

INET published a paper, written by John Kay, that deals with the relationship between economics and the world we live in. [The Map Is Not the Territory: An Essay on the State of Economics](#) spells out methodological critiques of economic theory in general, and of DSGE models and rational expectations in particular.

INET forwarded Kay's paper to a handful of economists and invited them to respond. Here we offer a perspective by Michael Woodford, John Bates Clark Professor of Political Economy at Columbia University.

What's Wrong with Economic Models? A Response to John Kay

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John Kay's thought-provoking essay¹ argues that economists have been led astray by excessive reliance on formal models derived from assumptions that bear too little similarity to the world we live in. And it is surely true that at least at times, disastrous decisions have been made through reliance on models that proved to be incorrect. Some of the statistical models used by financial institutions to value derivative securities based on mortgages just before the recent financial crisis provide a case in point.

But I do not believe that the route to sounder economic reasoning will involve an abandonment of economists' penchant for reasoning with the use of models. Models allow the internal consistency of a proposed argument to be checked with greater precision; they allow more finely-grained differentiation among alternative hypotheses, and they allow longer and more subtle chains of reasoning to be deployed without both author and reader becoming hopelessly tangled in them. Nor do I believe it is true that economists who are more given to the use of formal mathematical analysis are generally more dogmatic in their conclusions than those who customarily rely upon more informal styles of argument. Often, reasoning from formal models makes it easier to see how strong are the assumptions required for an argument to be valid, and how different one's conclusions may be depending on modest changes in specific assumptions. And whether or not any given practitioner of economic modeling is inclined to honestly assess the fragility of his conclusions, the use of a model to justify those conclusions makes it easy for others to see what assumptions have been relied upon, and hence to challenge them. As a result, the resort to argumentation based on models facilitates the general project of critical inquiry that represents, in my view, our best hope for some eventual approach toward truth.

¹ John Kay, "The Map is Not the Territory: An Essay on the State of Economics," September 2011. Posted at <http://ineteconomics.org/blog/inet/john-kay-map-not-territory-essay-state-economics>

Of course, Kay does not deny the usefulness of models as such. He admits that there are proper uses of models and of mathematical reasoning, yet argues that economists too often employ models of the wrong sort, computer simulations of “artificial worlds” that are mistaken for literal descriptions of reality. But I am not convinced by this proposed distinction. It is true that some useful models do not pretend to literally represent the world, and are intended merely to clarify the connections among general concepts or to allow qualitative rather than quantitative conclusions to be drawn. But often models are needed that are intended to be analogues of some actual economy, in the sense that mathematical quantities in the model are intended to represent things in the world, such as U.S. real GDP in the third quarter of 2011. For example, one may admit that in a situation that one wishes to analyze, several mechanisms will all simultaneously be in play, and one may wish to form a judgment about which of these factors are likely to outweigh the others as a quantitative matter. It is hard to see how one can form a judgment about such a question, except by reasoning with the use of a mathematical framework (at least a humble spreadsheet) in which the quantities in one’s model are intended to represent quantities in the real world, and the relations among the quantities that are assumed in one’s analysis correspond to relations that one believes should exist among the corresponding quantities in the world, at least to a useful approximation.

I believe, then, that it is inevitable that economic analysis will largely be conducted with the use of mathematical models, and that often these models will propose “complete descriptions” of “artificial worlds.” This does not mean, of course, that the conclusions obtained from such models should be regarded as applicable to the real world, simply because of the rigor of the reasoning used in deriving conclusions within the world of the model. An assessment of the realism of the assumptions made in the model is essential --- not, of course, an assessment of whether the model literally describes all aspects of the world, which is never the case, but an assessment of the realism of what the model assumes about *those aspects of the world* that the model pretends to represent. It is also important to assess the robustness of the model’s conclusions to variations in the precise assumptions that are made, at least over some range of possible assumptions that can all be regarded as potentially of empirical relevance. These kinds of critical scrutiny are crucial to the sensible use of models for practical purposes. They make the sound use of model-based reasoning harder, but they hardly represent an abandonment of model-based deductive reasoning. Indeed, the greater the extent to which one’s models provide complete descriptions of artificial worlds that are intended as analogues of real economies, the more obvious are the possibilities for such critical scrutiny of the validity of one’s reasoning.

