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Horizons of Understanding: A Review of Ray Fair's *Estimating How the Macroeconomy Works*

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Ray Fair's Estimating How the Macroeconomy *Works is the latest in a series of books by Fair that build, estimate, and apply his macroeconometric model to study the U.S. economy. In this book, Fair updates the model to incorporate the most recent data and uses it to analyze several important empirical questions, such as whether the U.S. economy moved into a new age of high productivity in the last half of the 1990s and the dynamics of prices, output, and unemployment. This review places his work in the context of the historical evolution of aggregate econometric models, compares it with the current developments in the estimation of dynamic stochastic general equilibrium models, and discusses some salient aspects of Fair's contributions.*

1 *Introduction*

James C. Scott, in his fascinating book *Seeing Like a State*, remarks that a pervasive high modernist ideology underlies much of modern social sciences. Scott defines high modernism as a strongly self-confident belief in scientific discovery and the subsequent rational design of a social order commensurate with that knowledge.

There is no purer example of the high modernism impulse in economics than the vision behind the design and estimation of macroeconometric models. For over seventy years, since the pioneering investigations of Jan Tinbergen (1937 and 1939), researchers have aspired to build a coherent representation of the aggregate economy. Such a model would be the most convenient tool for many tasks, which, somewhat crudely, we can classify into the trinity of analysis, policy evaluation, and forecasting. By analysis, I mean the exercises involved in studying the behavior and evolution of the economy, running counterfactual experiments, etc. Policy evaluation is the assessment of the economy's responses in the short, middle, and long run to changes in policy variables such as taxes, government expenditure, or monetary aggregates. By forecasting, I mean the projections of future values of relevant variables. All those exercises are manifestations of high modernism in action: instances of formal understanding and deliberate control. Not surprisingly, the political compromises of many of the economists

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originally engaged in the construction of macroeconometric models revealed a common predisposition toward a deep reorganization of the economic life of western countries.

Since Tinbergen's contributions, much effort has been invested in this intellectual bet. What do we have to show after all these years? Have we succeeded? Have all the costs been worthwhile? Or has the project been a failure, as with the other examples of high modernist hubris that Scott compiles?

Ray C. Fair's *Estimating How the Macroeconomy Works* (Harvard University Press, 2004) starts from the unshakable premise that the task has been valuable, that in fact we have a working model of the U.S. economy or, at least, a good approximation to it. Moreover, he states that we have achieved the goal by following a concrete path: the one laid down by the Cowles Commission simultaneous equations framework. In the preface of a previous book, Fair (1994) characterized his research as a rallying cry for the Cowles Commission approach. His new monograph keeps the old loyalties by updating the multicountry (MC) model that he has been focusing on since 1974 (chapters 1 and 2) and by applying it to answer topical questions, such as the effects of nominal and real interest rates on consumption and investment, the dynamics of prices, wages, and unemployment in the United States, or whether the second half of the 1990s witnessed the appearance of a new economy (chapters 3 to 12).

Fair's passionate defense of his practice and his steadfast application of the MC model to measure a range of phenomena by themselves justify acquiring *Estimating How the Macroeconomy Works* by all economists interested in aggregate models. It was no accident that, even before the editor of the journal contacted me with the suggestion of writing this review, I had already bought the book and placed it with its two older brothers, *Specification, Estimation, and Analysis of Macroeconometric Models* and *Testing Macroeconometric Models*, in my library.[1]

Reading the books as a trilogy is a rewarding endeavor that yields a comprehensive view of what the Cowles Commission approach is all about, how to implement it, what it can deliver, and how it compares with the alternative methodologies existing in the profession, in particular with the increasingly popular estimation of dynamic equilibrium economies. But, perhaps to understand this comparison better, it is necessary to glance at the history of macroeconometric models.

2 *Great Expectations*

It is difficult to review Lawrence R. Klein and Arthur S. Goldberger's 1955 monograph, *An Econometric Model of the United States, 1929–1952*, without sharing the enthusiasm of the authors about the possibility of building a successful representation of the U.S. economy. In barely 160 pages, Klein and Goldberger developed an operating model that captured what was then considered the frontier of macroeconomics; estimated it using limited-information maximum likelihood; and described its different applications for analysis, policy evaluation, and forecasting. Premonitory proposals, such as Tinbergen's *Statistical Testing of Business-Cycle Theories* or Klein's *Economic Fluctuations in the United States, 1921–1941*, although promising, did not really deliver a fully working model estimated with the modern formal statistical techniques associated with the Cowles Commission.[2] In sharp contrast, Klein and Goldberger had that model:

[1] I guess I am too young to have had a chance to obtain the very earliest volumes of the series, *A Model of Macroeconomic Activity* (1974 and 1976), which went out of print decades ago.

[2] Much of the material in this section is described in considerable detail in Ronald G. Bodkin, Klein, and Kanta Marwah (1991).

elegant, concise with twenty endogenous variables and fifteen structural equations, relatively easy to grasp in structure (although not in its dynamics), and with an auspicious empirical performance.

The excitement of the research agenda was increased with the companion monograph by Goldberger, *Impact Multipliers and Dynamic Properties of the Klein–Goldberger Model*, published in 1959, and the paper by Irma Adelman and Frank L. Adelman (1959). Goldberger's book computed the multipliers of the model for endogenous and exogenous changes in tax policy, both at impact and over time. Even more pointedly, Goldberger demonstrated the model's superior forecasting ability for the years 1953–55 in comparison with a battery of naive models (random walks in levels and in differences) proposed by Milton Friedman and other researchers less convinced of the potentialities of the model. Therefore, Goldberger's contribution compellingly illustrated how the Klein–Goldberger model was a productive tool for policy evaluation and forecasting.

