

What economic agents do: How cognition and interaction lead to emergence and complexity

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Published online: 12 May 2007
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Abstract Kohn (The Cato Journal, 24(3):303–339, 2004) has argued that the neoclassical conception of economics—what he terms the “value paradigm”—has experienced diminishing marginal returns for some time. He suggests a new perspective is emerging—one that gives more import to economic processes and less to end states, one that bases behavior less on axioms and more on laboratory experiments. He calls this the “exchange paradigm”. He further asserts that it is the mathematization of economics that is partially at fault for leading the profession down a methodological path that has become something of a dead end. Here I suggest that the nascent research program Kohn has rightly spotted is better understood as distinct from its precursors because it is intrinsically *dynamic*, permits agent actions out of equilibrium, and treats such actions as occurring within networks. Analyzing economic processes having these characteristics is mathematically very difficult and I concur with Kohn’s appeal to computational approaches. However, I claim it is so-called multi-agent systems and agent-based models that are the way forward within the “exchange paradigm,” and not the cellular automata (Wolfram, A new kind of science, 2002) that Kohn seems to promote. Agent systems are generalizations of cellular automata and support the natural abstraction of individual economic agents as software agents.

Keywords Agent-based modeling · Heterogeneous agents · Self-organizing systems · Emergence · Complexity

JEL Codes B4 · D5 · D8

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We repeat most emphatically that our theory is thoroughly static. A dynamic theory would unquestionably be more complete and therefore preferable....A static theory deals with equilibria....For the real dynamics, which investigates the precise motions, usually far away from equilibria, a much deeper knowledge...is required.

John von Neumann and Oskar Morgenstern

Theory of Games and Economic Behavior (1944: 44–45).

1 The neoclassical “sweet spot”

Mathematical economics has been with us for something like a couple of generations, the vagaries of dating intellectual history being what they are. While the early pioneers of the approach were often quick to circumscribe the applicability of the assumptions they found necessary to make for analytical tractability, today such caveats are less commonly noted in print, although no less necessary for satisfactory interpretation of results (Mirowski 1989, 2001). Whether this norm of (non-)exposition is simply an efficiency standard or rather a behavioral adaptation to avoid dealing with unpalatable falsehoods is, for present purposes, immaterial. What I am interested in exposing is the microstructure of these assumptions and their inter-relation—their ecological character, as it were.

Consider the assumption of *rationality*. It is a standard postulate of our field that agents can figure out—whether through induction, deduction, or in combination—the future of their world, at least on average. They then act to give themselves maximum welfare in that world, or so goes the common specification of behavior. This stipulation manifests itself in a variety of ways, from “no arbitrage” conditions to “profit maximization” assertions to claims that real people do not throw money away. For a span of time only briefly shorter than its existence, this notion of rationality has been the focus of caustic, even withering criticism (Simon 1957; Kirman 1993; Simon 1997a, b, c). The logical arguments used against it include the obviously bounded capacity of humans to figure out how to best behave (Simon 1978), the intrinsic (e.g., computational) difficulties associated with certain classes of computation (Papadimitriou and Yannakakis 1994), and the lack of procedural or algorithmic bases for the assumed rational behavior (Simon 1976). Empirically, we have today very strong and accumulating evidence for the systematic ways that human behavior departs from rationality requirements in a large number of experimental conditions. Indeed, while it was conventional early on to term these departures from rationality “anomalies,” the phenomena are now so well-entrenched empirically, so reproducible in distinct laboratory settings, and so robust across economic environments, that what seems genuinely anomalous is the idea that human behavior could ever even approach true rationality. Furthermore, while it could be the case that the assumption of rational behavior is credible for a small subset of people, it is certainly the case that not all agents are equally rational, as is implicit in conventional theoretical models.

Assumptions about *agent heterogeneity* span the spectrum in economic theory, from arbitrarily heterogeneous agents in general equilibrium theory to representative (and thus essentially homogeneous) agents in macroeconomics. The norm in many economic models is to have some finite number of agent types, although even this finiteness restriction is sometimes relaxed in game theory. The foibles of representative agents are by now well-known (Kirman 1992), but do not seem to have much discouraged their use. This is especially strange because it is the differences between people that lead them to “...truck,

barter and exchange...” in Adam Smith’s ([1776] 1976) now well-known phrase. If people were identical there would be little reason for them to interact in the first place.

