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Complexity, economics, and public policy

Steven N. Durlauf
University of Wisconsin, USA

Abstract
This article considers the implications of complex systems models for the study of economics and the evaluation of public policies. I argue that complexity can enhance current approaches to formal economic analysis, but does so in ways that complement current approaches. I further argue that while complexity can influence how public policy analysis is conducted, it does not delimit the use of consequentialist approaches to policy comparison to the degree initially suggested by Hayek and most recently defended by Gaus.

Keywords
complexity, agent based models, emergence, economics, public policy

1. Introduction

This article considers some of the implications of complex systems ideas for the study of economics and the evaluation of public policies. Positive and normative issues are both examined. My analysis largely ignores other social sciences, although much of the argumentation applies to applications of complexity to them. I focus on economics both because it constitutes my area of expertise and because the formal modeling approaches in economics can be compared relatively easily to the formal modeling approaches found in complexity science.

My discussion ranges across several interrelated issues. First, I argue that complexity thinking can enrich the way in which economists conceptualize various phenomena, but

Corresponding author:
Steven N. Durlauf, Department of Economics, University of Wisconsin, 1180 Observatory Drive, Madison, WI 53706-1393, USA
Email: sdurlauf@ssc.wisc.edu

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that this enrichment is complementary to current economic methodology. This claim is justified by considering a range of definitions of complexity in order to highlight the features of complex systems that differ from conventional economic reasoning. In doing this, I reject suggestions that complexity represents a new paradigm for the social sciences. On the other hand, I argue that certain properties of complex systems are of potential value to economists. Second, I consider the implications of complex systems thinking for policy evaluation. These implications derive from my claims as to what complexity adds to economic modeling. This discussion assumes standard (from the perspective of economics) modes of policy evaluation which are typically consequentialist. Third, I examine arguments raised in an important essay by Gerald Gaus (2007) which uses complexity-based ideas to question the utility of so-called expedient policies versus principled policies, and thereby provides a defense of views often associated with Friedrich Hayek. The expedient versus principled distinction in policies refers to choosing policies to achieve certain consequences. Gaus challenges the use of consequentialist criteria for policy evaluation on the grounds that complexity fundamentally limits the ability of a policy-maker to predict policy consequences. In my judgment, one should take a substantially more positive view of expedient policy actions than Gaus or Hayek, although their arguments are important in understanding the limits to expedient policy.

2. Complex models and the current state of economic science

In order to assess what complex systems methods can contribute to economics, it is naturally necessary to specify what is meant by a complex system. Unfortunately, there is no single accepted definition of a complex system. I therefore consider a number of candidate definitions to determine the implications of the proposition that the economy is complex. In exploring different definitions of economic complexity, my goal is not to identify a best definition per se, but rather to understand the implications of different definitions for the evaluation of positive and normative economic questions.[1] Also, it should be clear that the way in which these definitions are presented is not intended to provide anything deeper than an organizing framework to tease out how the notion of a complex economic system differs from conventional economic models and thereby allow one to understand how complexity can augment the current body of economic research.

One possible definition of a complex system is that it is a system comprised of a population of interacting, heterogeneous agents in which the behavior of each agent can be described as a function of the behaviors of other agents, as well as of other factors. This definition incorporates every model that (as far as I am aware) has been claimed to be an example of a complex system and captures two key ideas that motivate the study of such systems: the existence of a heterogeneous population, so that aggregate behaviors are understood to be regularities across a diverse population, and the specification of interdependences across the individual elements, so that the aggregate regularities constitute something more than the application of laws of large numbers to independently distributed observations. From the vantage point of this definition, it is a triviality to conclude that, as an empirical matter, the observed economy is an example of the realization of a complex system. Further, the baseline neoclassical general equilibrium model is a complex system under this definition, outside of the special case of representative

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agent models.[2] Indeed, the only nontrivial part of this definition is the assumption of heterogeneity, since any nontrivial model of heterogeneous economic actors will include interdependences in their decisions.

The fact that the neoclassical general equilibrium model fulfills my first candidate definition indicates that the definition does not capture the way in which complex economic systems are conceptualized by their advocates, since the rhetoric associated with economic complexity often claims that complex economic models constitute a direct challenge to the neoclassical approach. A second approach to the definition of a complex system is one that preserves the notion of a heterogeneous interacting population, but places requirements on the ways in which interactions occur, and so provides a distinction from neoclassical economic models. An example of this type of definition is proposed by Epstein (2006: 5):

> To the generativist, explaining macroscopic social regularities, such as norms, spatial patterns, contagion dynamics, or institutions requires that one answer the following question:
>
> *How could the autonomous local interactions of heterogeneous boundedly rational agents generate the given regularity?*

While this definition is developed with respect to what he regards as required for an explanation of a social phenomenon, it embodies substantive assumptions about how individual agents behave, since these substantive assumptions are required in order to construct a model that is generative in Epstein's sense. To be clear, Epstein's definition is proposed for so-called agent-based models, but this term means nothing more than that the models are dynamic and are studied using computer simulations rather than analytically, so I believe that it is fair to treat Epstein's definition as a candidate for a general description of a complex system.

Epstein's definition is of interest because it links complex systems with particular assumptions about the ways in which individual economic actors behave and markets function that stand in opposition to the assumptions of the baseline neoclassical general equilibrium model with uncertainty (see Arrow and Hahn, 1971). By the baseline model, I refer to a theoretical modeling environment in which individual economic agents are rational and possess common information sets, markets are complete (that is, all possible transactions can be implemented), and prices equate supply and demand in all markets at all points in time. In contrast, from Epstein's vantage point, complex systems, when applied to the economy, embody very different assumptions about individual behavior and about how individuals interact. This is true along two distinct dimensions.

First, individual actors are assumed to be boundedly rational. This typically means that the belief-formation process of each agent can be described as a simple function of certain past data available to each agent. Often, the available information is determined by the agent's direct interactions with others. In other words, agents are assumed to follow simple rules by which beliefs are formed and to form these beliefs on subsets of the information that is present in the economy as a whole. The idea that beliefs about the future are conditioned on information from the past, of course, says nothing about rationality per se. Applications of complex systems to contexts such as stock markets typically

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involve simple heuristics for belief formation that translate information from the past into beliefs about the future in ways that do not correspond to the predictive content of the past from the perspective of probability theory. This is where complex systems, under this conception, differ from neoclassical general equilibrium assumptions. To be clear, the baseline neoclassical general equilibrium model does not assume perfect foresight. Rather, it assumes that individual beliefs are rational in the sense that given an agent's information set, the agent's beliefs correspond to the probability statements that describe the environment under study. Further, the neoclassical model does not necessarily assume that agents have identical beliefs about the same uncertain event; this may occur in equilibrium, for example, if prices aggregate information in a certain way, but this is not guaranteed.

Second, moving beyond Epstein's specific language to the way in which complex systems typically work, the complex systems view of interactions that occur between agents is that they are often direct. In other words, interdependences between agents are not always mediated by what in economics is known as 'complete markets'. In the baseline general equilibrium model, agent interdependences occur because of prices; to put it more strongly, all interdependences occur because of the effects of others on market clearing prices. In complex systems, agents exhibit qualitatively different forms of interdependence. Learning may be observational, that is, one agent assesses the expected costs and benefits of a choice based on observing the experiences of others or by inferring information the other agent possessed from the agent's actions. Alternatively, because certain markets are ruled out, the interdependences of agents are different than would occur if markets were complete.

This second property, in turn, implies a third difference with the baseline neoclassical model, one which often is not made explicit: complex systems models typically treat markets as incomplete. This is implicit in Epstein's definition when he refers to local interactions; he is ruling out population-wide interactions. Hence, the existence of economy-wide markets such as the national market for Treasury Bills is absent from his conception. The absence of markets, in turn, means that complex systems often appear to be closer to models from game theory than models from general equilibrium theory. In fact, self-defined complex economic models are generally examples of population games, which are simply games with large numbers of agents. What I wish to emphasize is that relative to the baseline general equilibrium model, the interdependences between agents are typically restricted in various ways that generally involve direct interdependences as opposed to the interdependences that are implicit in market transactions.

Together, the assumptions of bounded rationality, direct interdependences, and incomplete markets constitute a clear alternative to the behavioral and institutional assumptions that underlie neoclassical general equilibrium models. These deviations from the baseline general equilibrium model of an economy under uncertainty can all be justified, in particular contexts, on empirical grounds, in the sense that the general equilibrium assumptions can be challenged. However, there are two difficulties regarding the complex systems approach, if characterized by the use of models that make some subset of the three assumptions I have identified, as a challenge to conventional economics.

