007 something went massively wrong in the economy – of that there can be little doubt – and when things go wrong, people seem to be hardwired to seek out villains. Among the favorite culprits were the venality and corruption of businessmen (and, I have no doubt that the crisis exposed a good deal of that, even if it did not cause it); the short-sightedness, self-servingness, and ignorance of politicians and regulators (and that too has considerable plausibility); and, for many, the failure of economics as an advice-giving profession.

This last was neatly encapsulated in the 'Queen's Question,' posed by Elizabeth II to a group of assembled economists in London: 'Why did no one see it coming?' This was a question on the lips of the public as well. But I believe that it reveals a fundamental confusion about what can be expected of any science – not just economics. My colleague Timur Kuran (1989) argues that political revolutions are always unexpected – perhaps not in a broad-brush construal of 'unexpected,' but in detail with respect to timing, precipitating event, and severity. He makes a kind of efficient-markets point. Governments have a strong incentive to anticipate and diffuse opposition and, therefore, to gather and analyze information to make conditional forecasts of challenges to their regimes, and then to take countervailing actions that ensure that such challenges are foiled. As a result, *successful* revolutions are the ones that blindside the government, the ones that they fail to nip in the bud. I suggest that a similar dynamic is at work with regard to economic policy. Governments have strong incentives to avoid economic disasters, and the ones that actually strike are the ones that economic advisors and policymakers fail to predict, given the knowledge and analytical tools available to them. As Monty Python put it, 'No one expects the Spanish Inquisition.'

No science makes unconditional forecasts. Our best understandings of economics tell us that there are no crystal balls. It makes no more sense to blame economics for not seeing the crisis coming than it does to prosecute – as happened in Italy – geologists for not predicting the timing and location of a devastating earthquake. Let me be clear, I am not claiming that economists have no role to play nor am I absolving them from trying to develop deeper knowledge and using it for the good. Geologists have developed ever deeper understanding of the Earth's inner working, and these have been of great help in the design of buildings, bridges, roads, and so forth that minimize the damage of earthquakes. Similarly, deeper economic knowledge may aid in better design of regulation and policy to minimize the effects of economic fluctuations. Just do not expect economics to end unforeseen economic earthquakes. The problem is that ignorance and uncertainty are fundamental parts of the human condition. The challenge is not to provide a crystal ball, not to provide unconditional predictions – that would simply be magic, not science. The challenge is to reduce and manage our ignorance. The challenge is hard – it is more like the challenge of geology than, say, of chemistry. Geology and economics deal with open, complex systems that are constantly evolving. The challenge of economics may be harder still, since – unlike rocks – people have minds of their own. Molecules display reliable patterns of behavior; people change and adapt their behavior to suit their own ends and often to thwart the actions of policymakers.

III

Although I have thrown cold water on some of the overwrought reactions to the financial crisis, I nonetheless embrace the conference title in my own title, 'The Struggle for the Soul of Macroeconomics.' But for me it is not a battle of good and evil nor of pacts with the Devil. It is a struggle over the metaphysics and method of economics, played out for the hearts and minds of rather ordinary, muddled, good natured, but not necessarily very reflective economists: more Homer Simpson than Dr. Faust.

More than a decade later, the hopes of the reformers of economics have largely been dashed. I am not a bit surprised. It is exactly what in many conversations at the time of the crisis I said at the time would happen. Much of the critical focus was on the DSGE model. The critics vastly overstated the role of the DSGE model in guiding practical policy. Actual policymakers are heterogeneous improvisers who draw on many sources of analysis and information. Sometimes they learn from it. Sometimes they use it selectively to reinforce their prejudices. In the end policy decisions are pragmatic and political, not necessarily principled or coherent. Still, the DSGE model was a central element of the economic mainstream whose voice was heard in the policymakers' conversation.