The remaining leg of the trinity of analysis, policy evaluation, and forecasting was covered by Adelman and Adelman. In an innovative study, the Adelmans reported that the Klein–Goldberger model, when hit by exogenous shocks, generated fluctuations closely resembling the cycles in the U.S. economy after the Second World War. The match was not only qualitative but also quantitative in terms of the duration of the fluctuations, the relative length of the expansion and contractions, and the degree of clustering of peaks and troughs.

Spurred by the success of Goldberger and Klein, many groups of researchers (several of them led by Klein himself at the University of Pennsylvania) built increasingly sophisticated models during the 1960s and early 1970s that embodied modern versions of the IS–LM paradigm. Examples include the postwar quarterly model; the Brookings model, which inaugurated the era of team efforts in the formulation of macroeconometric models and generated important technology in terms of construction, solution, and implementation of models; the Wharton and DRI models (which opened macroeconometric models to commercial exploitation); the Bureau of Economic Analysis (BEA) model; or the MPS model, which we will discuss below. A vibrant future seemed to lie ahead.

3 *Death . . .*

Contrary to expectations, the decades of the 1970s and 1980s were not kind to large-scale aggregate models. Blows against them came from many directions. To keep the exposition concise, I will concentrate on three groups of challenges: those complaining about the forecasting properties of the models, those expressing discomfort with the econometric justifications of the models, and those coming explicitly from economic theory.

In an early display of skepticism, Charles R. Nelson (1972) used U.S. data to estimate ARIMA representations of several key macroeconomic variables, such as output, inflation, unemployment, or interest rates. Nelson documented that the out-of-sample forecast errors of the ARIMA equations were systematically smaller than the errors generated by the MPS model of the U.S. economy (originally known as the FRB–MIT–PENN model, Bodkin, Klein, and Marwah 1991).

Nelson's findings were most disappointing. Simple ARIMA models were beating the MPS model, a high mark of the Cowles Commission approach. To make things worse, the MPS was not a merely academic experiment. In its evolving incarnations, the MPS model was operationally employed by staff economists at the Federal Reserve System from the early 1970s to the mid 1990s (see Flint Brayton et al. 1997). Here we had a state-of-the-art MPS model, with all the structure incorporated in its behavioral equations (around sixty in the early 1970s) and its extra variables, and yet it could not hold its own against an ARIMA. What hope could we have then that the intellectual bet of building an aggregate macro model would pay off? The message of Nelson's paper was strongly reinforced by the apparent inability of the existing econometric models to account for the economic maladies of the 1970s (although, to be fair, the news of the empirical failures of macroeconometric models during the 1970s were grossly exaggerated).

A second strike arose directly from within econometric theory. During the late 1970s, there was a growing dissatisfaction with the casual treatment of trends and nonstationarities that had been unfortunately widespread among large-scale Keynesian models. The explosion of research on unit roots and cointegration following the papers of Clive W. J. Granger (1981), Nelson and Charles I. Plosser (1982), and Peter C. B. Phillips (1987) highlighted how much of the underpinnings of time series analysis had to be rethought.

But the econometric problems did not stop at this point. Christopher A. Sims (1980), in his famous article on *Macroeconometrics and Reality*, asserted that the troubles in the econometric foundation of the conventional models ran even deeper. Elaborating on a classic article by Ta-Chung Liu (1960), Sims forcefully argued that the style in which economists connected models and data was so unfitting that it invalidated their venerable identification claims. Instead, he proposed the estimation of vector autoregressions (VARs) as flexible time series summaries of the dynamics of the economy.

Finally, a third and, for many researchers, fatal blow was the Lucas critique. Robert E. Lucas (1976) famously criticized the lack of explicit microeconomic foundations of the macroeconometric models, not because it disconnected the models from standard economic theory (which was certainly a concern in itself), but mainly because it implied that the models were not adequate for policy analysis. Relations estimated under one policy regime were not invariant to changes in the regime. Consequently, the models were unsuited to the type of exercises that economists and policymakers were actually interested in. Lucas's argument was nothing but a particular example of a much more general reexamination of the relation between models and data brought about by the rational expectations (RE) revolution.

Some economists have contended that the empirical relevance of the Lucas critique has been overstated. For instance, Sims (1980) claimed that regime changes were rare and that most policy interventions were done within the context of a given regime (see, also, the formalization of this idea by Eric N. Leeper and Tao Zha 2003). Thus, they were suitable for analysis with a reduced-form empirical model. However, those counterarguments missed the true strength of Lucas's insight within the professional discourse. By uncovering the flaws of large-scale structural models, Lucas destroyed the meta-narrative of the Cowles Commission tradition of model building. Suddenly, econometric models were not the linchpin of modern macroeconomics, the theoretical high ground that ambitious researchers dreamt of conquering and subjugating. No, macroeconometric models had become one additional tool, justified not by emphatic assertions of superiority, but by modest appeals to empirical performance and reasonability. But these were moderate qualities and moderation rarely rallies young researchers.

4 . . . *And Transfiguration*

Within this widespread sense of disappointment with the existing situation, Finn E. Kydland and Edward C. Prescott's (1982) prototype of a real business cycle (RBC) model held an irresistible allure. Kydland

and Prescott outlined a complete rework of how to build and evaluate dynamic models. Most of the elements in their paper had been present in articles written during the 1970s. However, nobody before had been able to bring them together in such a forceful recipe. In just one article, the reader could see a dynamic model, built from first principles with explicit descriptions of technology, preferences, and equilibrium conditions, a novel solution approach, based more on the computation of the model than on analytic derivations, and what appeared to be a radical alternative to econometrics: calibration. Kydland and Prescott presented calibration as a procedure for determining parameter values by matching moments of the data and by borrowing from microeconomic evidence. The model's empirical performance was evaluated by assessing the model's ability to reproduce moments of the data, mainly variances and covariances not employed in the calibration.