First, I am unaware of any evidence that the alternative assumptions employed in complex economics systems, notably the types of bounded rationality and missing

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markets assumed in this body of models, are themselves empirically well motivated. It is one thing to say that individual behavior empirically deviates from a particular notion of rationality; it is quite another to argue that a particular deviation is empirically sensible. While the empirical literature in behavioral economics may give good reasons to modify the conceptualization of rationality employed in the economic models,[3] it does not justify the way in which rationality is replaced by alternative assumptions in the complex economics literature. To give a concrete example, Lux and Marchesi (1999) use a complex system with boundedly rational traders to show how stock-market booms and crashes may occur. Their model, however, also produces predictability in stock prices, which not only violates a well-established empirical regularity about stock price levels, but would mean that risk-neutral traders are in essence 'leaving money on the table' in the environment they study. The problem with Lux and Marchesi (1999) and much of the complex systems literature is that the rationality assumptions of neoclassical economics are replaced with alternative assumptions that render the agents, to put it bluntly, stupid. For the stock market, the interesting question, in my judgment, is how extremely smart (notice I do not necessarily mean 'rational' in the way economists conventionally define the term) agents can produce large swings in asset prices. This has not yet been accomplished by any contribution to the complexity literature.

Similarly, agent-based models which rely on local interactions assume that agents are arrayed in some plane or higher-dimension analog and assume rules for interactions that have no connection to reality. Markets are ruled out by assumption, as opposed to failing to exist for reasons such as transaction costs, coordination failure, and the many other reasons that economists have identified. The Axtell and Epstein (1996) Sugarscape model, for example, allows for local markets, but rules out others without any justification. Within economics there are decades of research on models with missing markets, but the accepted standard in the literature is to consider environments in which there are good reasons why the markets do not exist. To give an example far from complexity, in models of intergenerational mobility (for example, Loury, 1981), a key assumption is that parents cannot borrow against their children's future income, which is an easily justified assumption because of the unenforceability of such contracts. For both bounded rationality and incomplete markets, there may, I repeat, be empirical and theoretical reasons to justify these deviations from the baseline neoclassical model. However, economics does not advance when these deviations are introduced in an ad hoc fashion.

Further, the various deviations from neoclassical assumptions that one finds in complex systems, in particular those found in agent-based modeling, are conceptually far less radical than is often claimed; they are often nothing more than specific examples that fall into far broader areas of current economic research. In each case, these broader areas of research are unrelated to complexity per se. With respect to the relaxation of rationality, behavioral economics is one of the most popular areas in contemporaneous research. Prior to the emergence of behavioral economics, evolutionary game theory emerged as a subfield specifically to study how different equilibria emerged among boundedly rational agents. Agent-based models are nothing more than evolutionary games with certain assumptions on how agents behave. In addition, the relaxation of the assumption of common knowledge on the part of economic agents has been an active area of research for more than three decades. Social economics, which focuses on direct interactions such

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as peer groups and role-model influences, has also blossomed in the past decade, albeit to a lesser extent than behavioral economics. While one finds some complex systems in this literature (and this includes my own research), the systems are useful because they facilitate analytical mathematical calculations, produce interesting equilibrium properties, and allow theoretical models with social components to be brought to data. General equilibrium theory with incomplete markets has been studied since the 1980s and one could even argue that much of modern macroeconomics has involved the introduction of market frictions into earlier generations of aggregate models. So while complexity offers a particular perspective on the appropriate micro-foundations for economic theory, parts of this perspective are already embedded in contemporaneous microeconomic research. This new research has proceeded without any reliance on the mathematical properties of complex systems.

Is there any importance to my argument that complex economic models are a subset of broader efforts within economics to modify the baseline general equilibrium model? I argue that there is, because the instantiation of deviations from the baseline neoclassical model that have proven valuable to understanding the economy have proceeded without the use of complex systems modeling. Complex systems methods (and here I would identify agent-based modeling as particularly unsuccessful) have simply failed to produce substantive new insights into the economy. For example, while the study of imperfect information in economics, which is based on fundamental deviations from the baseline neoclassical model, has led to fundamental ideas such as adverse selection and moral hazard, there do not exist *any* comparable conceptual advances that have derived from complex systems analysis such as occurs when information assumptions on agent belief formation are restricted to local environments a la Axtell and Epstein's Sugarscape, for example. Complex economic models have been used to generate certain data properties of general interest, such as stock-market volatility, but have not provided new ways to understand economic phenomena. This is not surprising since the ways in which complex economic models have introduced deviations from what has been defined as the baseline economic model are, as argued above, ad hoc.

My criticisms of current applications of complexity to economics do not imply, of course, that substantive advances cannot occur from the use of complex systems methods. Anderson (1972) argues, in an ur-text for complexity advocates, that many-body physics is not reducible to particle physics; an analogy to economics might be drawn that the modeling of populations of economic actors may not be reducible to the modeling of individual decision problems. Such a claim would amount to an economics without the methodological individualism that lies at the core of modern economic theory (Becker, 1993). By 'methodological individualism' I mean the idea that one conceptualizes macroeconomic outcomes as the aggregation of well-posed individual choices, that is, choices that reflect an individual's preferences, constraints, and beliefs. The demonstration of such irreducibility in the context of particular economic phenomena would indeed be revolutionary, but this has not been done. Nor is it clear that it is achievable. In my view, there is no more important objective to economic research than the ability to predict the effects of policies under counterfactual scenarios. I cannot envision ways to do this which do not account for the ways in which individuals make decisions, such as forming beliefs about the future. Relative to Anderson, if a policy rule changes the

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behavior of each of the individual agents in an interacting population, it presumably must, outside of special cases, change how the aggregate of the agents behaves. It is the constancy of the particle physics behavior that allows the decoupling which he discusses. Similarly, suggestions that substantive economic conclusions can be derived from empirical regularities are illusory. As Heckman (2000, 2005) has argued, the properties of interest in economics, again including policy effects, may only be understood through the prism of a priori modeling assumptions; the data do not speak independently of models.

The observation that complex systems have been of interest in economics and elsewhere because of their aggregate properties, provides another way to define such systems. A third vantage point from which to define a complex system in a way that distinguishes it from my first definition is to characterize these systems by the qualitative regularities they imply for aggregate realizations of the systems. This is the approach taken by Mitchell (2009), who describes a complex system as follows.

> Systems in which organized behavior arises without an internal or external controller or leader are sometimes called *self-organizing*. Since simple rules produce complex behavior in hard-to-predict ways, the macroscopic behavior of such systems is sometimes called *emergent*. Here is an alternative definition of a *complex system*: a system that exhibits nontrivial emergent and self-organizing behaviors. (Mitchell, 2009: 13)

From this perspective, complexity science has two goals. First, it attempts to understand how microscopic interactions lead to macroscopic outcomes. Second, it is interested in emergent properties, which, following Crutchfield (1994), are perhaps better defined as properties that occur at a different level of aggregation than the description of the components of the system. So, to be concrete, ice is an emergent property of a collection of interacting water molecules. In some respects, the properties that Mitchell associated with complex systems also apply to neoclassical general equilibrium theory. The theory describes agents making interdependent choices that determine aggregate price and output levels. Further, general equilibrium environments can exhibit emergent properties. The most famous example is Adam Smith's invisible hand, whose model instantiation is the first welfare theorem of economics and says that a competitive equilibrium (of course, with many assumptions) will be Pareto optimal, so that no individual can be made better off without making someone else worse off.[4] Another example is Schelling's famous example (1971) of how complete racial segregation can emerge in a population in which no individual is especially racist; while his model can be cast as an example of a complex system, his analysis did not require this, nor do formalizations of Schelling's work such as Young (1998: ch. 3, section 5).

Nevertheless, Mitchell's definition highlights an important aspect of the value of complex systems analysis to economists. In general equilibrium theory, there is a celebrated result known as the Sonnenschein theorem,[5] which, roughly speaking, states that given aggregate data for an economy, one can always specify a description of individual agents who, in a complete-market, general-equilibrium context, make decisions that give rise to the observable aggregates.[6] In order for general equilibrium theory to have empirical content, it is necessary to delimit the heterogeneity in the economy in some way. One

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popular resolution of heterogeneity in macroeconomics is the so-called representative agent model, which assumes that aggregate economic activity can be treated as if it were generated by a single economic actor. Much recent research in macroeconomics has introduced heterogeneity into macroeconomic modeling explicitly in order to understand how heterogeneity matters for specific contexts, a leading example being an environment in which idiosyncratic risk cannot be fully insured away because of market frictions. This approach is well surveyed by Heathcote et al. (2009). Outside of macroeconomics, recent approaches have focused more directly on understanding what sorts of assumptions on heterogeneity can lead to aggregate implications. Research on the empirical implications of restrictions on heterogeneity for aggregate demand systems, summarized in Hildenbrand (1994), is an important and (relatively) recent development in this regard. Emergence is an example regarding which complexity has something important to offer economists, as it provides a constructive mechanism for overcoming the Sonnenschein theorem. Emergence is of interest for reasons beyond the Sonnenschein theorem, which is specific to general equilibrium models. Much interesting empirical and theoretical work has been done on what appears to be the ubiquity of Zipf's law (or, more generally, power laws), in contexts ranging from the size of firms (examples include Axtell (2001) and Luttmer (2007)) to cities (examples include Gabaix (1999) and Ioannides and Overman (2003)) and finance (an example of which is Gabaix et al. (2008)). While I have been critical of the strength of the empirical evidence for power laws in socioeconomic data (see Durlauf, 2005), these criticisms involve the question of whether tests for Zipf's law have sufficient power to discriminate its presence from various fat-tail alternatives. Regardless, this literature has the value of documenting empirical regularities that can help guide theories of these various phenomena, although as Brock (1999) insightfully argues, a limit of some of this empirical work is that it focuses on objects that may not be of intrinsic interest to economists.[7]