The DSGE model (or, more precisely models of the DSGE type) involve simple sets of dynamic, simultaneous equations connecting a few macroeconomic variables, such as GDP, employment, consumption, investment, and interest rates. It is built around the idea that the aggregate behavior of the economy can be captured through the device of a representative agent, whose preferences are captured by a simple, textbook utility function, and who takes the GDP of the whole economy to be her income. The representative agent is modeled in the same way that a microeconomist would model the behavior of a single person maximizing her utility given a personal budget. The DSGE model is a stripped down Walrasian general-equilibrium model, in which the representative agent forms complete plans for the future based on current information and anticipations of future developments guided by rational expectations. The rational expectations hypothesis permits the representative agent to make mistakes about the future, but models those mistakes as random or unsystematic and models the agent's expectations as consistent with the agent's current understanding of how the economy operates, encapsulated in the DSGE model itself. The representative agent is, therefore, not merely a consumer, worker, and investor – but also an economist.

Even the purveyors of the DSGE model found it to be inadequate to the financial crisis. They immediately recognized that it could not address the *financial* crisis when it did not have a financial sector. But their strategy for dealing with the crisis was not to embrace the notion of root-and-branch reform of economics, but to make incremental improvements to their model. If the model lacked a financial sector, then add one. If differences in the economic position of different kinds of people needed to be addressed, expand the model from one representative agent to two or three. If the insights of behavioral or neuroeconomics seemed to have some explanatory power for human action, then modify the representative agent's utility function to reflect those insights. Such tweaking of the model had already been underway ahead of the financial crisis. The crisis accelerated it. But all the time, the core structure and logic of the model remained untouched.

The incremental strategy is not disreputable in itself: we expect sciences to adapt and develop in the face of new empirical findings and new normative goals. The real problem is not the incrementalism but that the DSGE framework limits the way in which empirical evidenced can be acquired and framed and constrains how its lessons could be applied to understanding the economy. The real struggle for the soul of economics is a struggle over whether empirical data or prior theoretical commitments have the upper hand. It is a struggle between empiricism and rationalism.

IV

The struggle for the soul of economics is the old one between apriorists (or deductivists), such as the early John Stuart Mill, Ludwig von Mises, and Lionel Robbins, and the antitheoretical empiricists, such as the German Historical School and some kinds of American institutionalists, as well as many lay commentators. Most economists, however, do not occupy these extremes, but are like Homer Simpson – muddling along, tugged by opposing intellectual and methodological forces, and dealing with them inconsistently. It is hard to be dispassionate or disinterested about current events; so, I will instead address an historical case: the so-called 'Measurement without Theory' debate of 1947–1949 between Tjalling Koopmans (1947, 1949) and Rutledge Vining (1949a, 1949b).

This case has been widely discussed over the past 70 years. But I am not sure that the right lessons have been learned. Among the mainstream, it is largely seen as a mismatch with a Koopmans victory. Koopmans was the director of the Cowles Commission and later Nobel Prize winner, and Vining, who specialized in regional economics, was a player from the National Bureau of Economic Research's (NBER's) second team – so unsung, that his grave is the only relevant photograph that I could find on Google Images. Koopmans' targets, however, were notable: Wesley Clair Mitchell, who was the dean of American business-cycle analysis and the founder of the NBER, and his coauthor, Arthur Burns, later Chairman of the Federal Reserve and U.S. ambassador to Japan. It began with Koopmans (1947) review and methodological critique of Burns and Mitchell's *Measuring Business Cycles* (1946). It is to Burns and Mitchell that we owe the standard methods for determining the chronology of business cycles that are widely used today. In the 1940s, the Cowles Commission itself had developed what ultimately became the dominant econometric methodology for estimating systems-of-equations models, with macroeconometric models of the type pioneered by Jan Tinbergen as the intended beneficiary.

Koopmans characterizes Burns and Mitchell as the Keplers of economics, as mere gatherers of facts and purveyors of shallow generalizations, and the Cowles Commission as the Newtonian advocates of deep, unifying, and general theoretical insight. As Vining (1949a, pp. 77, 78) noted in his reply to Koopmans, there was a certain chutzpah in Koopmans' donning the mantel of Newton, when up to the time of Koopmans review the Cowles Commission had focused principally on methodology and offered few concrete macroeconomic results: Koopmans was declaring victory before the race was run.