After Kydland and Prescott, dozens of researchers jumped into building RBC models, embracing computation and calibration as the hallmarks of modern macroeconomics. The basic model was explored, extended, and modified. Instead of the original productivity innovation, a multitude of shocks began to appear: shocks to preferences, depreciation, household productions, etc. Instead of a Pareto-efficient economy, economists built models with plenty of imperfections and room for an activist fiscal and monetary policy. Instead of a single agent, they built models with heterogeneity of preferences, endowments, and information sets. Even the name by which the models were known changed and, by the late 1990s, they started to be known by the tongue twister dynamic stochastic general equilibrium (DSGE) models.

Among all of these changes, the one that concerns us the most here was the slow, nearly imperceptible, but ultimately unstoppable return of econometrics as a methodology to take DSGE models to the data. Calibration was always the shakiest of all the components of the Kydland and Prescott's agenda. By rejecting statistical tools, their approach attempted to push economics back to an era before Haavelmo. Matching moments of the data without an explicit formalism is, once you dig beyond rhetoric, nothing except a method of moments carelessly implemented. Borrowing parameter values from microeconomics forgets that parameters do not have a life of their own as some kind of platonic entity. Instead, parameters have meaning only within the context of a particular model. Worst of all, there was no metric to assess the model's performance or to compare it with that of another competing model, beyond a collection of ad hoc comments on the simulated second moments of the model.

Nevertheless, calibration enjoyed a period of prosperity and for good reasons. First, as recalled by George W. Evans and Seppo Honkapohja (2005), the early results on the estimation of RE models were heartbreaking. Model after model was decisively rejected by likelihood ratio tests, despite the fact that many of these models seemed highly promising. Calibration solved this conundrum by changing the rules of the game. Since it was no longer important to fit the data but only a small subset of moments, researchers had a much lower hurdle to overcome and they could continue their work of improving the theory. Second, in the 1980s, macroeconomists had only primitive solution algorithms to approximate the dynamics of their models. Furthermore, those algorithms ran on slow and expensive computers. Without efficient and fast algorithms, it was impossible to undertake likelihood-based inference (although this difficulty did not affect the generalized method of moments of Lars Peter Hansen 1982). Finally, even if economists had been able to compute RBC models efficiently, most of the techniques required for estimating them employing a likelihood approach did not exist or were unfamiliar to most economists.

But the inner logic of the profession, with its constant drive toward technical positioning, would reassert itself over time and, during the 1990s, the landscape changed dramatically. There were developments along three fronts. First, economists learned to efficiently compute equilibrium models with rich dynamics. There is not much point in estimating stylized models that do not even have a remote chance of fitting the data well. Second, statisticians developed simulation techniques like Markov chain Monte Carlo (McMc), which we require to estimate DSGE models. Third, and perhaps most important, computers became so cheap and readily available that the numerical constraints were radically eased. By the late 1990s, the combination of these three developments had placed macroeconomists in a position not only where it was feasible to estimate large DSGE models with dozens of equations, but where it was increasingly difficult to defend any reasonable alternative to formal inference.

And indeed DSGE models began to be estimated. A class of DSGE models that has enjoyed a booming popularity both in terms of estimation and policy applications is the one that incorporates real and nominal rigidities. The structure of this type of model, as elaborated, for example, by Michael Woodford (2003), is as follows. A continuum of households maximizes their utility by consuming, saving, holding real money balances, supplying labor, and setting their own wages subject to a demand curve and Calvo's pricing (e.g., the households can reoptimize their wages in the current period with an exogenous probability; otherwise they index them to past inflation). A competitive producer manufactures the final good by aggregating a continuum of intermediate inputs. The intermediate inputs are produced by monopolistic competitors with capital and labor rented from the households. In the same way workers do, the intermediate good producers face the constraint that they can only change prices following a Calvo's rule. Finally, the monetary authority fixes the one-period nominal interest rate through open market operations with public debt. Other standard features of this model include varying capacity utilization of capital, investment adjustment costs, taxes on consumption, labor, and capital income, monetary policy shocks, and quite often, an international economy sector (as described in Stephanie Schmitt-Grohé and Martín Uribe 2003).

Given their growing complexity, these models with real and nominal rigidities are also sometimes known as medium-scale DSGE models, although some of them, running up to eighty or ninety stochastic equations (as many as early versions of the MPS model, for example), hardly deserve the title. Since these models are particularly congenial for monetary analysis, numerous central banks worldwide have sponsored their development and are actively employing them to help in the formulation of policy. Examples include the models by the Federal Reserve Board (Rochelle M. Edge, Michael T. Kiley, and Jean-Philippe Laforte 2007), the European Central Bank (Frank Smets and Raf Wouters 2003 and Kai Christoffel, Günter Coenen, and Anders Warne 2006), the Bank of Canada (Stephen Murchison and Andrew Rennison 2006), the Bank of England (Richard Harrison et al. 2005), the Bank of Finland (Juha Kilponen and Antti Ripatti 2006 and Mika Kortelainen 2002), the Bank of Sweden (Malin Adolfson et al. 2005), and the Bank of Spain (Javier Andrés, Pablo Burriel, and Ángel Estrada 2006), among several others.

With medium-scale models, macroeconomists have responded to the three challenges listed in section death. With respect to forecasting, and despite not having been designed for this purpose, DSGE models do a reasonable job of forecasting at short run horizons and a remarkable one at medium horizons. And they do so even when compared with flexible reduced-form representations like VARs (as reported for the Euro Area-Wide model of the European Central Bank by Christoffel, Coenen, and Warne 2007). Second, many of the models explicitly incorporate unit roots and the resulting cointegrating relations implied by the balanced growth properties of the neoclassical growth model at the core of the economy. These aspects receive an especially commendable treatment in Edge, Kiley, and Laforte (2007). Third, modern DSGE models are estimated using the equilibrium conditions of the economy, eliminating the requirement of specifying exclusionary restrictions that Sims objected to (although, unfortunately, not all identification problems are eliminated, as documented by Nikolay Iskrev 2007). Fourth, since we estimate the parameters describing the preferences and technology of the model, the Lucas critique is completely avoided. Thus, it looks like we are back where we thought we were at the late 1960s: with good and operative aggregate models ready for prime time. But are we really there?