Emergence is not the only property of complex systems that is of interest to social scientists. A cognate property of emergence is universality, which means that for large classes of specifications of the details of a complex system, the aggregate properties of the system are qualitatively similar. This is of enormous importance since features of an interacting environment, such as the precise network structure that links individuals, are not observable outside of special cases, and so it is critical to be able to work with models which are universal in the sense I have described. The emergent properties of complex systems interact with the substantive behavioral assumptions that one makes about individuals, of course. Notice that universality does not imply that 'anything goes'. Universality holds for broad classes of models with interaction structures, as will be shown below in the context of the Brock-Durlauf model. Universality, on the other hand, does not mean that aggregate outcomes are independent of whether agents form beliefs myopically or rationally.

A third interesting property of complex systems is phase transition. A system with phase transitions has the property that its qualitative features can change abruptly around certain parameter values. Phase transitions represent a counterpoint to universality in that small differences in some feature of the system can lead to large differences in the behavior of aggregates. A classic example of a phase transition in nature is the change of water from solid to liquid at 0°C (32°F). Socioeconomic phase transitions are not readily

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identified from casual observation, given the many factors which affect individual choices. However, the nonlinearities that are embedded in complex systems suggest that the phenomenon is empirically possible. Further, even if one has not observed a phase transition for a given population of individuals, its possibility matters. In evaluating the effects of a policy on average behavior, it is possible for a population to be so configured that a small change in private incentives causes a qualitative change in the community. To make this concrete, consider the effects of a program offering college scholarships on high-school graduation in a community. The bang per buck of the program will be very high if the population's behavior is near a phase transition point between a low- and high-graduation-rate community.

Therefore, in understanding what complexity brings to economics, one should understand the complexity perspective at two levels. First, complex economic models have embodied a substantive challenge to particular theoretical assumptions concerning the rationality of individual agents and the degree of market completeness in the economy – assumptions that are common, but by no means universal in mainstream economics. Second, complex models provide a mathematical or computational framework for the analysis of the aggregate properties of populations of heterogeneous agents. However, neither of these dimensions constitute a challenge to the core logic of economic models, which in my view has two irreducible elements: (1) modeling individual decisions as purposeful and (2) deriving the aggregate consequences of these decisions according to whatever rules adjudicate the interdependences across agents. To say that an individual is boundedly rational is a statement about how he makes decisions, not a statement about whether he should be modeled as doing so. To say that rules exist to characterize the effects of interdependences between agents does not say what these rules are. The rules can be as simple as saying that my behavior at one point in time is a function of what my neighbors did in the last period or as sophisticated as requiring that prices equate supply and demand in every market. This is why it is possible to talk about fixed-price equilibrium in an economic model, which is an equilibrium in which prices are predetermined, and so supply is not equated to demand. Similarly, this is why evolutionary game theory represents a class of economic models. I acknowledge that other economists or social scientists might well argue that economics should be further delimited to focus on certain phenomena (such as unemployment, as opposed to, say, dialect use) or emphasize the role of prices and markets. But I reject these narrow conceptions as economics has evolved into a way of thinking about social problems. The 'economic imperialism' often associated with the name of Gary Becker, who extended the perspective I have described to contexts ranging from the family to crime, is consistent with my broad-church perspective as to what constitutes the basis of 'the economic way of looking at behavior' (Becker, 1993).

This view of complexity and its relationship to 'standard' economics is very different from what one sees both in the popular media and in writings (rarely by economists) that assert that complexity represents a paradigm shift for economics in particular and the social sciences in general. One reason for such grandiose claims is that there appears to be a widespread misunderstanding of the way in which economists think about economic theory.[8] The baseline general equilibrium model does not constitute the basis on which economists think about the economy, but rather provides a logical template

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to channel thinking. Arrow and Hahn (1971: vi–vii), in their classic textbook, provide a good description of the nature of general equilibrium theory as used by economists:

> There is by now a long and fairly imposing line of economists from Adam Smith to the present who have sought to show that a decentralized economy motivated by self-interest and guided by price signals would be compatible with a coherent disposition of economic resources that could be regarded, in a well defined sense, as superior to a large class of alternative dispositions. Moreover, the price signals would operate in a way to establish this degree of coherence ... The proposition having been put forward and very seriously entertained, it is important to know not only whether it *is* true, but also whether it *could* be true ... If confirmation of the proposition we have been discussing has been found in a particular formalization of the economy, it then becomes interesting to see how robust the result is ... the point is this: It is not sufficient to assert that, while it is possible to invent a world in which the claims made on behalf of the 'invisible hand' are true, these claims fail in the actual world. It must be shown just how the features of the world regarded as essential in any description of it also make it impossible to substantiate the claims. In attempting to answer the question 'Could it be true?', we learn a good deal about why it might not be true.

Arrow and Hahn make clear that the invisible hand is appropriately understood to be an idealization which may or may not be useful in a particular 'real world' context. Disagreements among economists on issues such as the degree of government intervention typically involve disagreements about the extent and nature of the deviations from the idealized general equilibrium environment.

Claims about complexity replacing conventional economics[9] suffer from a second problem, namely, the incorrect equating of formal economic theory with the entire body of economics, a much larger body of research which incorporates both empirical economics and policy evaluation. While some empirical economics involves the full delineation of an economic environment, so that empirical analysis is conducted through the prism of a fully specified general equilibrium model, other forms of empirical work use economic theory in order to guide, as opposed to determine, statistical model specification. Further, a distinct body of empirical economics is explicitly atheoretical, employing so-called natural experiments to evaluate economic propositions and to measure objects such as incentives. Policy-evaluation research can be decomposed similarly. The bottom line is that much of what constitutes economic knowledge is empirical.

There is another perspective which is often invoked in distinguishing complex systems approaches from neoclassical economics, namely, that complex economic models introduce dynamical considerations that are absent from standard methods. This claim is that complex systems approaches represent a challenge to conventional economics because they explicitly focus on how a given socioeconomic property represents the limiting behavior of a dynamic process and, as such, represent a 'gold standard' for social science modeling. Epstein (2006: 51) provocatively summarizes this view as

> The motto, in short is ... If you didn't grow it you didn't explain it.

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This dynamical systems challenge thus involves a choice of definition for what it means for a model to have explanatory value. From the perspective of the philosophy of science, it is easy even for an outsider to philosophy, such as myself, to see why Epstein's claim is not persuasive. I take it as well accepted by any philosopher of science that science has multiple objectives; this is beautifully summarized in Putnam (1987). For example, one objective of science is *understanding*, which is distinct from *explanation*. Many complex systems are black boxes, employed because the mathematical system of interest is too complicated to study using analytical tools. The outcomes of such exercises may be explained in the sense that they derive from a set of rules for interactions, but the nature of the analysis can preclude understanding of how these phenomena arise from the interactions that take place. An example of this is the use of complex systems to study stock-market volatility; these models explain that boundedly rational agents whose behavior is correlated can produce large swings, but it is not clear why the swings take the forms they do or why the magnitudes are so large.[10] In contrast, the efficient-markets hypothesis, whose logic may be (crudely) summarized as saying that stock-market participants quickly eliminate excess profit opportunities and thereby encode information relevant to stock prices into the prices, provides an understanding of a range of phenomena. Among these I would include (1) the general finding that over short horizons, stock prices essentially obey a random walk, (2) the observation that orange juice futures prices seem to encode better information about short-run weather fluctuations than official government forecasts of the weather, and (3) the absence of substantial financial sector profits given the level of financial market activity, that is, the fact that as a fraction of the total volume of transactions, the profits of the financial sector are miniscule.[11]

On its own terms, Epstein's definition suffers from what in economics is known as an 'identification problem'.[12] Namely, even if his concept of explanation is correct, it begs the question of what is learned if a given model generates a particular phenomenon. The black-box nature of agent-based modeling, at least in its current state, makes it impossible to assess whether very different structures can produce the same answer. Identification problems also plague so-called 'econophysics', which involves the application of tools physicists have developed to study large populations of objects to economic data. Econophysics tools have developed in the context of physical theories that predict certain aggregate properties, and so focus on the measurement of these properties. The utility of these tools for the social sciences is, however, limited by the fact that they were developed in very different empirical contexts than those that face a social scientist. Social scientists often employ data sets of limited size, which raise issues of the accuracy of estimates that do not arise in natural science contexts. A nice example of this problem is illustrated by LeBaron (2001), who demonstrates that claims of power laws in asset-price returns, one of the main claims of the econophysics literature, are fragile in the sense that very similar phenomena can be generated by stochastic processes (stochastic volatility models) that are standard objects in the financial economics literature. The response to this argument by Stanley and Plerou (2001), the former being the originator of the term 'econophysics', indicates that identification problems are simply not part of the thought space of physicists as they evidently do not understand the point of LeBaron's article, which is that economic data do not speak for themselves.