More important, Koopmans simply misunderstands Kepler's achievement. My philosophical lodestone, Charles Sanders Peirce, who had studied Kepler's reasoning in great detail, called his determination of the elliptical orbit of Mars 'the greatest piece of Retroductive reasoning ever performed' (Peirce, 1931, para. 74). He chided Mill, who, like Koopmans, saw Kepler as merely describing the facts. Mill, Peirce wrote, lacked 'much practical acquaintance with astronomy' and, so, 'betray[ed] total ignorance' of Kepler's actual work (Peirce, 1931, para. 72). Peirce's detailed account of Kepler's investigation has been borne out by recent scholarship (Voelkel, 2001).

We find it easy to think like Mill and Koopmans about Kepler. We are so familiar with the idea of elliptical orbits that we tend to see the inferential problem as starting with the observational data neatly laid out on a two-dimensional Cartesian grid seen from above. The ellipse then jumps right off the off the page: all we needed to do to quantify it is to run the right regression on the data, and everything falls into place. But Kepler could not see it that way. He had to work from the inside with observational points scattered on the dome of the sky and make the leap of imagination that they could lie on a single plane. In his day, astronomy was branch of geometry and its goals were entirely descriptive. It was widely known among astronomers that Mars could be described as tracking some sort of oval with the Sun inside. But taken seriously that contradicted (a) the Earth-centric view of the universe; and (b) the belief that the planets must move with uniform speed. To preserve both while conforming to new observations, astronomers added complex epicycles to the Ptolemaic system.

Kepler knew that the Sun was large relative to the planets. He imagined that it could exercise some prepotent force over the planets. This was a vague hypothesis, but it proved crucial. It was a hypothesis drawn not from astronomy but from physics. Physics dealt with causes, not mere description, and aimed at identifying what was real. The prepotency of the Sun justified Kepler in entertaining the hypothesis that the oval was an ellipse, because the ellipse gives a special role to its foci and offered a special place to situate the Sun, and the varying distances of each point on the ellipse from its major focus would account for the non-uniform motion of the planet. But Kepler was not done: With great ingenuity, he posited a possible ellipse and adjusted it stepwise, based on the deviations of the ellipse from the observations in a way that, at each step, preserved its prior success and minimized the deviation. Kepler had to determine not only the shape of the ellipse quantitatively, but also – among infinite possibilities in three-dimensions – the orientation of the plane on which it lay.

Copernicus had not had much trouble with the Church for his heliocentric view of the planets. Copernicus was an astronomer, and astronomers were allowed to offer an instrumentalist, 'as if' view of their constructions. Kepler was persecuted by the Church precisely because he married astronomy with physics and made assertions about causes and what was really true. Kepler's causal hypothesis was crude, but it was the essential step in his ultimate success.

Contrary to Koopmans, Kepler was not Newton's data collector. Newton did not focus on the data points, but on the ellipse that Kepler had inferred and on Kepler's laws. Using deeper mathematics and conjecturing a quantified description of Kepler's prepotent force of the Sun, he gave a simple account of the orbit in terms of mass, gravity, and the laws of motion. Universalizing, it became the theory of gravity and unified other phenomena – a masterpiece, to be sure, but one that was accepted only slowly, because Newton's gravity had no deeper explanation of its origin than had Kepler's prepotent force. For us, the critical point is that Kepler was a vital step to Newton in a process that was a sequence of empirically justified, *theoretical* achievements. Both Kepler and Newton made real discoveries of a theoretical kind, supported by data, and Newton built on Kepler's theory more than on his data.

As Vining points out, in economics, Koopmans proposes to collapse the sequence into one step in which the observations are to be made consistent with the final theory. Koopmans' version of the Cowles program – not necessarily shared by everyone at Cowles – is strongly apriorist. The empirical element is only measurement consistent with a presumed theoretical model. He omits any notion of discovery or any question of re-evaluating the fundamental theoretical elements, as Kepler had done, in the light of data. In misrepresenting Kepler accomplishments, which were both observational and theoretical and mutually dependent and in misrepresenting the relationship of Newton's work to Kepler's, Koopmans failed to see how poorly his program analogized to the development of celestial mechanics in the seventeenth century.