There is, however, an important respect in which I do believe that much model-based economic analysis imposes a requirement of internal consistency that is unduly strong, and that may result in unnecessary fragility of the conclusions reached; and I suspect that this has a fair amount to do with the unease that Kay expresses about modern economic analysis. It has been standard for at least the past three decades to use models in which not only does the model give a complete description of a hypothetical world, and not only is this description one in which outcomes follow from rational behavior on the part of the decisionmakers in the model, but the decisionmakers in the model are assumed to *understand the world in exactly the way it is represented in the model*. More precisely, in making predictions about the consequences of their actions (a necessary component of an accounting for their

behavior in terms of rational choice), they are assumed to make exactly the predictions that the model implies are correct (conditional on the information available to them in their personal situation).

This postulate of “rational expectations,” as it is commonly though rather misleadingly known, is the crucial theoretical assumption behind such doctrines as “efficient markets” in asset pricing theory and “Ricardian equivalence” in macroeconomics. It is often presented as if it were a simple consequence of an aspiration to internal consistency in one’s model and/or explanation of people’s choices in terms of individual rationality, but in fact it is not a necessary implication of these methodological commitments. It does not follow from the fact that one believes in the validity of one’s own model and that one believes that people can be assumed to make rational choices that they must be assumed to make the choices that would be seen to be correct by someone who (like the economist) believes in the validity of the predictions of that model. Still less would it follow, if the economist herself accepts the necessity of entertaining the possibility of a variety of possible models, that the only models that she should consider are ones *in each of which everyone in the economy is assumed to understand the correctness of that particular model*, rather than entertaining beliefs that might (for example) be consistent with one of the other models in the set that she herself regards as possibly correct.

While the postulate of rational expectations does not follow inevitably from the desire to build complete models --- in the sense of models in which one’s assumptions are spelled out in sufficient detail to allow precise conclusions to be reached --- there is a sense in which a commitment to the postulate of rational expectations does require one to commit oneself to *more complete* models than would otherwise be necessary in order to address the question that one wishes to answer. For purposes of macroeconomic analysis it is typically necessary to specify structural relations --- say, an “Okun’s Law” relationship between the unemployment rate and real GDP, or a “Phillips curve” relationship between inflation and unemployment, or a “Taylor rule” to describe the behavior of the Fed --- that one may believe to have some current validity without necessarily expecting them to remain equally valid for all time. If the questions that one wishes one’s model to answer relate only to the evolution of aggregate time series over the next few years, it is not necessarily a problem that one would not be sure how to specify these relations far in the future, and so the unknowability of the future is not a reason to abandon any attempt at model-based analysis. But in the case of rational-expectations analysis, it is not possible to analyze questions about the near term without specifying structural relations far in the future --- technically, in many models, infinitely far into the future. This is because one cannot demand consistency between people’s expectations and what the model predicts unless the model also makes predictions about the future. But if the model makes those predictions about the future on the basis of expectations that people will be expected to have at that future time about a still more distant future, then it is also necessary for the model to make predictions about that still more distant future, and so on without end. As a result, “complete specification” of one’s model involves much more heroic assumptions, about the foreseeable eternal validity of particular structural relations, if consistency with the postulate of rational expectations is required.

At the same time, acceptance of the postulate of rational expectations makes it possible to regard models as complete explanatory frameworks, and to obtain fully determinate answers from them, even when they include only very partial descriptions of people’s actual situations. In reality, the

way that people act in particular choice situations is likely to depend not only on a bare specification of the choices available to them on that occasion, but on other aspects of their circumstances as well --- their history and their other engagements --- that determine the way that they perceive the particular choice situation when they encounter it. But the postulate of rational expectations implies that these contextual factors should be irrelevant --- except, that is, to the extent that the different aspects of people's lives *really are* interdependent, when correctly understood (something that the economic analyst will, at least for analytical convenience, happily assume not to be the case). Hence the style of modeling favored by rational-expectations analysis involves both radical abstraction from many aspects of people's current circumstances that we know a fair amount about, and heroic specificity about aspects of the future about which we know close to nothing. This is perhaps the aspect of the models that leads Kay to compare them to video games.