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Finally, attacks on mainstream economics by complexity advocates, such as those made by the prominent economist Alan Kirman (2010), ironically ignore the fact that the discipline of economics is an evolving complex system. What I mean by this is that the body of economic theories is constantly being empirically assessed and adapted in response to identified shortcomings. This adaptation occurs both in response to scholarly research, such as controlled experiments that have led to the flourishing of behavioral economics, as well as in response to major economic events such as the 2008 financial market meltdown, which has led to a renaissance of research on bubbles and crashes. I am aware that this claim has been frequently challenged by critics of economics, but a full defense of my position would require a separate article. Instead, I briefly describe the interplay of theory and empirics in terms of modern macroeconomics.

To oversimplify a complicated history, the rise of rational expectations and dynamic approaches to macroeconomics, led by Robert Lucas and Thomas Sargent, occurred because of the failure of conventional macroeconomic theory at that time to account for the changes in the relationship between unemployment and inflation as well as because of logical problems with the theory. Recently, the empirical limitations of these models, many of which were documented by Lars Hansen and Thomas Sargent,[13] have led these authors to explore macroeconomic environments that relax the rational expectations assumption, in the sense that agents are no longer assumed to know the true model of the economy. In these environments, this model uncertainty is structured in a way such that individuals cannot even assign probabilities to the different models that they believe may describe the economy. This leads Hansen and Sargent to employ minimax decision-making criteria, in which individuals, in essence, assume when making decisions that the least-favorable model (to them) will turn out to be the correct one. This is known in economics as 'robust' decision-making and will be discussed in more detail below. Here, it is sufficient to observe that Hansen and Sargent (2010) have shown how a transition from the conventional rational expectations approach to one that allows for robust decision-making on the part of economic actors can facilitate understanding asset-price movements.

I make this seeming digression for two reasons. First, claims about the empirical failings of economics do not justify a paradigm shift to complex systems approaches of the type one sees in agent-based modeling and elsewhere. The body of economic theory is itself adaptive and the failure to incorporate complex systems may speak of the limited value of these tools, not to resistance on the part of economists. Second, to a large degree, no empirical case has yet been made for complex systems approaches. I am unaware of any serious effort, for example, to show that a complex systems view of the macroeconomy provides better predictions than the current generation of dynamic, stochastic general equilibrium models in macroeconomics.[14] As for claims that complex systems models predicted the financial crisis of 2007, I have seen no evidence that these models also predicted the absence of a crisis for the previous several decades. The fact that a model can produce a financial crisis does not empirically support it; any number of conventional economic models can do the same; the question is whether a given model made a real-time prediction that is something deeper than a stopped clock giving the correct time twice per day. Nor does there exist a body of methods for empirical assessment and internal consistency checking for complex systems models that correspond to the

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econometric tools for model comparison and specification testing that are routinely applied to conventional economic models.

2.1. An example of a 'simple' complex system

In order to see how a complex system can contribute to economic analysis, in this subsection I describe a set of models I have developed in joint work with William Brock which employ tools from statistical mechanics to develop a set of models of interdependent discrete choices and which may be used to understand how social factors can influence a range of aggregate socioeconomic outcomes (Brock and Durlauf, 2001a, 2001b, 2006, 2007). Examples of such phenomena range from rates of cigarette smoking to nonmarital fertility to graduation from high school to criminality, in which one can think of these behaviors as chosen from a discrete set and in which one wants to account for social influences in these choices; for these examples and many others, social influences have been argued to be an important determinant of individual behavior.[15]

I describe a binary-choice version of this class of models, largely for simplicity. A particular version of the binary-choice model I describe turns out to be mathematically equivalent to the mean field approximation of the Curie-Weiss model of ferromagnetism, which is a canonical example of a complex system in physics. Variations of the model can produce mean field approximations to spin glasses, which are the canonical version of a complex system (see Durlauf, 1997). The model illustrates how ideas of emergence, universality, and phase transition can arise in a complex socioeconomic model.

The objective of the model is to describe the choices of a population of I individuals, indexed by i. Following Brock and Durlauf (2001a, 2001b, 2007), choices, denoted as ω_i, are coded so that they lie in the set $\{-1,1\}$. For example, -1 can denote *had a child while a teenager*, while 1 denotes *did not have a child while a teenager*, if one is studying teenage fertility. This model is directly derived from an individual decision problem. Each choice is associated with a payoff level $V_i(\omega_i)$. Individuals are members of a common group. The difference between the payoffs for the two choices is assumed to be additive in the different factors that have been defined:

$$V_i(1) - V_i(-1) = h_i + J_i m^e_{i,g} - \beta^{-1}\varepsilon_i. \tag{1}$$

Here h_i is a measure of i's private, deterministic incentive differences between the choices. The idea is that some factors, such as education, are measurable; I use a scalar for such factors for convenience. The beliefs that i has about average group behavior is measured by $m^e_{i,g}$; if $J_i > 0$, then there is a tendency for individuals to make similar decisions.[16] Finally, ε_i is a random, private utility differential, and so is not observable to the analyst; each of these unobservables is assumed to be drawn from a common, known distribution function $F_\varepsilon(\ )$. The parameter β measures the degree of unobserved heterogeneity, that is, when β is small, then heterogeneity is large. Individual i chooses 1 if, and only if, $V_i(1) - V_i(-1) > 0$, which is to say that an individual acts rationally in the sense that he makes the choice that makes him best off. Since

$$\Pr(V_i(1) - V_i(-1) \geq 0) = \Pr\left(\varepsilon_i \leq \beta_i h_i + \beta_i J_i m^e_{i,g}\right) = F_\varepsilon\left(\beta_i h_i + \beta_i J_i m^e_{i,g}\right), \tag{2}$$

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it must be the case that

$$\Pr(\omega_i = 1|h_i) = F_\varepsilon\left(\beta_i h_i + \beta_i J_i m^e_{i,g}\right). \tag{3}$$

To complete the model, I assume that expectations are rational, which means subjective beliefs correspond to the group-level means

$$m^e_{i,g} = m_g = 2\int F_\varepsilon\left(\beta h + \beta J m_g\right) dF_{h,\beta,J|g} - 1. \tag{4}$$

Here $F_{h,\beta,J|g}$ is the empirical within-group distribution of h,β,J. The description of a process for individual choices combined with its associated self-consistency condition fully specifies a model.

This general description of the equilibrium value of m_g illustrates that there is an interplay of private incentives and social incentives in the determination of the equilibrium. Hence, the model's behavior is sensitive to the density of individual-specific parameters $F_{h,b,J|g}$. If these parameters are constant across individuals, so that $h_i = h, \beta_i = \beta, J_i = J \forall\, i$, then the average choice in the population reduces to a simple nonlinear equation:

$$m_g = 2F_\varepsilon\left(\beta h + \beta J m_g\right) - 1. \tag{5}$$

The simplified model's equilibrium description (Equation 5) has a number of interesting properties. Fixing β, one can prove that for any value of βh there exists a threshold T_1 such that if $\beta J > T_1$, there are at least three solutions to Equation 5, whereas if $\beta J < T_1$, then the solution to Equation 5 is unique. Equivalently, for a given value of $\beta J > 1$ there exists a threshold T_2 such that if $\beta h < T_2$, there exist at least three solutions to Equation 5, whereas if $\beta h > T_2$, the solution to Equation 5 is unique. The roles of h and J are intuitive. Relatively large values of J compared to h mean that incentives to conform to others can swamp the tendency of private incentives to lead individuals to a given behavior. Why does the value of β play a role? The degree of heterogeneity in the population is measured by β. When this heterogeneity is large (β is small), then the values of these private determinants of individual behavior will often be large in magnitude and thereby swamp the interdependences in terms of the qualitative behavior of the population as a whole.