5 *Estimating How the Macroeconomy Works in Context*

Fair's book takes a more skeptical view of the ability of modern DSGE models—not directly, as it does not discuss those models in detail, but implicitly, through its adherence to the traditional Cowles Commission approach. Fair is to be applauded for his position: first, and foremost, because there is much of value in the Cowles Commission framework that is at risk of being forgotten. His books may play a decisive role in the transmission of that heritage to newer generations of researchers. Second, because we work in a profession where there is a strong tendency to embrace the latest fad and to ignore existing accomplishments. And third, because intellectual diversity is key to maintaining a vibrant econometric community.

Fair's MC model is composed of two sections (or submodels): the U.S. model and the rest of the world model. The U.S. model has six sectors: households, firms, financial, foreign, federal government, and state and local governments. Their behavior is captured through thirty stochastic equations (nine for the household sector, twelve for firms, five for the financial sector, one for imports, and three for government). In addition, there are 101 accounting identities. This makes the U.S. model one of moderate size, especially if we compare it with other models in the Cowles Commission tradition. For instance, the Washington University macro model marketed by Macroeconomic Advisers for commercial applications has around 442 equations.[3]

The rest of the world model is composed of thirty-eight countries for which structural equations are estimated (thirteen with quarterly data and twenty-five with annual data) and twenty countries for which only trade share equations are estimated. For the thirty-eight countries with structural equations, Fair estimates up to fifteen stochastic equations, the same for all of the countries but with possibly different coefficients. Then, the United States and the rest of the world components are linked through a set of equations that pin down trade shares and world prices (which generates an additional equation for the U.S. model). When we put together the two sections of the MC model, we obtain a total of 362 stochastic equations, 1,646 estimated coefficients, and 1,111 trade share equations.

The basic estimation technique of the model's equations is two-stage least squares (2SLS). The data for the United States begin

[3] See http://www.macroadvisers.com/content/WUMM2000_overview.pdf, accessed on January 7, 2008.

in 1954 and as soon as possible for other countries, and ends between 1998 and 2002. Fair also performs an extended battery of tests for each equation, including tests for additional variables, for time trends, for serial correlations of the error term, and for lead variables, or Donald W. K. Andrews and Werner Ploberger (AP) (1994) stability tests among others. Finally, there is some testing of the complete model, basically by examining the root mean squared error between the predicted and actual values of the variables, and exercises in optimal control using certainty equivalence.

The spirit of the estimated equations is firmly rooted in the Keynesian tradition and allows for the possibility of markets not clearing. For example, consumption depends on income, an interest rate, and wealth. Investment depends on output and an interest rate. Prices and wages are determined based on labor productivity, import prices, unemployment, and lagged prices and wages. Besides, the model accounts for all balance-sheet and flow-of-funds constraints, as well as for demographic structures.

Instead of spending time on a detailed explanation of the model and the different applications, I will outline several aspects of Fair's work that I find of interest, particularly in relation to the practice of other researchers. Nevertheless, the reader is invited to consult Fair's book and his web page (http://fairmodel.econ.yale.edu/main2.htm,) for the details of the model. One of the most outstanding features of Fair's work has always been his disclosure policy, a true gold-standard for the profession. The book provides an extremely detailed description of the model, with all of the equations and data sources and definitions. On Fair's web page, the visitor can obtain the latest updates of the model[4] and the accompanying documentation, memos, and papers. The interested researcher can also download the Fair–Parke program used for computations of the model: point estimation, bootstrap, forecast, simulation, and optimal control. The software is available not only in an executable form, but also as the source code in FORTRAN 77, which is a treat for those economists concerned with numerical issues.

5.1 *National Income and Product Accounts*

The first example of the important lessons from the Cowles Commission approach I want to discuss is the need for a thoughtful treatment of the relation between National Income and Product Accounts (NIPA) and the definitions of variables in the model. Jacob Marshack, when he organized the work of the Cowles Commission at University of Chicago in 1943–44, considered that data preparation was such an important part of the process that he put it on the same level as model specification and statistical inference.

NIPA are a complicated and messy affair. The task in front of statisticians is intimidating: to measure the production, income, and inputs of a large, complex economy, with thousands and thousands of ever-changing products and with active trading with the rest of the world. To make things worse, the budget allocated to national statistics institutes like the BEA is awfully limited (the president's budget request for the BEA for fiscal year 2008 was $81.4 million). Consequently, the BEA needs to make numerous simplifying assumptions, implement various shortcuts, and rely upon surveys with sample sizes that are too small for the task at hand.

But even if we gloss over the data collection difficulties, there is the open question of the mapping between theory and measurement. The objects defined in our models are not necessarily the objects reported by the BEA. For instance, the BEA computes real output by deflating nominal output by

[4] All operational macroeconometric models are living entities. They require constant care and incorporation of new information.

a deflator that weights consumption, investment, government consumption, and net exports. However, following the work of Jeremy Greenwood, Zvi Hercowitz, and Per Krusell (1997 and 2000), investment-specific technological change has become a central source of long run growth and aggregate fluctuations in many estimated DSGE models. Investment-specific technological change induces a relative price for capital in terms of consumption that is not trivially equal to one as in the standard neoclassical growth model. This implies that in these models, to be consistent with the theory, we need to define real output as nominal output divided by the consumption deflator. In this and many other cases, the econometrician has to readjust the data to suit her requirements. Pages 180 to 188 of the book and the 101 identities in the model (equations 31 to 101) show how, over the years, Fair has done an incredibly heedful job of mapping theory and data.

