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Not Half Bad

A Modest Criterion for Inclusion

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Abstract

To understand a complex system (e.g., an economy, an ecosystem, the global climate system), scientists often rely on models. Models simplify reality by focusing on certain parts of a system, and the relationships between them, while ignoring, by necessity, others. Advocates of complexity theory often boldly claim (partly by virtue of greater realism) that they can improve upon the standard neoclassical economic framework. A much weaker claim supports the promotion of this new class of models or any class of models: even if the complexity framework makes less accurate predictions than the neoclassical approach, the complexity framework can be of use because its models differ.

Background

The focus of this Forum was the potential for complexity economics. Participants also advocated greater engagement with more accurate behavioral models as well as the adoption of some ideas from biology—notably group selection. These new models and ideas are often positioned as replacements to a flawed dominant model. Here, I stake a much weaker claim. I argue that these models deepen our understanding and can do so even if they are less accurate than existing models provided that they are sufficiently different.

To make sense of a complex system such as an economy, an ecosystem, or the global climate system, we rely on models. Those models simplify reality, focusing on certain parts of a system and the relationships between them and, by necessity, ignoring other parts. The Arrow-Debreu general equilibrium theory from economics, for example, includes preferences and production possibility sets, but rules out many types of externalities. It also does not address social influences on preferences (it takes them as given) nor does it include behavioral regularities that depart from rational choice.

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Those exclusions do not imply that all economists or even most economists deny social influence, externalities, or more behaviorally accurate models of behavior. In fact, a growing number of economic models include those features as well as networks, learning by doing, behavioral biases, identity, and social learning. The reason for making a stark assumption, be it tractability or parsimony, is less relevant than whether a model which excludes features of the real world can be useful. General equilibrium models, for all of their critics, hold up well to that criterion. Variants of general equilibrium models have been used with modest success to predict future outcomes, to understand past and current outcomes, to explain the results of past actions, and to guide and improve policy choices (for a list of uses of models, see Epstein 2008).

Modest success implies a degree of failure. And modern economics can be accused of some notable ones—including the failure to head off, much less predict, the most recent global economic collapse. Critics are justified in contemplating alternative paradigms (Colander and Kupers 2014). Challenges to the prevailing economic paradigm are particularly warranted given that the Arrow-Debreu general equilibrium model, the Solow growth model, and dynamic stochastic general equilibrium models all assume optimizing agents and convex production technologies.

This coherence of these core models separates economics from the other social scientists. Economics has a canon, which when presented in potted version can seem almost laughable: optimizing firms, identical consumers, perfect and common expectations about future prices, and a system in equilibrium or on the way to one. A closer and fairer consideration of the discipline while not denying conformity at the core—a standard set of introductory models used at the undergraduate and graduate level—balances this critique with a nod to the substantial diversity at the frontiers.

Over the past thirty years, economics has undergone substantial and substantive changes. Behavioral economic models, heterogenous agent models, network models, and dynamic adjustment models have all transitioned from the fringes to the mainstream. Many of those ideas (e.g., bounded rationality, diversity, networks, and dynamics) contradict the abridged version of economics taught to undergraduates, and trotted out by critics.

Many of the same new features that are animating economics also lie at the core of complex adaptive systems theory and evolutionary theory. Not surprisingly, owing to the excitement about complexity and the success of evolutionary theory, these features also motivate calls for a new paradigm. Complex systems scholars who advocate abandoning the equilibrium approach often construct two-column charts in which they compare the standard economic model with the complex systems approach. This volume contains one such example (see Table 18.1 in Gowdy et al., this volume). Words like rational, homogeneous, self-regarding, naïve group selection, and so on are used to typify the standard economic approach, whereas their opposites (bounded,

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heterogenous, other-regarding, multilevel selection, etc.) are used to describe the complexity approach.

The two-column chart proves persuasive to many because even the most cursory awareness of economic activity and processes points in favor of the complexity approach. People are not rational. We make mistakes. We are also diverse and, at times, prosocial. Most economic processes lack a central planner. What happens in the economy emerges. Most importantly, the economy does not appear to be in equilibrium. The combined value of all capital stocks often changes in value by one to three percent on any given day, a fact hardly in accord with a system at rest. The official disciplinary response—to the extent that one exists—is to say that the fact that the real economy is not in equilibrium does not imply that equilibrium models cannot be of use.

The reaction from economists to these critiques has been to absorb some of the assumptions of evolutionary theory and complexity theory, but not to abandon their core models or standard techniques. Ideas from evolution and complexity have become add-ons. To give just one example, network economics is now a standard course in most top graduate economics departments.

In other words, the modern economics profession might best be described as catholic in orientation: no reasonable assumption goes unmodeled. The discipline contains many useful models and ideas. The value of a model rests on whether it achieves its goal of predicting, explaining, aiding in design, or simply helping us to explore the set of possible ways to organize our economic lives. Owing to that, I see little justification for claims to toss out the old paradigm and replace it with a new one based on ideas from evolution and complexity. That places me at odds with some contributors to this volume. At the same time, I strongly believe that we need to construct and advance a set of new economic models based on complexity, as a complement and challenge to the existing approach.