The multiple solutions to Equation 5 represent multiple equilibria; in other words, the microeconomic structure of the model does not uniquely determine its aggregate properties. Intuitively, if interdependences across agents are strong enough relative to private incentives, then the behavior of a group is no longer uniquely determined. When $h \neq 0$, it is possible for an equilibrium set of choices to occur such that a majority of the population makes choices with one sign, while h has the opposite sign. This is interesting because such an equilibrium is socially inefficient. The interaction effects lead the population to make average choices that are inconsistent with the choices that would be generated by their private incentives. In other words, while all agents are individually rational, the population is collectively acting in an undesirable fashion. This is one way to understand how poverty traps can occur: bad initial conditions lead to behaviors that become self-reinforcing (see Durlauf, 2006b). Furthermore, the equilibrium behavior of the model qualitatively changes around the values T_1 and T_2. The property that the qualitative features of a system can change abruptly around certain parameter values is

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known as a 'phase transition'. Phase transitions represent a counterpoint to universality in that small differences in parameters can lead to large differences in the behavior of aggregates.

Finally, one can see that the properties of the aggregate equilibrium exhibit universality, that is, they do not depend on a specific interaction structure that has been chosen, in which each agent reacts to the expected average of the entire population. Suppose the index i constitutes integers $1, \ldots, I$. Envision the agents as located on a circle and interpret the distance between agents i and j as $|i-j|$. Finally, assume that the locations on the circle imply that $|1-I| = 1$. One can then consider a model of local interactions between agents in which each agent is only affected by the behavior of his nearest neighbors. In other words, one can replace the payoff structure in Equation 1 with the following payoff structure:

$$V_i(1) - V_i(-1) = h_i + \frac{J_i}{2} \sum_{|j-i|=1} \omega_{j,g}^e - \beta_i^{-1} \varepsilon_i. \tag{6}$$

The circle is thus simply a device to ensure each agent has two neighbors that are 1 unit apart from him. One can show that under the simplifying assumptions to eliminate parameter heterogeneity that I have made (that is, $h_i = h, J_i = J \,\forall\, i$), the average choice level is still described by Equation 5. This model is mathematically equivalent to the mean field approximation of the Ising model of ferromagnetism.

Relative to the sorts of substantive behavioral assumptions made in the complexity literature, note that this model did not need bounded rationality to produce the properties I have described. The introduction of bounded rationality would have made the system more complicated, but produced qualitatively similar aggregate features, so long as the beliefs of individuals about average behavior are monotonically related to the mathematical expectation of average behavior. Other forms of bounded rationality could, of course, produce qualitatively different results; this has yet to be explored.[17]

Similarly, the properties did not require any consideration of dynamics. Introducing dynamics is done in Blume and Durlauf (2003). All of the interesting features derive from the interaction structure, including the emergence of multiple equilibria, and do so employing standard microeconomic reasoning. I started with individual decision problems, considered optimal decisions based on the factors that determine relative payoffs under different choices, and produced an equilibrium by requiring that the interdependences between the individual decisions obey some sort of self-consistency condition, in this case, that beliefs are rational with respect to the actual properties of the aggregate choice levels in the population. This is standard operating procedure in economics.

This model also can be used to illustrate the dangers associated with importing physics models into economics and simply relabeling the objects in complex physical systems as economic actors. The model I have described started with individual decision problems, from which conditional probability statements about behavior are derived and from which probabilistic descriptions of the population as a whole are generated. Complex physical systems start with the conditional probabilities; their users typically provide *ex post* justifications for the conditional probabilities as descriptions of behavior.

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But this can go awry. For example, Brock and Durlauf (2001a) give an example of this model such that if a social planner were to set individual choices in order to maximize utility, this would amount to replacing the expected average m_g with the actual average of the choices in the population. In physics, this replacement gives the full Curie-Weiss model as opposed to its mean field approximation. Physicists use the mean field approximation to facilitate computations, since the full model is analytically intractable. But for economists, they are substantively different models.

3. Implications for economic policy: microeconomics

The simple model presented in Section 2 illustrates what I regard as some of the main messages of complex systems thinking for policy evaluation. These messages follow from three general features of complex systems: (1) nonlinearity, (2) multiplicity of equilibria, and (3) phase transitions.

Abstractly, nonlinearity, as manifested in Equation 5, represents a challenge to policy evaluation because its presence implies that it is difficult, based simply on available data, to infer policy effects when policies lie outside the range of past experience. This is a standard problem in statistics and econometrics and is known as the 'extrapolation problem' (see Manski, 2007: section 1.4). This problem is not necessarily insoluble, in the sense that it is a statement about the statistical properties of data when the data are analyzed without recourse to social science theories. Put differently, the nonlinearities intrinsic to complex systems suggest the importance of structural estimation. If one has an econometrically implementable behavioral model of individual decisions (one example of which is the Brock-Durlauf model), then one can evaluate how these decisions will change under counterfactuals. Therefore, one implication of the complexity perspective, in my view, is that the role of economic theory in policy evaluation is enhanced, not diminished, because economic theory can provide guidance on the nature of the nonlinearities that policy needs to address.

A second implication of complexity for public policy is that it introduces complications in terms of equity/efficiency tradeoffs. Consider the complex model I described for binary choice and suppose that the binary choice in question is high-school graduation. The government has a fixed amount of income to allocate to college scholarships. One strategy is to allocate the money equally across 10 high schools; another is to concentrate the scholarships among students within one high school. Why might the latter provide more bang per buck? One reason concerns multiple equilibria. It is possible that by concentrating all resources in one school, the low high-school-attendance equilibrium could be eliminated. Moreover, even if the equilibrium is unique, the presence of interdependences in individual choices can create social multipliers (Glaeser et al., 2003), so that the equilibrium effects of a change in private incentives (in this case, the availability of a college scholarship) not only affect the recipient, but affect those with whom he interacts.[18]

Notice that these arguments say nothing about the ability of a policy-maker to affect outcomes per se. Nor do they say anything about limits to the ability of the policy-maker to predict the effects of his policy. The sense in which these come into play involves the extent to which one relies on a particular specification of nonlinearity in modeling the

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individual behavior under question. To the extent this model is incorrect, policy effects will, of course, be misassessed. However, this dependence does not logically depend on whether the system is complex. If $J_i = 0 \; \forall \; i$, so that all interdependences are eliminated, then assessing policy effects operating through h_i still requires that the dependence of h_i on a policy is correctly specified. Hence, if one is considering a cigarette tax, its effects depend on the utility loss to an individual from having to pay more for a cigarette.

Third, the properties of complex systems can exacerbate the difficulties in policy design generated by a policy-maker's ignorance. This is evident when one considers phase transitions. Knowledge of whether the model parameters produce a phase transition is obviously critical in getting the most bang per buck from a policy. Nevertheless, even when the policy-maker is ignorant about these details, one can affect behavioral outcomes. Sufficiently high taxes can make smoking prohibitively expensive and override any social influences on smoking decisions. Further, when one is uncertain about the correct model for a phenomenon (for example, whether it has one interactive structure or another, or whether the model's parameter values are associated with one or multiple equilibria), this uncertainty can be accounted for in assessing policies, so long as probabilities can be assigned to the unknown objects of interest. Brock and Durlauf (2001c) and Brock et al. (2003) provide model-averaging frameworks that respect policy-maker ignorance of the true economic model when assessing relative payoffs across policies.[19]

4. Complexity and policy evaluation: macroeconomics

Complex systems analysis has yet to elucidate any macroeconomic questions, with the exception of efforts to study financial market volatility, as noted above.[20] In my judgment, complexity can make a constructive contribution in terms of understanding macroeconomic phenomena. The reasons follow directly from my earlier comments about aggregation. While considerable progress has been made in introducing heterogeneity into macroeconomic models, these advances have generally been delimited by the sorts of modeling tools that have been employed. So, one conjecture is that complex systems methods may complement existing mathematical tools.

In terms of substance, there are reasons to think that complexity methods may have value in terms of insights into a range of phenomena. For example, the current financial crisis raises questions regarding the interdependence of different financial institutions. One way to make sense of claims that a given financial institution is too big to fail is that its bankruptcy would induce cascade effects throughout the financial system. Allen and Babus (2009) and Battiston et al. (2009) began the development of arguments of this type in the context of how interdependence can create systemic risk. It is very appealing to think of systemic risk as an emergent property of a financial system. Similarly, one can imagine that the structure of interactions between agents will determine how financial panics can arise and the way in which they are transmitted across a population (see Gai and Kapadia, 2007).

In making these conjectures, I should issue two caveats. Some of what I think of as richer modeling of economic phenomena may produce systems that look complex in terms of the introduction of elaborate interaction structures without producing qualitatively new properties such as emergence or universality. Allen and Gale (2000) develop

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a four-bank model to describe contagion and derive rich insights without the use of complex systems tools; Gai and Kapadia (2007) acknowledge that their contribution is to enrich this earlier article with a more elaborate network structure. Second, methods that are most likely to be of use in financial contexts derive from the mathematics of networks, much of which has evolved independently from complexity per se. An extremely readable overview of current network research in economics is Jackson (2008). A nice empirical example of work of this type is Iori et al. (2008), which uses network analysis to provide a detailed description of the Italian overnight money market. This type of detailed description provides the basis for empirically motivated advances in theoretical modeling.