In a sad comparison, sometimes DSGE macroeconometric models are not nearly as thorough in their treatment of NIPA data. Moreover, and most unfortunately, extended discussions of NIPA data are often frowned upon by editors and referees, who ask either they be cut or sent to remote appendices. It is not clear to me the value of employing a state-of-the-art econometric technique if we end up feeding it with inferior measurement.

5.2 *Number of Macro Variables*

The Cowles Commission simultaneous equations models employ many more data series than the new DSGE models. The difference is due to the problem of stochastic singularity. A linearized, invertible DSGE model with N shocks can only non trivially explain N observables. All the other observables are linear combinations of the first N variables. Consequently, any observation of a variable not included in the first N observables that is different from the one predicted by the model implies a likelihood of $-\infty$.

This point is seen more clearly with a simple example. Imagine that we have a linearized benchmark RBC model with a productivity and a demand shock. Since we have two shocks, we can account for two observables. Let us suppose that the econometrician chooses instead as her observables three variables: consumption, hours, and investment. Then, it is easy to show that investment in this model has to be equal to a linear combination of consumption and hours that does not have a random component (more generally, any of the three observables is a deterministic linear combination of the other two). Hence, any observation for investment that is different from the previous linear combination cannot be predicted by the model. Since, in reality, investment data do not neatly fall into the linear function of consumption and hours imposed by the model, we will compute a likelihood of $-\infty$ with probability 1.

Even if the number of shocks in medium-scale DSGE models has grown to a number between ten and twenty, we are still quite far away from the 248 variables of the U.S. model in Fair's book. Since it seems plausible that additional data will bring extra information to the estimation, the use of a large number of macro variables may be an important advantage of the MC model.

How can we bridge the distance between the Cowles Commission models and the DSGE models in terms of variables used for the estimation? One simple possibility would be to increase the number of shocks in the model. However, even with around twenty structural shocks, as in the richest of the contemporary DSGE models, we are already straining our ability to identify the innovations in the data. Worse still, the economic histories that we tell to justify them are becoming dangerously thin.

A second approach is to introduce measurement error in the observed variables. As we argued before, there are plenty of sources of noise in the data that can be plausibly modeled as measurement error (see Thomas J. Sargent 1989 for an exposition of this argument). However, introducing noise in the observables through measurement error is dangerous. We risk falling into a situation where the measurement error is explaining the dynamics of the data and the equilibrium model is a nuisance in the background of the estimation. In addition, measurement errors complicate identification.

An intriguing alternative is to follow the proposal of Jean Boivin and Marc Giannoni (2006). Boivin and Giannoni start from the observation that factor models have revealed how large macro data sets contain much information about the paths of series like output or inflation (James H. Stock and Mark W. Watson 1999, 2002). This suggests that the standard assumption that the observables derived from the DSGE model are properly measured by a single indicator such as gross domestic output is potentially misleading. Instead, Boivin and Giannoni exploit the relevant information from a data-rich environment by assuming that the variable implied by the model is not directly observed. Instead, it is a hidden common factor for a number of observables that are merely imperfect indicators of the exact variables. Boivin and Giannoni show that their idea delivers an accurate estimation of the model's theoretical variables and shocks. Furthermore, the estimates imply novel conclusions about key structural parameters and about the sources of economic fluctuations.

Boivin and Giannoni's work also helps to overcome another difficulty of DSGE models. We mentioned before that because of stochastic singularity, we can use only a reduced number of variables in the estimation. This raises the question of which variables to select for the estimation. Imagine that we have a model with three shocks. Thus, we can employ at most three variables. But which ones? Should we pick output, consumption, and hours? Or investment, consumption, and hours? Theory is not a guide for the selection of the variables, or at best, a weak one. Pablo Guerrón (2007) has documented how the use of different combinations of observables in the estimation of DSGE models has a large effect on inference. These problems would be avoided if we could use a large data set for the estimation of the model.

5.3 *Rational Expectations*

Fair does not assume that expectations are rational. In fact, it is one of the very first things he tells the reader (as early as page 4): It is assumed that expectations are *not* rational. Instead, he includes lagged dependent variables as explanatory variables in many of the equations and finds that they are significant even after controlling for any autoregressive properties of the error term. These lagged variables may capture either partial adjustment effects or expectational effects that are not necessarily model consistent, two phenomena that are hard to separate using macro data.

Fair recognizes that his choice puts him in the minority in the profession, but that he sees several advantages in it. First, RE are cumbersome to work with. Without them, one can safely stay within the traditional Cowles Commission framework, since the Lucas critique is unlikely to hold. Second, Fair does not consider it plausible that enough people are so sophisticated that RE are a good approximation of actual behavior. Third, and related to the previous point, he finds in his estimates a considerable amount of evidence against RE and little in its favor.

The first objection is intimately linked with the fact that dynamic equilibrium models with RE rarely imply closed-form solutions. We need to resort to the computer to obtain numerical approximations, which have problems and limitations of their own. For example, we can rarely evaluate the exact likelihood of the model but only the likelihood of the numerically approximated model. This causes problems for estimation (Jesús Fernández-Villaverde, Juan F. Rubio-Ramírez, and Manuel S. Santos 2006). Also, RE impose a tight set of cross-equation restrictions that are difficult to characterize and that limit our intuitive understanding of the model.