V

So what was Koopmans' apriori theory? The key elements are (1) methodological individualism (agents as constrained maximizers account for all regularities in the economy) and (2) Walrasian general equilibrium. Aggregate analysis is acceptable, but only if the aggregates are rigorously derivable from individual behavior. Formal derivability is always a key methodological virtue.

Koopmans' methodological vision became the dominant view in economics. It was often violated in practice. But violations were typically viewed as shortcomings: 'Yes, we are all sinners; but one must not admire sin.' These days there are strong opposing views to Koopmans' vision among microeconomists; but virtually none in the mainstream of macroeconomics. The New Classical Macroeconomics [e.g. in Lucas Jr's (1976) famous 'critique' paper] and the advocates of the DSGE model set themselves up in opposition to the material fruits of the Cowles Commission program – that is, in opposition to the program of large-scale macroeconometric models that Lawrence Klein initiated while at the Cowles Commission. Still, with respect to metaphysics and methodology, they are more Cowles than Cowles.

How so? DSGE modelers begin with a weakly defended methodological individualism. Koopmans had supported methodological individualism by noting that all properties of social things must somehow trace to individuals, because, without individuals, there would be no society. As Vining notes, Koopmans' ontological claim can be true without ruling out that ordered arrangements of such individuals may possess causal autonomy and provide the *right* (perhaps the only successful) level of scientific explanation. We do not account for the workings of internal combustion engines in terms of their constituent subatomic particles – and not because it is computationally difficult nor because we are content with crude approximation, but because quantum mechanics does not provide the relevant conceptual resources to do so. Ontological individualism does not imply methodological individualism (see Hoover, 2001, 2009a).

There is an element of hypocrisy among DSGE advocates. Koopmans was embarrassed by the lack of a formal account of aggregation from individual choices to the behavior of GDP, consumption, and so forth. After the Measurement without Theory controversy, but long before the advent of DSGE, microeconomic theorists demonstrated that the conditions for the derivation of aggregates from individual choices are so constraining that they cannot possibly be met in a world populated by people. Yet, never to my knowledge have DSGE advocates addressed the issue. In their recent, apologia for the DSGE approach, Lawrence Christiano and coauthors simply refer to the 'long tradition in macroeconomics … [of] the model economy … populated by a representative household' (Christiano et al., 2018, p. 119). Where have all the microfoundations gone?

DSGE advocates also endorse Koopmans' view that an empirical result that is formally derivable gains epistemic warrant, despite the falsehood of the premises from which it is derived. Calvo pricing, which is often built into DSGE models, imagines that firms are permitted to adjust pricing according to a lottery (Calvo, 1983). DSGE modelers favor Calvo pricing simply because the fact of sticky prices can be deduced from it. But not even the DSGE modelers themselves believe that Calvo pricing captures any actual economic mechanism. It is singular how the sin of adhockery is always the mote in their brother macroeconomist's eye and never the plank in their own.

Koopmans' theory was Walrasian and committed to the views that everything depends on everything else and that a successful theory had to be comprehensive in scope. Vining asks

> Is the Walrasian conception not in fact a pretty skinny fellow of untested capacity upon which to load the burden of a general theory accounting for the events in space and time which take place within the spatial boundary of an economic system? [Vining, 1949a, p. 81]

But at least Koopmans and Cowles intended all along that the skinny fellow be fattened up with more and more comprehensive models. The DSGE modelers presume that they can both claim the merits of the Walrasian model as comprehensive and methodologically individual and simultaneously the virtues of highly stripped-down models (Hoover, 2015).