Efforts to use complex systems to understand macroeconomic phenomena cannot proceed without careful attention to the decision-making of the individual agents under study. Universality says that qualitatively similar aggregate properties hold for classes of interaction structures, not that qualitatively similar aggregate properties hold for wide ranges of assumptions on the determinants of individual behavior. This is especially true with respect to the role of beliefs about the future in influencing individual behavior (see Durlauf, 2008).

5. Economic policy and the Gaus-Hayek critique

The discussion in Sections 3 and 4 has taken a very modest view of how complexity can affect policy-making. This is true in the sense that the discussion has implicitly employed the standard economic approach to policy analysis, in which each of the possible effects of a policy is delineated and probabilities are assigned to the possibilities. Given a policy-maker's preferences, policy can be partially ordered given the set of possible outcomes and associated probabilities. Gaus (2007) has recently launched a powerful critique of expedient approaches to policy evaluation, using ideas from complex systems theory. By 'expedient policies', Gaus essentially refers to policies that are chosen on the basis of the outcomes they generate; as such, his critique can be understood as questioning consequentialist justifications for policies, although with a subtle difference that I will address below. Gaus argues that the existence of strong limits to the predictability of expedient policies is a corollary of the proposition that society (in Gaus's view, a coupling of the economic and political systems) is complex. He both critiques and develops a sophisticated version of the ideas of Friedrich Hayek (1960, 1964, for example). Hayek's views are well summarized in the following statement:

> problems of economic policy … cannot be satisfactorily resolved by ad hoc decisions on particular questions but only by the consistent application of a principle that is uniformly adhered to in all fields. There is only one such principle that can preserve a free society: namely the strict prevention of all coercion except in the enforcement of general abstract rules equally applicable to all. (1960: 284)

Gaus (2007: 158) argues that

> The crux of complexity theory is that our predictions about what will occur are likely to be wrong. There is, then, a very strong case that our interventions are not apt to be expedient because we have radically incomplete knowledge.

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However, Gaus recognizes that this is not sufficient to argue against expedient policies which are justified on consequentialist grounds. Unpredictability may render the case for expedient policies weak, but not nonexistent. In the absence of other considerations, the weak case is sufficient to determine policy. In order for complexity to lead to such a conclusion, it is necessary that there exist alternative reasons for policy choice that can trump the weak consequentialist case. Gaus (2007: 159) states:

> If our only reasons to choose are reasons that aim at producing good results, then even if P_1 has only a miniscule advantage in expediency, we have reason to choose it, though we have firm reasons to doubt that choosing it is likely to turn out better than opting for P_2. The key to avoiding this is to allow another sort of reasoning: rule- or principle-based reasoning of the sort that is not outcome oriented.

Gaus goes on to provide a subtle defense of Hayek's belief that deference is due to evolved moral rules. Part of his defense involves the argument that evolved rules are likely to work relatively well; so while he avoids teleological arguments on evolved rules, he does allow a consequentialist defense for evolved rules as opposed to those imposed via a policy-maker. In my view, one can take a more optimistic view than Gaus with respect to consequentialist justifications for economic policies as determined by government authorities.

Before indicating where I disagree with Gaus, let me be clear that it is easy to identify predictive failures of economic models. I would in some cases go further than Gaus and argue that economic theory does not imply high levels of predictability for economic phenomena. For example, the random-walk theory of stock prices, which is an extremely accurate description of short-run price movements, explicitly implies that stock price changes are *not* predictable! More generally, economic models differentiate between economic structures and various types of shocks which, by definition, are not predictable. Nor is there any good reason to think that these shocks are necessarily predictable under better economics. Any economic model that in 1970 accurately predicted the trajectory of inflation and unemployment in late 1971 would have been, most probably, a bad model, since these aggregate variables were driven by the oil embargo, something hardly within the purview of economics and so an unforecastable shock from the perspective of economic theory.

Second, and this argument is made by Gaus, even if one stipulates that the predictive quality of economic models is low, this may miss the objective of policy analysis. The policy-relevant question is not whether economic models can provide a high degree of predictability per se, but rather that they facilitate the comparison of alternative policy regimes. Where I disagree with Gaus is on the question of whether, if most of the unpredictability of the economy is not subject to government interventions, there is necessarily a correspondingly modest role regarding government's capacity to raise social welfare. To give a simple example, suppose that aggregate output y_t is a random walk with constant variance errors ε_t. It is evident that the forecast errors $y_{t+k} - y_t$ will grow linearly, or to put it formally, $\mathrm{var}(y_{t+k} - y_t) = k\sigma_\varepsilon^2$. Hence, today's level of output is swamped by future shocks. But this has no bearing on the question of whether a policy intervention at time t can affect $y_t, y_{t+1}, \ldots$. Suppose that policy intervention p shifts y_t to $y_t + c$. If

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one considers the present discounted value of current and future output with discount rate β, then, assuming that the random-walk structure is preserved, the present discounted value of the intervention is $c/(1 - \beta)$. For a discount rate near 1, this value is quite large. This example indicates that unpredictability can be coupled with large, predictable policy effects. Cowen (2006) makes a similar argument, one that emphasizes that when the future is discounted, this can allow for the short-run, predictable consequences of policy to dominate decision-making. My emphasis is more on the decomposability of the certain and uncertain parts of a future variable that one wishes to influence via a policy change.

Beyond these theoretical considerations, one can identify cases in which policy effects have proven predictable. Here are two macroeconomic examples, one narrow and the other broad. In 1968, a temporary tax surcharge had little effect on consumer spending and thereby failed to stem inflation. As discussed in Okun (1971), the failure of this policy to have its intended effects follows from the logic of the life-cycle/permanent income hypothesis of consumption, which implies that consumers will react differently to temporary changes in income than to permanent changes, since the effects of the former can be smoothed over time. The Great Moderation, which refers to the reduction of inflation in the 1980s and 1990s, has been persuasively attributed by Sargent et al. (2006) to the Federal Reserve's adjustment of its policies in response to historical experience in a way that is consistent with contemporary views of the tradeoff between unemployment and inflation; in other words, policy-makers learned to act in accordance with contemporary economic theory.

The point of these examples is not that policy effects can always be accurately assessed or that economists can rest on their laurels in explaining such fundamental questions as international inequality. Rather, these examples indicate that the notion of policy unpredictability is more complicated than it may first appear. One could argue that each of these policy changes, all of which would appear to be expedient under the Gaus-Hayek definition, had effects that are consistent with the predictions of economic theory. Interestingly, for the case of macroeconomic policies, what I claim are predictable policy effects, from the vantage point of economic theory, were not expected by the government. But these failures of government expectations are in the tax-surcharge case, the result of lack of attention to economic theory and in the case of the Great Moderation, the result of adaptation to economic theory.

This qualitative concordance between predictions and outcomes largely constitutes what economists ask of an economic theory; more precise predictions typically involve careful empirical work in order to assess policy-relevant parameters. It also seems sufficient to argue that the justification for an expedient policy can be strong. In addition, part of the art of policy-making is the fine tuning of policies as their effects are revealed. This may be a form of expedient policy, but the expedience involves recognizing the limits to policy-maker knowledge and adjusting policy in order to achieve a certain goal. By analogy, adjusting a rocket's trajectory over the course of its flight is expedient, but is done so in response to factors that could not be anticipated in advance of the setting of the initial course.

Third, the Gaus-Hayek position on government policies seems to differentiate between rules that are independent of the state of the economy (or its history) and those

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that are not.[21] This strikes me as an excessively strong distinction and one that is inconsistent with modern economic thinking. A key modern idea in macroeconomics is the distinction between rules and discretion, something originally articulated by Kydland and Prescott (1977) in a classic article that still warrants reading. From this perspective, one distinguishes between rules such as monetary policy rules in which the Federal Reserve sets the short-run nominal interest rate as a function of the last period's unemployment rate, inflation rate, and interest rate[22] and a discretionary policy in which the Federal Reserve chooses an interest rate during each time period without commitment to a rule for choosing the rate. Commitment to rules allows the Federal Reserve to affect the public's expectations and so can have desirable effects. But such rules are expedient from the Gaus-Hayek perspective since they are state dependent. I see no principled reason why complexity should call into question the logic behind policy rules that are expedient in the sense of depending on the current and past state of the economy. These types of rules have been evaluated for their robustness to different types of policy-maker ignorance, though to be fair, these evaluations assume that the range of possible policy effects is relatively delimited. So, in my view, the modern approach to macroeconomic policy-making has largely rejected purely expedient policies. The modern approach looks for rules that imply that government actions, such as interest rates and levels of unemployment insurance, are dependent on the state of the economy.