Is there an alternative? I do not think that it will involve simply going back, as Kay suggests, to simply estimating statistical relationships like Keynes's "consumption function" and treating them as structural, without embarrassment. It is not plausible that there should exist a reliable structural relationship between current aggregate disposable income and aggregate consumer spending, the coefficients of which can be discovered through a simple regression of historical time series for one variable on the other. Decisions about saving (or equivalently, about the portion of current income that should be spent on current consumption) are surely forward-looking, and based on anticipations about one's likely need for the savings at some future date. Hence it is hard not to believe that expectations about future economic conditions, and not simply people's current levels of income, are among the key determinants of consumer spending. From this it follows that naïve regression estimates of the "marginal propensity to consume" are almost certainly severely biased, owing to correlation in the historical sample period between the measured variations in disposable income and the expectational variables that have been omitted from the regression. If so, they are likely to prove unreliable as a basis for forecasting the effects of policy changes, such as the effects on aggregate economic activity of increased government spending.

This has been an important lesson of the "rational expectations" literature in macroeconomics developed by Lucas and others, and I do not think it should be forgotten. At the same time, the mainstream alternative developed in response to this critique --- according to which aggregate consumer expenditure is modeled as the solution to the Euler equation (a condition for intertemporal optimality) of a representative household, under the hypothesis of rational expectations, has difficulty matching the statistical properties of aggregate data too closely. In order to avoid making strongly counter-factual predictions, current-vintage empirical DSGE models commonly assume preferences for the representative household that incorporate a high degree of "habit persistence," so that even when solved under the assumption of intertemporal optimization under rational expectations, consumer spending will not jump sharply in response to events that (at least according to the model) should predictably change the future path of household income.² But the postulate of strong habit persistence

² See, for example, Lawrence J. Christiano, Martin Eichenbaum, and Charles L. Evans, "Nominal Rigidities and the Dynamic Effects of a Shock to Monetary Policy," *Journal of Political Economy* 113: 1-45 (2005); and Frank Smets

has not found much support from studies of the behavior of individual households. An alternative explanation for the observation of persistent departures from the predictions of the rational-expectations Euler-equation model under more standard preferences would be the existence of persistent departures of actual household expectations from those implied by the rational-expectations solution of the economists' model.³

The macroeconomics of the future, I believe, will still make use of general-equilibrium models in which the behavior of households and firms is derived from considerations of intertemporal optimality, but in which the optimization is relative to the evolving beliefs of those actors about the future, which need not perfectly coincide with the predictions of the economist's model. It will therefore build upon the modeling advances of the past several decades, rather than declaring them to have been a mistaken detour. But it will have to go beyond conventional late-twentieth-century methodology as well, by making the formation and revision of expectations an object of analysis in its own right, rather than treating this as something that should already be uniquely determined once the other elements of an economic model (specifications of preferences, technology, market structure, and government policies) have been settled.

A number of ways of modeling expectations in economic models, that relax the strong assumptions of the rational-expectations hypothesis, have already been proposed. Three examples of long-standing research programs of this kind (each going back fifteen years or more) are the analyses of "eductive stability" by Roger Guesnerie;⁴ the theory of "rational belief equilibria" proposed by Mordecai Kurz;⁵ and the study of the learning dynamics resulting from constant re-estimation of econometric models, most extensively developed in the work of George Evans and Seppo Honkapohja.⁶ These are fairly different approaches, but each seeks to explain behavior as consistent with intertemporal optimization; each proposes definite bounds on the expectations of economic agents, corresponding to conceptions of the requirements of individual rationality (though not assuming the kind of *coordination* of expectations assumed by the hypothesis of "rational expectations"); and each can be incorporated

and Raf Wouters, "Shocks and Frictions in US Business Cycles: A Bayesian DSGE Approach," *American Economic Review* 97: 586-606 (2007).