However, computational complexity is less of an issue nowadays. Recently, important progress has been made in the solution and estimation of models with RE. Macroeconomists have learned much about projection and perturbation methods (Kenneth L. Judd 1992, Judd and Sy-ming Guu 1992 and S. Boragan Aruoba, Fernández-Villaverde, and Rubio-Ramírez 2006). Problems that looked intractable a few years ago are now within the realm of feasibility. In particular, perturbation allows us to handle DSGE models with dozens of state variables and obtain nonlinear solutions of the desired accuracy. Moreover, also thanks to perturbation, we can easily solve non linear optimal control problems in these models without imposing certainty equivalence (see, for example, the Ramsey policy design in Schmitt-Grohé and Uribe 2006).

Estimation methods have also advanced quickly, and we have powerful new tools such as McMc and sequential Monte Carlo (SMC) algorithms that allow us to handle, through simulation methods, extremely challenging estimation exercises (see Sungbae An and Frank Schorfheide 2007 for McMc and Fernández-Villaverde and Rubio-Ramírez 2005 and 2007 for an introduction to SMC algorithms).

Therefore, one could argue that it is more fruitful to solve directly for the equilibrium of the model and derive the estimating equation implied by it, instead of searching for empirical structures. In that way, we avoid encounters with the Lucas critique independently of whether or not it is empirically relevant (a tough question in itself because of the difficulty in separating regime changes in the estimation from other sources of structural change or from stochastic volatility in the innovations that drive the dynamics of the economy, as emphasized by Thomas A. Lubik and Paolo Surico 2006).

Fair's second objection to RE—that a majority of people are not sophisticated enough to have model consistent expectations—is more appealing and intuitive. It also opens important questions for macroeconometricians. One is the role of heterogeneity of beliefs in macro models. If expectations are not rational, there is no strong reason why we should expect that beliefs are homogeneous across agents. But, then, we need to think about how this heterogeneity aggregates into allocations and prices. For instance, we could have a situation where the only beliefs that matter for macro variables are the ones of the marginal agent. This situation resembles the results in finance where the pricing of securities is done by the marginal agent. If this agent is unconstrained, the existence of widespread borrowing constraints or other market imperfections has a negligible quantitative impact. Equally, with belief heterogeneity, it could be the case that the only expectations that matter are those of a small subset of agents that have RE or something close to it.

This intuition is explored in many papers. Lawrence Blume and David Easley (2006) present a particularly striking finding. They show that in any Pareto-optimal allocation with complete markets, all consumers will see their wealth vanish except the one whose beliefs are closest to the truth. Of course, there are many caveats. We require Pareto optimality, the theorem deals only with wealth distributions (and not, for instance, with labor supply), and the findings characterize the limit behavior of these economies.[5]

[5] And the long run may be far away. Think of the case where the divergent beliefs are about a low frequency feature of the data. It may take an inordinately long time before the beliefs have an impact on wealth distributions.

Regardless of one's view of the empirical relevance of Blume and Easley's result, departing from RE leads us quite naturally to heterogeneity of beliefs and to the need to develop econometric techniques to incorporate this heterogeneity.

A second question arising from the departure from RE is the modeling of learning. A classical defense of RE is the idea that relatively simple learning algorithms such as least-squares converge to the RE equilibrium (see Albert Marcet and Thomas Sargent 1989). Perhaps not surprisingly, this classical defense is easily broken in several contexts where we have self-confirming equilibria (SCE) in which the agents have the wrong beliefs about the world but there is nothing in the data that forces them to change their subjective distributions. One of the most remarkable facts about SCE is how easy it is to build examples where SCE exist. Disappointingly, as in the case of heterogeneous beliefs, it seems that macroeconometricians have not investigated this issue in detail (see, however, the pioneering work of Giorgio E. Primiceri 2006 and Sargent, Noah Williams, and Zha 2006).

All of which brings us back to the question of how to assess the importance of the departure from RE, an issue not terribly well understood. Krusell and Anthony A. Smith (1998) demonstrated, in the context of computing models with heterogeneous agents, that complicated forecasting rules can be approximated with fantastic accuracy by simple OLS regressions. Thus, even if the agents do not have RE, it is conceivable that their behavior in certain models may be well described by RE. Since assuming RE often makes our life easier by eliminating free parameters, we may as well do so.

Fair's final point deals with the empirical evidence on RE. This argument requires a nuanced assessment. Testing for RE is a challenging task. Commonly, the proposed tests are really joint tests of RE and other assumptions in the model. Imagine, for example, that we have a model like Robert E. Hall's (1978) where RE imply that consumption follows a random walk. If the data reject this prediction, it may be either because RE fail, because households face borrowing constraints, or because the parametric form of the utility function is misspecified. Disentangling all these possibilities requires, at best, considerable ingenuity, and at worst, it may not be feasible. Finally, RE appear in the model through complicated cross-equation restrictions. Because of this, it is not immediately obvious why it is possible in general to test for RE by adding lead values of variables in the equations of the model, as argued in the book. On many occasions, the solution of the DSGE model implies that only state variables, and not any lead variable, appear in the relevant equation for an observable. Testing for the presence of that lead variable then tells us that this variable is not significant despite RE being present in the data.

5.4 *Treatment of Nonstationarities*

A second point of departure of Fair's MC model from much of the literature is the treatment of nonstationarities. Fair observes that it is exceedingly difficult to separate stochastic from deterministic trends and that, consequently, it seems logical to follow the easiest route. Moreover, the point estimates will not be seriously affected, and the standard errors, whose asymptotic approximation breaks down in the nonstationary case, can always be approximated by the bootstrap. Hence, the working hypothesis of the book is that variables are stationary around a deterministic trend.