A peculiar feature of neoclassical economic models is that agents do not directly interact with one another, but rather with abstract economic objects like price vectors and aggregate economic statistics (e.g., the unemployment rate). This lack of *interaction* has been noted by various authors (Kirman 1997), and its significance is increasingly well understood. Indeed, there are important streams of economic research today that try to relax this assumption, but doing so can necessitate the relaxation of agent homogeneity and rationality assumptions mentioned above. Thus, this is a delicate business and one must proceed with great caution.

Finally, perhaps the core conviction of neoclassical oriented economists has to do with agent-level equilibrium. While some recognize that equilibrium is rarely realized or even unrealizable, and serves instead simply as a kind of ideal state, most conventional work in microeconomics and game theory today pursues agent-level equilibria as a matter of course. That is, when models are articulated, the only configurations of them that are deemed to be of interest are fixed-point equilibria at the level of individual decisions (e.g., Walrasian equilibria, Nash equilibria). Because of micro-equilibrium stipulations, an ancillary assumption is that all fluctuations in an economy are exogenous, which results in the awkward situation of economists usually seeking the basis for economic change in non-economic phenomena. At the very least this is an unsatisfactory state of affairs, if not a downright dereliction of duty—why cannot economic dynamics be produced by the economy itself?

This set of assumptions—agent rationality and homogeneity, non-interactiveness and equilibrium—constitute what might be called the neoclassical *sweet spot*—a group of interlocking and mutually reinforcing specifications for which relaxation of any one can lead to the impetus, if not the absolute necessity, to relax others. For instance, invocation of bounded rationality naturally leads to *heterogeneous* agents, as there is one way to be rational but many ways to depart from rationality. Similarly direct interactions—as soon as one permits local interactions between neighbors, say, there is no easy way for agents to be globally rational, and so their actions are inherently out of (global) equilibrium. In this fashion we see that the neoclassical sweet spot is subject to unraveling once any of its primary stipulations are varied.

It is an old saw that in economics “it takes a theory to beat a theory,” an example of which is the pervasiveness of rationality specifications in today’s journals despite the critiques by proponents of bounded rationality who, unfortunately, have no (simple) theory to stand in its place. However, the existence of this *de facto* meta-rule of economic methodology is also evidence of the weak empirical grounding of economic theory, for one can scarcely imagine such an assumption surviving in a strongly empirical discipline. For example, in chemistry or physics what would be the status of a theory that has been proven empirically incorrect, even if no substitute were available?

Up until recently there has been no alternative to the neoclassical “sweet spot,” but all this has changed significantly in the past decade, driven in part by advances in experimental methods and computational hardware and software. After further discussion of the neoclassical position below, and ultimately arguing that it is a brittle, fragile position, unstable to certain variations, I describe how new forms of computational models in economics may provide the basis for a methodology to take economics beyond mere mathematical expression and deductive inference. I believe that progress on the “exchange paradigm” agenda has little to do with Wolfram’s *New Kind of Science* and a lot to do with the importation of multi-agent systems computational techniques and tools into economics for purposes of model building. Some early results with such models constitute the several

examples described in the body of the paper. I conclude by asking whether it is realistic for any economic principles of great generality to exist in a world where the neoclassical assumptions no longer hold and, if not, does it matter?

2 The agent level

If one travels in sufficiently interdisciplinary scientific communities, one encounters two distinct kinds of skepticism from natural scientists about the possibilities of significant progress in understanding how economies actually work. Some natural scientists express concern about the lack of knowledge of human behavior under controlled, say experimental, conditions. Of course, dramatic progress has been made in this area, over the past two decades especially, and so on this front it is at least clear how to appease the critique if not completely abate it.

Skepticism of a different, more durable character arises from physical scientists, especially, who lament the non-existence of a reliable and robust general model of cognition. To them, a working knowledge of cognition—that is, even a phenomenological understanding and not necessarily one derived from the presumably “first principles” neuronal level—is required to build scientifically defensible models of human behavior. Here, while we can point to certain kinds of progress, we are much less close to being able to satisfy the critics and, more problematically for us, economists are not really in the business of creating such cognitive models and so are not in control of our destiny vis-à-vis this critique.