My reasoning relies on two arguments. First, the standard model and the complexity model differ substantially. The value of a pair of models depends largely on how much they differ. It is far better to have two dissimilar models than two nearly identical models. Second, the complexity-based model need not be better than the equilibrium model to be of use. It need only be not substantially worse in some contexts. To state the argument even more weakly, the complexity framework could be less able to explain variation in any context, and substantially so, yet still add to our understanding.

Differences at the Core

To see why two models are better than one, we need some background on the value of diversity (Page 2007). A model consists of a set of assumptions. A modeler then either derives or computes implications of those assumptions. If two models differ in their assumptions, they will likely produce different

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predictions or explanations. Therefore, the first step in my argument will be to detail a few of the differences in the two types of model.

The most important difference between the complexity approach and the standard approach concerns the fundamental actors—humans. Economic models take preferences (and often fixed preferences) as a primitive assumption. Complexity models take behavior as a primitive assumption (Epstein 2013). Neither should be seen as better than the other. One of the contributions of economics has been to link axiomatic assumptions about preferences —transitivity, completeness, and continuity—to the characterization of utility functions. From those utility functions (assuming rationality), one can derive behavior. In the standard model, behavior is then derived in the context of the model. It is not assumed.

A canonical complexity-based model of the economy makes assumptions about behavior and (typically) how that behavior changes over time either by learning or through social effects (which can be conceived of as learning or as conformity). This does not mean that the standard model and the complexity model need be incommensurate. The assumed behavior in the complexity model could be consistent with optimal behavior given a set of underlying preferences.

Most often, though, this will not be the case. The behavior—both that which is assumed and that which emerges in a complex systems model—will not be consistent with a fixed set of preferences. One reason for this will be that social effects produce behavior inconsistent with fixed preferences.

The distinction between preferences and behavior becomes more than semantics when one considers richer models. In the standard model, individuals interact and choose their behaviors (whether rational or not). Those behaviors, in turn, effect beliefs about what will happen in the future. We can then think of the economy as an ecology of preferences and emergent beliefs. That is a defensible framework, and it has had successes and failures in predictive and explanatory roles.

In a complexity model, the ecology is viewed in terms of behaviors or, better stated, behavior rules. This is an equally defensible framework. It, too, has had successes and failures. One domain where it has been quite successful is in modeling disease spread. This may well be because the relevant behavior is low dimensional (primarily spatial).

The standard model can include bounded rationality, prosociality, network, heterogeneity, and so on, but it will still differ from the complexity approach because even though preferences and beliefs imply behaviors, the types of behaviors that will be produced will differ. Relatedly, though a model with heterogeneous preferences will produce heterogeneous behaviors, the distributions across behaviors will differ from the types of heterogeneity that would be natural to assume if one were to just assume diverse behaviors. Moreover, the behavioral approach creates a dynamical system. This is not true of the preference-based approach.

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Given the differences in assumptions, the complexity framework encourages us to focus our attention on different features of the world. As just mentioned, complexity models focus more on dynamics. They also see structure and diversity from a more functional perspective (Page 2015). These differences, as I shall argue next, bring value.

The Value of Distinct Models

Models perform many functions. They unpack logic. They help explain phenomena. They guide action. They assist the design of institutions, rules, and laws. And, they enable us to explore worlds different from our own, as exemplified by the artificial worlds literature from complex systems.

Here, for pedagogical purposes, I focus on a single use of models: to predict and explain data. I make this restriction not because model diversity does not improve our logic, better guide action, improve design, and expand the set of things we can explore, but because within the predictive context, the argument can be made analytically.

The point will not be to have a horse race between the two models or to stake a claim that either model is correct. All models, to quote George E. P. Box and Norman Draper (1987:74), "are wrong." Instead, the point will be to show that by having two models, we can make a more accurate prediction. To quote Richard Levins (1966:20), the truth will reside at "the intersection of independent lies."

The formal argument proceeds as follows: Imagine that we have data of some economic phenomenon, *X,* that we wish to predict or explain. This could be stock prices, unemployment, commodities prices, or even the number of people at the El Farol Bar. Suppose also that we have two models: an economic model and a complexity model. We can represent the total variation in the variable of interest as a box (Figure 17.1). Each model makes assumptions with the goal of explaining that variation. Each model simplifies the world. Each model implicitly creates categories in which distinct real-world observations have the same predicted outcome.

Consider first the economic model. As a first approximation, we can evaluate the model's predictive accuracy by R^2, the percentage of variation explained. The remainder of the box, the variation that's unexplained, the total

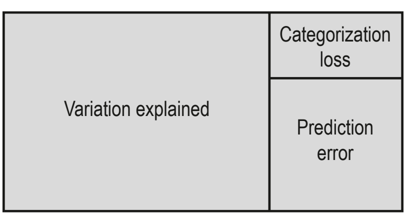

Figure 17.1 The components of total variation.

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model error, has two components: the categorization loss (the variation unexplained due to the categorization) and the prediction error (the squared difference between the mean value in each category and the predicted value for that category).