It is easy to be sympathetic to Fair's pragmatism. Much work went into the study of nonstationarity during the 1980s and 1990s. At the end of the day, many researchers felt (paraphrasing Lawrence J. Christiano and Martin Eichenbaum's 1990 article) that we do not know and probably we do not care that much. It is even harder to see why nature should have been so kind to econometricians as to have neatly divided time series into those with deterministic trends and those with unit roots. More complex specifications, such as fractional integration and other long memory processes, are most likely a better description of the data (see Peter M. Robinson 2003).[6]

I have only two small arguments to add to Fair's well-constructed argument. First, simplicity sometimes runs in favor of unit roots. Often, the presence of a unit root allows us to rewrite the model in a more convenient parameterization. This is the case, for example, in DSGE models with long run growth, where the presence of a stochastic trend simplifies derivations. Pragmatism can now be employed to justify the use of unit roots. Second, one may want to be careful when designing and evaluating a bootstrap with respect to the way in which nonstationarity appears in the model since it will have an impact on the simulation efficiency.

5.5 *Stability of Estimates*

Nonstationarities are not the only source of change over time that researchers should be concerned about. One aspect that I always found salient in the data is parameter drifting. Fair's results seem to uncover strong evidence of this variation in parameters over time. To illustrate this, let us examine equation 1 in the model, which explains the log of consumer durables (*CS*) in real per capita terms (divided by *POP*). Both in the version in *Testing Macroeconometric Models* (1994) and in the version in *Estimating How the Macroeconomy Works* (2004), the right-hand-side (RHS) variables are the same except for total net wealth per capita (*AA/POP*) and a time trend (*T*) in the second version: a constant, the percent of population over sixteen that is between twenty-six and fifty-five years old minus the percent between sixteen and twenty-five (AG1), the percent of population over sixteen that is between fifty-six and sixty-five years old minus the percent between sixteen and twenty-five (AG2), the percent of population over sixteen that is older than sixty-six minus the percent between sixteen and twenty-five (AG3), the lag log of per capita real services consumption, real per capita disposable income ($YD/(POP * PH)$), and the after-tax bill rate (*RSA*). The point estimates (with the t-statistics in parenthesis) are reproduced in table 1.

We see, for instance, how the coefficient on AG2 has gone from being 0.2293, a positive (but not significant) value that was interpreted as supportive of the life cycle theory of consumption, to −0.3907, a negative and significant value that seems more difficult to reconcile with standard consumption theories. Maybe more relevant, the coefficient on lag services consumption has fallen from 0.9425 to 0.7873, a considerable reduction in the persistence of the variable over time.

Fair fully recognizes this feature of the equation by using an AP stability test to detect a break in the late 1970s. The break (perhaps a manifestation of a smoother change in the parameters over time) may be interpreted as an indication of changes in the underlying structure of the economy, as the reaction to policy regime changes, or as a sign of misspecification. In fact, detecting changes in coefficients is not a problem of the model, but a proof of the usefulness of employing econometrics to organize our thinking about the data. Interestingly, there is also considerable evidence of parameter drifting when we estimate DSGE models (Fernández-Villaverde and Rubio-Ramírez 2008).

Another possibility behind the changes in parameters over time may be the presence of stochastic volatility. There is growing

[6] This observation raises the issue of why more structural models are not estimated using more flexible stochastic processes for shocks.

TABLE 1
EQUATION 1, LOG (CS/POP)

RHS Variable	Estimate (1954:1–1993:2)	Estimate (1954:1–2002:3)
constant	0.087 (2.17)	0.0572 (1.48)
AG1	−0.2347 (−2.86)	−0.3269 (−4.40)
AG2	0.2293 (0.99)	−0.3907 (−2.91)
AG3	0.2242 (1.14)	0.7687 (4.89)
log $(CS/POP)_{-1}$	0.9425 (29.58)	0.7873 (19.31)
log $(YD/(POP*PH))$	0.057 (1.88)	0.1058 (−5.75)
RSA	−0.0009 (−3.93)	−0.0012 (5.75)
log $(AA/POP)_{-1}$	—	0.0172 (3.50)
T	—	0.0004 (4.42)

evidence that time-varying volatility is key to understand the evolution of the U.S. economy (Margaret M. McConnell and Gabriel Pérez-Quirós 2000 and Sims and Zha 2006). The most important phenomenon discovered by this literature has been the big drop in observed volatility of macroeconomic aggregates in the United States over the last several decades. Stock and Watson (2003) have coined the term the great moderation to refer to this observation. Even if least-square type estimates will still be consistent in the presence of heteroskedasiticity of the shocks, the standard errors will be misleading. Moreover, the small sample distributions of the estimates will cause an undue instability of the parameters. The only way to test whether we face parameter drifting or stochastic volatility of the shocks (or a combination of both) is to estimate models with both characteristics and hope that the data are sufficiently informative to tell us about the relative importance of each channel.

5.6 *Classical Estimates versus the Bayesian Approach*

Fair's work is firmly positioned within the classical framework for inference. Most econometricians will find his approach familiar and intuitive. This classical heritage contrasts with much of the estimation of DSGE models, which has been done predominantly from a Bayesian perspective.

This growing popularity of Bayesian methods is one of the newest aspects of the revival of macroeconometrics that we described in section 4. Researchers have been attracted to the Bayesian paradigm for several reasons. First, Bayesian analysis is a comprehensive framework for inference based on decision theory. This foundation allows a smooth transition from the results of the model to policy advice. Second, the Bayesian approach is well suited to deal with misspecification and lack of identification, both important problems in DSGE modeling (Fabio Canova and Luca Sala 2006; Iskrev 2007). Furthermore, Bayesian estimators perform well in small samples and have desirable asymptotic properties, even when evaluated by classical criteria (Fernández-Villaverde and Rubio-Ramírez 2004). Third, priors allow the introduction of presample information, which is often available and potentially informative. Finally, Bayesian methods are often numerically more robust than classical ones such as maximum likelihood.

Of course, there are also problems with the Bayesian approach. Many economists express discomfort about the use of priors and about the cost of eliciting them in highly parameterized models. Extensive prior robustness analysis is rarely conducted. Hence, it is often hard to assess the influence of priors in final estimates. Second, even the most robust numerical methods may hide subtle convergence problems. Finally, Bayesian thinking is still unfamiliar to many economists, making it difficult to transmit substantive results in an efficient way.