So far the analogies between Koopmans and the DSGE modelers have been about content. But there are strong meta-methodological analogies as well. For Koopmans and for the DSGE advocates, the failure of an opposing approach to adhere to their *a priori* theoretical presuppositions are automatic grounds to reject the analysis. The first line of an early version of Christiano et al.'s apologia for DSGE runs: 'People who don't like dynamic stochastic general equilibrium (DSGE) models are dilettantes' – that is, someone a truly professional macroeconomist may ignore or scorn (Christiano et al., 2017, p. 2). (It is unfortunate that the editors appear to have forced the authors to remove this point from the published version. It simply obscures their actual belief that anyone opposed to DSGE models should be turned out of macroeconomics.)

VI

'"Well, calling names won't catch dinner," said the Ethiopian' to the Leopard. What are the methodological alternatives? Vining points to an example – actually neoclassical, but not Walrasian: Milton Friedman and Simon Kuznets' (1945) analysis of consumption, which is the precursor to Friedman's later permanent-income hypothesis. Many people are leery of Friedman, either from the belief that he is an ideologue in every aspect of his economics or from what I regard as a profoundly misguided reading of his methodological position as instrumentalist (Hoover, 2009b). But let me lower the heat by repairing to the one whom Friedman acknowledges to be the font of his methodological vision: Alfred Marshall (see Marshall, 1885[1925]).

Marshall (1885[1925], p. 25) rejected the view that economic theory was a set of concrete truths. Rather, he argued, economic theory is a set of conceptual and logical tools that are used to frame concrete problems in order to reveal their hidden essences and to discover the 'manner and action of causes' (p. 51). We acquire economic knowledge through laborious digging into facts with theory as our picks and spades (see Hoover, 2006). This is necessarily a contextual, piecemeal, archaeological process – in part, because the economy is complex and, in part, because it is ever changing and, in part, because economics serves a variety of pragmatic interests. Marshall's view is not accurately characterized, as textbooks generally do, as *partial equilibrium*. (And Friedman, when he excoriates Walrasianism, is not attacking general equilibrium, but comprehensiveness as a *sine qua non* of appropriate modeling. See Friedman, 1949 [1953], pp. 89–92.) For some problems, some type of consideration of the economy as a whole cannot be ruled out. Marshall's plea is not to ignore interdependence altogether, but to adopt perspectival investigations motivated and adapted to particular purposes, rather than imagining that a model of everything – what Paul Teller (2001) has called the Perfect-Model Model – is either feasible or desirable. Marshallian modeling would be strongly theoretical, but also strongly disciplined by empirical data, not only in determining particular parameter values à la Cowles, but also in choosing the effective theoretical framework for such measurement.

VII

Back to the soul: What would economics look like if we followed Marshall?

First, it would be humble. Humility is not the product of a weak-minded tolerance. In the words of the Buffalo Springfield song of my youth:

> *There's battle lines being drawn*
>
> *Nobody's right, if everybody's wrong*
>
> …
>
> *Singing songs and carrying signs*
>
> *Mostly say, hooray for our side*

While this gets the psychology of tribalism and name-calling down pat, it is actually more to the point that 'Nobody's wrong, if everybody's right.' We cannot all be right. But unless we are mad or self-righteous, we know that we have no sovereign way to be certain that *we* are the ones who are right. And, in fact, *everybody* could very well be wrong – including us.

So, second, a Marshallian economics would be a *critical* and *fallibilist* economics. It matters what is right. We cannot be certain that anything we believe is finally true. We need to be willing to assess through empirical testing any seriously maintained alternative hypothesis or model. A problem with Koopmans and the DSGE advocates is that they do not take this critical stance seriously enough to ever allow them to question their framework in light of seriously offered alternatives. Criticism requires both that we be open to the possibility of error and that we devote conceptual and intellectual effort to developing methods of exposing it, and that we do not simply look the other way when it is exposed.