Gaus criticizes what I have called the rules-based approach to modern policy-making. He argues

> Nevertheless, it might be insisted, that we can make pairwise comparisons of social policies, even if we do not know the best. However, because we have no reliable idea of the total consequences of major social policies or social institutions, we are not even in a position to make accurate one-on-one comparisons ... Thus we are seldom in a position to determine whether one option is 'good enough,' if this means that it does a relatively good job in getting us where we want to go. (Gaus, 1998: 25)

At this level of generality, Gaus is of course correct. But it is not clear that policy-makers have so little knowledge of the welfare effects of policies or their overall consequences as is required to make the critique operational for a number of important circumstances. For example, the successes of central banks in fighting inflation do not seem to fall into this category. In my view, for the sorts of economic policies that are debated, purely principled policies of the type Gaus discusses are a special case of the general rule-based policies that economists consider. The defense of purely principled rules needs to be made relative to the sorts of state-dependent rules that are their natural alternatives. In fact, there the differences between my views and Gaus's are less than meets the eye, since he would argue that consequentialist considerations naturally should be part of the process of rule construction, which places him close to my rule-based view of policy. Where we differ is in my relative confidence in the extent to which these rules can be state dependent. Even this difference can be partially reconciled in that Gaus emphasizes the inability of a policy-maker to make fine-grained predictions. My view is that this inability is exactly what makes efforts to construct robust policies so important. Hence, my argument is that policy-maker ignorance is part of

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what is to be addressed in constructing policy rules; in my view, this does not lead to a distinction between principled and expedient policies so much as the search for sturdy policy rules.[23]

One response to these arguments is that I have misinterpreted Gaus and Hayek in that I have ignored their concerns about unintended consequences. Gaus gives the example of how compromises on slavery in the US Constitution had unintended consequences decades in the future. As such, Gaus's argument is a species of the general attack on consequentialism made by authors such as Lenman (2000). Lenman's argument is that consequentialist calculations are so fraught with unpredictability as to render them useless for rules of conduct. Gaus takes a far more nuanced view, avoiding untenable claims that consequentialist justifications are nonexistent.

In order to evaluate the unintended-consequences argument, one needs to take a stance on what generates the uncertainty that leads to the consequences. If one has a well-defined set of mathematical equations that describe an economic phenomena of interest, then unpredictability can be equated to the specification of probabilities associated with the different outcomes. Gaus's example of the long-term problems associated with the US Constitution does not speak to the decision problem faced by delegates at the Constitutional Convention. For one thing, judging decisions under uncertainty by *ex post* outcomes does not speak to their rationality at the time the decisions were made. Further, my reading of the relevant history suggests that the delegates at the Constitutional Convention were well aware that they were temporizing on an issue that threatened the unity of the country. The bottom line is that unintended consequences cannot function as a distinct argument against a policy if the potential for the consequence is part of an expected payoff calculation, which is the standard way in which to engage in Bayesian statistical decision theory. In other words, unintended consequences, as defined here, are accounted for in expected payoff calculations.

However, Gaus and Hayek can invoke a second claim of misunderstanding with respect to how I have interpreted the concept of unintended consequences. One can imagine cases in which probabilities cannot be assigned to possible outcomes as well as cases in which the possible outcomes are not even known. An example of the former occurs when the behavioral parameters that will determine the effects of a policy cannot be identified from available data. An example of the latter is a phenomenon such as technological innovation, regarding which, I personally would not have any idea on how to specify the candidate technologies in 2090, let alone assign probabilities that they will have been realized. This corresponds to the idea of novelty invoked by Gaus in describing Hayek. These types of uncertainty do not fall into the Bayesian paradigm (in which probabilities are defined with respect to all unknowns), and so my first response on unintended consequences is off point.

Before addressing this version of unintended consequences, it is useful to note that neither the inability to specify probabilities on the support of possible outcomes nor the inability to specify the support of possible outcomes associated with a policy has to do with complexity per se. Identification problems in econometrics can lead to problems in uncovering the probabilities of outcomes under counterfactuals, given the observable data. As for the inability of a policy-maker to describe the support of possible outcomes, it is not clear to me why this relates to complexity, as understood by current scholars.

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Hayek's conception of complexity calls into question the use of mathematics to describe social phenomena, including the support of possible outcomes.[24] Modern complexity theory is mathematical, and at least in terms of the models with which I am familiar, always implicitly defines supports of outcomes. Of course, complexity can lead to very large state spaces for outcomes and imply that the assignment of probabilities is problematic, but that is a separate matter from the inability to know what outcomes are possible. The point is that the conception of complexity employed by Gaus and Hayek may be richer than that allowed by the current mathematical models of complexity, and so my arguments do not do them justice to the extent that I rely on mathematical conceptions of complexity.

With respect to limits to assigning probabilities to outcomes, this problem is the basis of much contemporary work in decision theory. I have already alluded to work by Hansen and Sargent using minimax criteria for decision-making in the context macroeconomics. On the microeconomics side, Manski (2007, 2011) has advocated the use of minimax regret, which is another way of making decisions when probabilities cannot be assigned. Minimax regret differs from minimax in that one does not compare policy effects when the policy-maker's ignorance is replaced with the worst possible resolution of the uncertainty he faces, but rather evaluates policies according to how badly a policy can do relative to its alternatives. I do not defend either of these approaches so much as argue that there are ways to address the absence of knowledge of probabilities that do not require the abandonment of outcome-based criteria. To be fair, I am unaware of any philosophical work that has explored the relationship between outcome-based and non-outcome criteria for policy evaluation when the outcome-based criteria are based on this type of decision theory.

Of course, the implications of minimax or minimax-regret criteria for policy evaluation will be sensitive to the details of the systems under study. Here complexity can qualitatively matter. Recall that in the Brock-Durlauf model that I outlined the number of equilibria in an environment depends on the values of certain parameters. When probabilities cannot be assigned to the possible values of these parameters, then it is easy to see that worst-case scenarios for a policy can be radically different according to whether or not the range of possible parameter values passes over the values which induce phase transitions. However, this does not mean that policy evaluation will therefore tend to favor policies which are relatively passive. The potential for multiple equilibria, for example, may be an incentive for stronger efforts to affect private incentives, so that a population does not run the risk of being trapped in a bad equilibrium.

As for the issue of an unknown support for outcomes, this is a much deeper issue than ignorance of probabilities on a known support and one concerning which, as far as I know, relatively little work has been done in modern decision theory. One response lies in the comparative nature of policy evaluation. If a policy-maker is comparing two policies, the relevant question is whether the support of outcomes differs across the policies. If no such differentiation exists, then I do not see a principled basis for uncertainty in the support to affect policy comparisons.

Perhaps one can argue that even if they cannot be enumerated, different policies lead to qualitatively different types of unknown outcome supports. One might imagine that this is true for large versus modest policy changes. Gaus, I believe, would argue this

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is an example of why policies should follow principles not consequences. But I am not sure that this conclusion follows. For example, suppose one compares Britain's 1832 Reform Act to the Chartist demand for universal suffrage. The principled position (I believe) supports the Chartist position and would have done so even without the luxury of retrospection. But here the uncertainty about social and political stability would seem,[25] on minimax grounds, to favor a more modest policy change.[26] In other words, exclusive reliance on principles rather than outcomes is problematic without a stance on the related unintended consequences induced by the principled choice.

None of this, of course, challenges Gaus's important argument that policy evaluation involves assessments between consequentialist and non-consequentialist criteria and that there is often a tendency to overstate the evidence related to the consequentialist basis for policy. Further, I agree with the corollary to this view that there are contexts in which non-consequentialist reasons should trump consequentialist ones because of the inability to say much about the latter; I have in fact used Gaus's analysis to argue against racial profiling precisely on the grounds that the evidence that profiling minimizes crime is too weak to rebut a presumption that policies should treat innocent persons equally (Durlauf, 2006a). My differences with Gaus amount to the position that difficulties in predicting the effects of policies should not to any large extent eliminate the role of consequences in policy comparisons or the dependence of policies on the state of the economy, even if one accepts the view that economic complexity is a fundamental impediment to predictive accuracy regarding policy effects. Rather, as is occurring in modern economic theory, attention needs to focus on issues of policy robustness in the face of policy-maker ignorance.

6. Conclusion

In this article, I have provided some ideas on how complex systems thinking matters for economics and for economic policy evaluation. My conclusions are modest in that I argue that complexity can provide ways to enrich contemporaneous economic theory, primarily in terms of the mathematical tools it provides. I have explicitly rejected claims that complexity represents a paradigm shift for social scientists and have argued that it does not fundamentally affect policy evaluation.

While largely skeptical of the existing literature, I am by no means dismissing the potential value of complex systems methods to economics. I believe it remains an exciting and potentially important research direction. For this to happen, complexity needs to be regarded as a set of mathematical tools that can facilitate the modeling of richer economic environments than is allowed by the current set of mathematical methods available to economists. To ask more of these methods is to confuse technique with substance.