³ As examples of how alternative hypotheses about expectations can explain serial correlation in the growth rate of expenditure without the hypothesis of habit formation, see for example Christopher D. Carroll and Martin Sommer, "Epidemiological Expectations and Consumption Dynamics," working paper, April 2003; Ricardo Reis, "Inattentive Consumers," *Journal of Monetary Economics* 53: 1761-1800 (2006); and Fabio Milani, "Expectation Shocks and Learning as Drivers of the Business Cycle," working paper, August 2010.

⁴ See the papers collected in Roger Guesnerie, *Assessing Rational Expectations 2: Eductive Stability in Economics*, Cambridge, MA: MIT Press, 2005.

⁵ See the papers collected in Mordecai Kurz, ed., *Endogenous Economic Fluctuations: Studies in the Theory of Rational Beliefs*, Springer Verlag, 1997.

⁶ See, for example, George W. Evans and Seppo Honkapohja, *Learning and Expectations in Macroeconomics*, Princeton: Princeton University Press, 2001. Related approaches are discussed in Thomas J. Sargent, *Bounded Rationality in Macroeconomics*, Oxford: Oxford University Press, 1993.

into general-equilibrium macroeconomic models in which individual decision problems are as complex as those postulated in mainstream rational-expectations DSGE models.⁷

We are currently far from any consensus on which, if any, of these approaches will prove most fruitful for practical macroeconomic modeling. Further investigation --- not only of the logical structure of these theories but of their consistency with both observed behavior and available survey evidence on the changes over time in people's actual expectations --- will likely allow greater clarity than is possible at present. But it is not obvious that we should ever expect to obtain a theory allows an economic analyst to predict what people will *necessarily* expect in a given economic environment. In fact, both the "eductive stability" approach and the theory of "rational belief equilibria" only identify *sets* of possible beliefs (and hence possible outcomes) that are consistent with the proposed theoretical restrictions in a given economic model, rather than yielding unique predictions.⁸ I think that other approaches, such as the econometric learning models of Evans and Honkapohja, are best viewed as having a similar aim --- that is, as seeking to identify a range of plausible belief specifications in a particular economic model, rather than to provide a single prediction that can be expected on *a priori* grounds to be correct. A model of "least-squares learning," for example, provides a definite prediction, but only in the case of a particular assumption about the regression model that people use to make their forecast, and the theory does not identify a unique specification of the forecasting model that one should expect people to use in a given economic environment. A prudent use of such an approach for economic policy analysis would surely need to consider a variety of possible assumptions about the forecasting approaches used by economic agents, quite apart from the consideration that would be given to uncertainty about the correct specification of the economic environment.

This absence of a single clear prediction about how people should forecast is often considered to be a reason *not* to entertain such hypotheses, and instead to prefer the hypothesis of rational expectations, which aims to provide a unique prediction about expectations in a given economic environment.⁹ But a more sensible approach may be to accept that one should only expect one's model

⁷ Examples of intertemporal general-equilibrium monetary-macroeconomic models using the above approaches to modeling expectations include Roger Guesnerie, "Macroeconomic and Monetary Policies from the 'Eductive' Viewpoint," in Klaus Schmidt-Hebbel and Carl Walsh, eds., *Monetary Policy Under Uncertainty and Learning*, Santiago: Central Bank of Chile, 2007; Mordecai Kurz, "A New Keynesian Model with Diverse Beliefs," working paper, Stanford University, September 2011; Sergey Slobodyan and Raf Wouters, "Learning in an Estimated Medium-Scale DSGE Model," CERGE-EI Working Paper no. 396, Charles University, Prague, November 2009; and the work of Fabio Milani, cited above.