To state this formally:

$$\text{Total Model Error} = \text{Categorization Loss} + \text{Predictive Error.} \tag{17.1}$$

Let us next turn to the complexity model. As noted above, it makes different assumptions, which will manifest as a different categorization of reality and to different predictions. We can compare the variation explained by the complexity model to that of the economic model. However, as I will show under some rather mild conditions, combining these distinct predictions will be better than taking the better of the two.

The formal argument can be written as follows: Let E denote the prediction from the economic model and C denote the prediction from the complexity model. Let Θ denote the value of the outcome being predicted. Assume that we have N events indexed by i. Given this notation, the prediction by model $E(C)$ of event i will be written as $E(i)$ $(C(i))$ and the true value as $\Theta(i)$. We can then define the *model error* to equal the expected squared difference between the prediction and the true value:

$$\text{Model Error }(\text{E}) = \sum_{i=1}^{N}\left[E(i) - \Theta(i)\right]^2 \tag{17.2}$$

$$\text{Model Error }(\text{C}) = \sum_{i=1}^{N}\left[C(i) - \Theta(i)\right]^2 \tag{17.3}$$

The model error of the average of the two models, denoted by AVE, is given by the following expression:

$$\text{Model Error }(\text{AVE}) = \sum_{i=1}^{N}\left[\frac{E(i)+C(i)}{2} - \Theta(i)\right]^2. \tag{17.4}$$

This can be rewritten as:

$$\begin{aligned}\text{Model Error }(\text{AVE}) = \frac{1}{4}\Bigg[\sum_{i=1}^{N}\Big(\left[E(i)-\Theta(i)\right]^2 + \left[C(i)-\Theta(i)\right]^2 \\ +2\left[E(i)-\Theta(i)\right]\left[C(i)-\Theta(i)\right]\Big)\Bigg].\end{aligned} \tag{17.5}$$

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First, we will make an assumption that the predictions by the two models are independent.[1] Note that independence implies a substantial amount of model diversity. If the models make similar assumptions based on similar categories, their errors will be correlated. As a rule of thumb, the more distinct the models are, the less correlated their predictions. Formally, independence implies that the last term equals zero. We then obtain the following expression for the error of the two models:

$$\text{Model Error }(\text{AVE}) = \frac{1}{4}\left[\sum_{i=1}^{N}\left[E(i)-\Theta(i)\right]^2 + \left[C(i)-\Theta(i)\right]^2\right] \quad (17.6)$$

which can be simplified as:

$$\text{Model Error }(\text{AVE}) = \frac{1}{4}\left[\text{Model Error }(\text{E}) + \text{Model Error }(\text{C})\right]. \quad (17.7)$$

This expression implies that so long as the squared error of the complexity model (C) is not more than *three times* the squared error of the economic model (E), the average of the two models will be better than the economic model. Though the distinction between being better and adding value should be obvious, it is worth restating. To argue that the complexity model should replace the economic model requires that the complexity model has lower predictive error. To advocate that the complexity model adds value requires only that its error is less than three times that of the standard model. That's a low bar.

Perhaps the bar is too low. It relies on independence, a strong assumption. Let us, then, make a more conservative assumption that the third term (the correlation in the models) equals half the magnitude of the first term—the model error of the economic model. In this case, *the complexity model need only have less than twice the model error of the standard model to improve accuracy.* To borrow from Goldstein, McAfee, and Suri (2014), a second predictive model need only be "not half bad" to add value. The unassuming algebraic expression provides a powerful rationale for multiple diverse models.[2]

In presenting these criteria for inclusion, it should not be presumed that I believe that complex systems models will barely reach them. To the contrary, I expect that there will be many cases where the complexity approach wins by a large margin and where the standard model struggles to remain above the bar. Ideally, the two classes of models will produce a healthy dialogue and improve individually and collectively. That improvement need not lead to convergence, to a middle ground, and that's good. The formal argument presented

[1] This is an unrealistic assumption. Hong and Page (2009) show that it implies a unique relationship between categorizations for binary predictions. Nevertheless, it remains a useful benchmark for intuition.

[2] Goldstein et al. (2014) investigate and find support for the not half bad principle in the case of prediction markets using data.

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above shows that we don't want convergence. We want difference, meaningful difference.

Summary

As should be clear to anyone who reads this volume, the complexity approach has much to recommend it. The strengths of this growing new type of science has led advocates to claim that it should supplant standard economic models. That claim is partly based on the fact that the complexity approach makes more realistic assumptions. Pick one: Rational or behavioral? Equilibrium or dynamic? Linear or nonlinear? Based on realism, complexity wins. It is also partly based on the fact that a larger set of phenomena can be modeled.

The point of this brief note is that complexity models need not be seen as competitors to the standard models. One can make a weaker claim that complexity models need not even be as effective in order to add to our understanding. In fact, complexity models may need only be "not half bad" to increase predictive accuracy. They may need to be even less than "not half bad" to improve our ability to act, design, and explore. Thus, we should encourage and fund research on complex systems models of the economy. Our support for such models need not be predicated on them being better or even having the potential to be better. They only need to be sufficiently different to add value (Colander and Kupers 2014).

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