When Edward Prescott (1986), the grandfather of the DSGE model, found that his models did not fit the data, his reaction was to say (a) that his models were based on well-founded economic theory, without ever telling us what gave him the confidence in their well-foundedness; and (b) that it was simply a case of 'Theory Ahead of Measurement.' These reactions could be true. But giving how little critical analysis that the real-business-cycle/DSGE school has engaged in, they sound more like special pleading. Thomas Sargent recalled

> My recollection is that Bob Lucas and Ed Prescott were initially very enthusiastic about rational expectations econometrics. After all, it simply involved imposing on ourselves the same high standards we had criticized the Keynesians for failing to live up to. But after about five years of doing likelihood ratio tests on rational expectations models, I recall Bob Lucas and Ed Prescott both telling me that those tests were rejecting too many good models. [Sargent, 2005, pp. 567–568]

Prescott went on to reject standard statistical and econometric testing procedures in favor of *calibration*, a procedure in which parameters are chosen subjectively or with fairly loose justifications (see Hoover, 1995; and Hartley et al., 1997).

Are there available alternatives? Let me note three, though I cannot go into detail:

First, David Hendry and Grayham Mizon's methodology of *encompassing* in econometrics (Mizon, 1984; Hendry, 1988; Hendry & Mizon, 1993). Encompassing is the technical implementation of a simple logic: when faced with alternative econometric specifications to explain the same variable, consider the competing specifications within effectively a more general specification that nests them. Then test whether each alternative can be removed without loss of explanatory power. One may be dominant or each may prove to carry essential information. A dominant specification may be accepted on probation. Any other outcome implies that we have more work to do.

Second, Katarina Juselius and Søren Johansen's *scenario analysis for cointegrated vector autoregression models* (Hoover et al., 2008; Juselius, 2017).[1] This involves identifying the maintained hypotheses about probability distributions and the implied nonstationary comovements (or cointegration) of the variables from theoretical models and determining statistically whether they are admissible. The tests of admissibility are especially powerful, since cointegration is a feature of the data that can be identified without recourse to a theoretical economic model. Of course, as with all methods, these methods do make some presuppositions; but these are not very restrictive and, in any case, are implicit in the theoretical models that they analyze, so that they meet the models tested on common ground and do not set up a straw man. DSGE models evaluated through scenario analysis have routinely been shown to be inconsistent with the facts of cointegration (Juselius & Franchi, 2007).

These two cases are both methodologies for model evaluation and comparison. The third case, is John Muellbauer's work on credit in the macroeconomy (Muellbauer, 2018, 2020). It is harder to describe in a quick summary, since it exemplifies Marshall's notion of laboriously interrogating the data. But he, along with David Hendry, gives a good description of it in a recent paper (Hendry & Muellbauer, 2018). They provide a concrete example of neoclassical theory being used flexibly to illuminate the complex functioning of financial markets of macroeconomic importance (such as the mortgage market).

VIII

The DSGE family of models deserves to be taken seriously and evaluated against seriously maintained alternatives. But in the hands of its most vociferous advocates, it has been insulated from such comparisons. They advocate a set of prior constraints on the form of models – representative agents, rational expectations, dynamic optimization, general equilibrium – which, if not incorporated into competing models, automatically classifies them as inadmissible. The models are not unempirical; data play a role in their development. But they are Ptolemaic: when there is a mismatch between model and data, the DSGE modeler adds another epicycle, but does not reconsider any of the prior constraints. These prior constraints function like a Lakatosian hardcore (Lakatos, 1968/1969).

To change the metaphor, I sometimes think of the DSGE models as haiku. The 5/7/5 syllable pattern of haiku is arbitrary. That's OK for poetry. But the arbitrary rules of DSGE are not OK for science. Haiku is not the only admissible form of poetry; nor should the DSGE model be the only admissible form of macroeconomics. Here is my DSGE Haiku:

A shock surprises us;

Agents make optimal plans;

We are all happy.

It is poor poetry; and the DSGE rules are a poor way to pursue empirical economics.

In the end, saving the soul of economics comes down to three things:

(1) Humility;
(2) Vigorous criticism in the search for truth;
(3) In words of Charles Sanders Peirce, following 'The First Rule of Reason' 'which itself deserves to be inscribed on every wall of the city of philosophy:Do not block the way of inquiry.' [Peirce, 1931, para. 135]

Note

1. Although I am listed as a co-author with Juselius and Johansen, scenario analysis is my coauthors' approach; my role in our coauthored article was to clarify and draw out some of its methodological implications.

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