Given the different approaches to inference, a natural exercise would be to reestimate Fair's model with Bayesian priors (although we would need to take care to not be too influenced in the choosing of the priors by what we already know from 2SLS estimates). Then, we could assess the extent to which the estimates are different and whether the model's empirical performance increases or decreases. Such an experiment contrasting methodologies could be most instructive for both classical and Bayesian researchers and for policy makers interested in the use of macroeconometric models.

5.7 *Microeconomic Data*

An important feature of the MC model is the attempt to incorporate microeconomic data. For example, the demographic structure of the population appears in the consumption equations. Also, the flow-of-funds data are employed to link the different sectors in the model.

This is another aspect on which the large macroeconometric models of the 1960s and 1970s focused much attention and is an area where DSGE models still have much to learn. Most dynamic equilibrium models use a representative agent framework. In comparison, microeconometricians have emphasized again and again that individual heterogeneity is the defining feature of micro data (see Martin Browning, Lars Hansen, and James J. Heckman 1999 for the empirical importance of individual heterogeneity and its relevance for macroeconomists). Thus, DSGE models need to move away from the basic representative agent paradigm and include richer configurations. Of course, this raises the difficult challenge of how to effectively estimate these economies. Right now, DSGE models are estimated without individual heterogeneity just because we do not know how to do it otherwise. Hopefully, the next few years will bring improvements along this front.

5.8 *The Forecasting Record of the MC Model versus Alternatives*

One of the most staggering aspects of Fair's work over time has been his unrelenting determination to test his model, both in absolute terms, and in comparison with existing empirical alternatives.

A natural benchmark test is the model's forecasting record. We can go back in time and see how well the model accounts for the observations, both in sample and outside sample. However, before doing that and since we are dealing with aggregate data, the researcher needs to take a stand in relation to the revisions of the data and methodological changes. Statistical agencies are constantly revising data, both to incorporate further information (preliminary data release, first data release, second release, the first major revision, and so on) and to update their definitions to reflect advances in economic theory and measurement. The issue faced by all macro modelers is how to incorporate those changes in a consistent way. One possibility, followed by Fair (and that, personally, I think is the most reasonable), is to always use the most recent vintage of data. This amounts to asking the model to account for what we currently think actually happened.

This position, however, faces the difficulty that agents may be basing their decisions on real-time data. Hence, if we try to understand their behavior using final data, we may discover that we cannot properly explain their actions. We do not know much about the size of this bias. Perhaps, at the individual level, the effect is not very big because the agents, to a first approximation, need to know only a few prices (like the wages and the interest rate they face) that are directly observable. On the other hand, the government may find that using real-time data complicates their task substantially. Athanasios Orphanides (2001) has shown that real-time policy recommendations differed considerably from those obtained with ex post revised data during the 1970s.

Chapter 14 of the book compares the U.S. model with two alternatives—a seven variable VAR and an autoregressive components (AC) model where each component of output is regressed against itself and summed to generate forecasted GDP.[7] Fair finds that the U.S. model does better than the VAR and nearly always better than the AC model. Nevertheless, there is evidence that the VAR and the AC contain some independent information not included in the U.S. model (although this additional information seems small).

Chapter 14 motivates several additional exercises. First, we could include a Bayesian VAR in the set of comparing models. A common problem of classical VARs is that they are copiously parameterized given the existing data, overfitting within sample and performing poorly outside sample. By introducing presample information, we can substantially improve the forecasting record of a VAR. A second possibility is to impose cointegrating relations and estimate a vector error correction model (VECM). The long-run restrictions keep the model's behavior tightly controlled and enhance its forecasting capabilities in the middle-run. Third, we could estimate a VAR or a VECM with parameter drifting or stochastic volatility. Fourth, it would also be valuable to assess the performance of the rest of the world model. Finally, and in my opinion the most interesting exercise, would be to compare the forecasting record of the MC model with that of a modern DSGE model. Given the increased prominence of estimated DSGE models in policy making institutions, this evaluation will help us to gauge the empirical success of each approach.[8]

6 Concluding Remarks

Economists are inordinately fond of a Whiggish interpretation of our own history as a profession. Overfacile opinion in the humanities notwithstanding, the temptation of that teleological reading of our development is firmly anchored in the experience of decades of continuous increase in our horizons of understanding. Macroeconometrics is no exception to this progress. Despite some retreats in the 1970s and 1980s, we now have sounder models: better grounded in microeconomics, explicit about equilibrium dynamics, and flexible enough to incorporate a wide array of phenomena of interest. Better still, despite important remaining points of contention, the last few years have been marked by a growing consensus among macroeconomists about some fundamental outlines of the behavior of the economy.

Fair does not fully share such an optimist outlook of the history and status of modern macroeconomics. He thinks that a return to some of the programmatic principles of the Cowles Commission approach to econometrics is worthwhile and perhaps imperative. Some economists will find much to agree with in this book, while others will be less convinced. But Fair's position does not make his work any less valuable for this second group of researchers. As I argued above, even those economists more interested in the estimation of DSGE models have much to learn from the Cowles Commission tradition. Fair's book is an excellent place to do so. After all, both supporters of more traditional models and fans of DSGE models agree about a fundamental point: that building and

[7] There is a third model, Fair's U.S. model extended with eighty-five auxiliary autoregressive equations for each of the exogenous variables. I will not discuss this model because of space constraints.

[8] A first but incomplete step in this comparison is done in Fair (2007).

estimating dynamic models of the aggregate economy is possible and important for society's welfare. Coming back to the beginning of the review: high modernism still reigns undisputed in economics, and we are all much better off because of it.

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