Notes

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1. The importance of being careful about what complexity means in terms of economics is highlighted by Holt et al. (2011), who use as evidence of the increasing role of complexity in recent economics research ranging from behavioral economics to econometrics to evolutionary game theory – none of which, in my view, have anything to do with complexity per se. As will be seen below, I will argue that these developments indicate why complexity has made, at best, a limited contribution to economics. In my view, the developments these authors cite undercut their arguments as these areas of new research simply reflect the growth of economic knowledge. For example, when these authors state that 'Econometric work dealing with the limitations of classical statistics is redefining how economists think of empirical proof' (Holt et al., 2011: 363) they could be referring to developments ranging from the rise of Bayesian econometrics to the study of identification problems, none of which have been influenced at all by ideas from either the mathematical complexity literature or any self-defined complex systems approaches to economics.
2. Representative agent models are constructed from the behavior of a single agent, who is assumed to represent the entire economy. Such models are commonly used in macroeconomics to sidestep the difficulties of drawing empirical implications for aggregate variables when heterogeneity is present. That said, much modern macroeconomic research has focused on the introduction of heterogeneity in order to avoid the representative agent framework.
3. Even here, one must be careful. The finding of deviations of rationality in laboratory or even field experiments has yet to be reconciled with the many economic studies that have found that models which assume agent rationality well explain observed behavioral patterns. I will not give specific examples since the corpus of empirical economics is so vast.
4. See Blume and Durlauf (2001) for interpretation of the first welfare theorem as an emergent property and Gaus (2011) for additional discussion.
5. For a description, see Sonnenschein (2008). The theorem is also sometimes called the Sonnenschein-Debreu-Mantel theorem.
6. General equilibrium theory does imply that aggregate demand functions are homogeneous of degree 0 in prices, which means that if every price in the economy changes proportionately, then behaviors are unaffected. This is not important to my argument.
7. One of Brock's arguments (1999) is that economists are typically more interested in conditional than unconditional distributions, that is, one is more interested in the conditional distribution of an individual's income given his family background and his education than the unconditional distribution. So I see one challenge to complexity advocates as the linking of their measurement methods to the objects that are of the greatest interest to economists and policy-makers.
8. Another reason, of course, is the tendency of researchers to overstate the importance of their own research. This is especially egregious in the case of 'econophysics', where physicists, with little knowledge of the discipline of economics, pronounce it to be hopelessly flawed. As a corollary, reputable physics journals such as *Physica A* now routinely publish papers on economic phenomena that for the vast majority of economists are either uninteresting empirical exercises or theoretical models whose micro-foundations amount to the relabeling of the objects of physical systems as economic actors. A disgruntled fringe group of economists also publishes in forums of this type, but I think they do so in order to avoid engagement with the broader discipline. I regard the development of econophysics publishing outlets as a very unhealthy development, as their main effect is to ensure that the dimensions along which

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complexity can contribute to economics are isolated from the larger body of economic research because these dimensions are mixed with bad and ill-informed economic reasoning and in many cases outright ignorance of the economics literature. So long as economics is not taken seriously by 'econophysicists', their work will not move beyond dilettantism.

9. Holt et al. (2011) is a good example.
10. A common answer to the objection that agent-based models are black boxes is that one can see how results change when the computer code changes. But this is to me question begging, as one may not know *why* results are robust or fragile. Note that agent-based modeling is distinct from computational economics, which is a thriving area of research that has allowed economists to study more complicated models than are amenable to analytical study. My general complaint is that the agent-based models typically do not build on what is known analytically, but rather start from ad hoc rules for individual behavior and interindividual interdependences. That said, there are important exceptions. Blake LeBaron's work is, in my judgment, the best work using agent-based methods to produce understanding. See LeBaron (2006) for an overview of his and others' work in finance; his assessment is more charitable than mine, partly because work in finance using agent-based models has been of higher quality than in other areas.
11. The first argument represents my assessment of the empirical literature on asset-price behavior; the latter two arguments are taken directly from Ross (2005: 64).
12. Epstein (2002) is a good example of a model that is purported to explain the rise of civil violence, but ignores identification. While the model generates the rise, nothing in the analysis indicates why other dynamic social sciences models could not produce the same phenomenon, nor is any feature of Epstein's model shown to distinguish it from the plethora of conventional economic models of diffusion. Epstein may argue that his model captures spatial diffusion in a way not modeled in conventional models, but this is neither strictly true (diffusion in social networks is closely analogous to spatial diffusion) nor is the introduction of spatial diffusion into conventional models a conceptually radical innovation. To be very concrete, Lohmann's model of the rise of civil unrest in East Germany (1994), while not about violence per se, is closely analogous to what Epstein wants to examine. Lohmann develops an information-diffusion model of the rise of unrest (where the key information is the response of the police to demonstrations) that does not employ any agent-based methods. The challenge for agent-based modelers is to demonstrate that their methods can provide additional empirical insights beyond what Lohmann is able to do, for example. With the partial exception of work in financial markets, I simply have not seen any persuasive case in which new empirical insights have been generated.
13. See Hansen and Sargent (2008) for a comprehensive overview of their work.
14. A recent editorial by leading complexity advocates, Farmer and Foley (2009), arguing the virtues of agent-based approaches over standard economics modeling is noteworthy for the lack of evidence of accomplishment by the former class of models. The claimed successes of agent-based methods do not include any substantive perspectives on economics or any evidence of predictive superiority over conventional methods. Nor does it make clear how the agent-based models are derivative from standard economics approaches. For example, the editorial touts an agent-based model involving leverage without noting that the economist involved in the project had developed the basic economic ideas using general equilibrium theory. See Fostel and Geanakoplos (2008) and Geanakoplos (2009).

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15. For surveys, see Blume et al. (2010), Brock and Durlauf (2001b), and Durlauf and Ioannides (2010).
16. The assumption that agents react to their beliefs about the behavior of others rather than to the actual behavior of others is made for simplicity and renders the model more natural for a large population than for a small one. Soetevent and Kooreman (2007) study this model when actual behavior is observed.
17. It is important to recognize that if one wishes to weaken the rationality assumption I make, this weakening should be done in a principled way, that is, in a way that can be justified on empirical or theoretical grounds. If one simply rejects rationality and says that expectations can assume any value, then the model could be reconciled with any pattern of choices simply by varying the individual beliefs, which would render the model uninteresting.
18. See Durlauf (2006b) for further discussion of this type of tradeoff.
19. Such self-citation should not be construed as suggesting that these papers suggested the idea of model averaging; rather, they are particularly concerned with the role of model averaging in policy evaluation.
20. This remark is pejorative in that there have been a number of studies that have purported to apply complexity to macroeconomic phenomena. The problem, in my view, is that this work has amounted to relabeling objects in statistical mechanics models as economic actors, thereby losing the substantial features of a macroeconomic system. The interested reader should consult my review (Durlauf, 2008) of Aoki and Yoshikawa (2006). It is certainly the case that unlike the literatures on social interactions and finance, for example, complex systems work has had no effect on macroeconomics. The reasons for this are (1) that the applications have such weak micro-foundations that the aggregate behaviors of the models are uninteresting and (2) that the models have not been shown to match qualitatively actual macroeconomic data (in the ways that define current empirical practice), let alone provide superior fit to standard macroeconomic approaches.
21. This may be unfair to Gaus in the sense that he is writing at a more abstract level than I am. In other words, once one conditions on particular socioeconomic contexts, the distinction between principled and expedient policies becomes more subtle.
22. Monetary policy rules of this type are known as 'Taylor rules'. See Orphanides (2008) for details.
23. One important feature of Gaus (2007), which I have not addressed in this discussion, is his subtle and qualified defense of rules that emerge from social evolution. It is not clear what policy domain is affected by this defense; I have trouble seeing implications for monetary policy from this defense. Also, it is important to note that the multiple steady states that complex systems evidence can be path dependent, which means that the particular steady state to which the economy evolves (assuming the state is dynamically stable) is determined by initial conditions. As such, socially inefficient outcomes can be the product of evolution; this occurs for the Brock-Durlauf model, for example.
24. The only work I have ever seen that attempted to develop a mathematics of novelty is that stemming from Walter Fontana (for example, Fontana and Buss, 1996), and is based on λ-calculus methods developed in the context of computer science and mathematical logic. I do not believe that these efforts, as intellectually impressive as they are, led to any substantive social science insights, nor am I aware that the research program is still alive for the social sciences, as opposed to evolutionary theory, for example. But I do not adequately understand the relevant mathematics to comment sensibly on its potential.

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25. This claim relies on my own reading of the relevant history. If this reading is incorrect, the logic of the argument still goes through, if not its application to British suffrage.
26. As a matter of history, the 1832 Reform Act was followed by a sequence of subsequent expansions of suffrage, so that the Chartist goal was only achieved after World War I.

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About the author

Steven N. Durlauf is Vilas Research Professor and Kenneth J. Arrow and Laurits R. Christensen Professor of Economics at the University of Wisconsin at Madison.

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