⁸ Other recent approaches that seek only to establish bounds on the range of possible beliefs include the "imperfect knowledge economics" proposed in Roman Frydman and Michael Goldberg, *Imperfect Knowledge Economics: Exchange Rates and Risk*, Princeton: Princeton University Press, 2007; and the "near-rational expectations" proposed in Michael Woodford, "Robustly Optimal Monetary Policy with Near-Rational Expectations," *American Economic Review* 100: 274-303 (2010).

⁹ In fact, rational expectations equilibrium is also indeterminate in many well-posed economic models, so that a large multiplicity of possible specifications of expectations are consistent with the hypothesis of rational expectations. See, for example, Roger Guesnerie and Michael Woodford, "Endogenous Fluctuations," in J.-J. Laffont, ed., *Advances in Economic Theory: Proceedings of the Sixth World Congress of the Econometric Society*, Cambridge: Cambridge University Press, 1992. Some respond to this difficulty by seeking to strengthen the

of the economy to deliver a range of plausible outcomes, rather than a unique prediction. This would not render models useless as guides to the selection of public policies; modeling approaches of the kind mentioned above all still imply very different sets of possible outcomes in the case of alternative policies, and a comparison of these sets can still provide a basis for preferring one policy to another. For example, one might seek to determine which policy ensures the highest *lower bound* for one's measure of social welfare, across the set of possible beliefs that are regarded as plausible possibilities in the environment that would be created by the policy.¹⁰

Allowance for a set of possible outcomes under a given policy would lead to an approach to policy design that would focus on the robustness of policy to possible variations in the way that the consequences of the policy are understood by people in the economy, rather than focusing solely on the optimality of the policy if events unfold precisely as planned. It should lead, for example, to a concern to design policies that make it more difficult for asset bubbles to occur, or that should reduce the economic distortions that result from them when they do occur, rather than ignoring these issues on the ground that in a rational-expectations equilibrium the bubbles should not occur. It should also lead to greater attention to the communication policies of central banks and other governmental actors, rather than assuming that official explanations of policy are irrelevant given that economic agents can be expected to have rational expectations --- and that these "rational" expectations depend only on governmental actions, not upon speech.

A great deal remains to be done in order to develop practically useful models in this spirit. However, the approaches mentioned above, and other related approaches, are currently the focus of a fair amount of research. Notably, an international research network has recently been organized, under the leadership of Roger Guesnerie, aimed at further understanding of the problem of expectational coordination.¹¹ The network intends to develop further the approaches to the modeling of expectations mentioned above, among others; to confront them with empirical evidence on the character of expectation formation; and to develop their implications for macroeconomics and financial economics in particular. This is, in my view, an agenda whose time has come.

If I am right, then, about the future of macroeconomics, we will not dispense with models, not even with the aspiration to build models that are intended to represent actual economies; and I suspect that even much of the conceptual scaffolding of current DSGE models will prove useful in constructing the models that come next. What we should outgrow, instead, is the aspiration to build models that can not only be regarded (at least provisionally) as correct representations of reality for purposes of policy analysis, but that can be *assumed to be self-evidently valid to everyone in the economy as well*. This will

hypothesis of rational expectations so as to achieve a unique prediction. An alternative approach treats the potential indeterminacy of expectations as a problem that one should seek to minimize through an appropriate choice of public policies. See, for example, the discussion of consequences for the choice of a monetary policy rule in Michael Woodford, *Interest and Prices: Foundations of a Theory of Monetary Policy*, Princeton: Princeton University Press, 2003, chapter 4.

¹⁰ An example of policy analysis in this spirit is provided in Michael Woodford, "Robustly Optimal Monetary Policy," cited above.

¹¹ See the announcement at <http://ineteconomics.org/grants/international-network-expectational-coordination>.

make the best way of modeling people's beliefs about the economy's future evolution an important topic of inquiry along with the other determinants of economic outcomes. The shift in perspective will have important consequences for the way that we seek to validate and parameterize our models, and likely even farther-reaching consequences for the way that models are used to assess policy proposals.