Kohn (2004) does not seem to think that the development of a better understanding of behavior is an appropriate area for economists to work in, but that rather it is in taking individual behavioral explanations to the social, aggregate level that economists really earn their pay.¹ While I do not disagree with the latter, to the extent that behavioral microstructure modifies macroscopic outcomes we, as economists, need to have the ability to develop our own capabilities to run experiments that adjudicate between competing behavioral hypotheses, and not solely rely on researchers from other fields to exogenously deliver such results to us. It may be that today we have comparative advantage in working the micro-to-macro link, but the economics discipline is expanding to create researchers who can credibly work on agent behavior, empirically, and that is to be applauded as our field makes progress towards being a truly scientific enterprise (Gintis 2004).

2.1 Cognition

How is it we got here—that is, to the neoclassical rationality postulate—in the first place? That is, how did economics develop *without* any coherent model of cognition? Herbert Simon, who made extensive contributions to economics but who also helped found the fields of cognitive science and artificial intelligence, and whose main appointment was in the Department of Psychology at Carnegie Mellon University for many years until the time of his death in 2001, analyzed the situation with characteristic clarity. Simon argued that neoclassical economics relied on *substantive rationality* as opposed to *procedural rationality* in its

¹ “[E]conomics as a discipline has no special claim to understanding the nature of individual behavior; presumably psychologists and cognitive scientists have much more to say about it. The stock in trade of economics, rather, is its understanding of the *aggregate outcome* of individual behavior—or more precisely, of the ‘unintended consequences’ of intended actions.”

equation of the existence of rational choice equilibria with their attainment in real economies (Simon 1976). He rightly noted that devices or institutions through which this equation would be justified are rarely if ever invoked, either in a model or in a persuasive story, and therefore no procedure or algorithm is specified by which such configurations would or could ever be achieved. For Simon, this was the Achilles' heel of rationality, that if humans did not have simple rules of thumb that would lead them to "good" outcomes, but rather had to be as smart as economists to figure out how to behave, then they could not be counted on to arrange themselves into "good" configurations.

Indeed, today we know quite a bit more about this than we did even a few years ago, mostly because computer scientists have begun working on traditionally game theoretic and economic problems, as ideas about strategic behavior have entered their field through so-called multi-agent systems, about which more below. Suffice it to say here, that a whole host of solution concepts and techniques in economics and game theory, from Walrasian and Nash equilibria on the one hand to various mechanism design problems (including, for example auction design) on the other, are known to be very difficult problems to solve *in principle*, and therefore unlikely to be easily solved in practice by real economic agents having very limited computational capacities (Papadimitriou 1994; Conitzer and Sandholm 2002).

Kohn rightly asserts that we should be more concerned with purposive agents than rational ones, and much of the work I will subsequently describe in this paper employs agents that are purposive. We often model such agents today as local utility 'gropers', that is, agents who inspect their local environment for utility gains and take actions that they believe will lead to satisfactory outcomes, at least with high probability. They typically have little global information and what they have may be significantly out-of-date. They may be able to acquire non-local information that is up-to-date but it can be costly to do so. They may have limited knowledge of their own preferences, and only understand these through a sampling or groping process whereby they try certain alternatives to learn how much it pleases them. Such specifications of behavior, while to some extent *ad hoc* insofar as they are not strictly based on the results of experiments, are no less *ad hoc* than empirically false rationality specifications (Camerer 1997, 2003).

Clearly it would be desirable to have empirically grounded behavioral specifications in lieu of such simple formulations of purposiveness. It is something of a curiosity that economists have shown little interest in the few early models of cognitive processes that have appeared, such as SOAR and ACT-R. Indeed, precisely because such models of individual decision-making have not been "hooked up" to standard economic models, it is unclear how results from the new subfield of neuroeconomics (Glimcher 2003) will be "hooked up" to otherwise standard economic models.

2.2 Interaction

In much of neoclassical economics, agents are treated as if they must decide what to do. To make such decisions, the agent is considered to have certain data about its economic environment available (e.g., prices, interest rates), obtained at no cost to itself. These data arise from economic activity, but usually individual agents are not modeled as setting the levels of such quantities. Rather, the actions of all agents create the levels of the economic variables in question, and each agent's behavior is treated as being consistent with such variables. For example, agents who prefer good *A* to good *B* buy more of *A* its price when relative to *B* falls, although the agents do not themselves participate in the price-setting process explicitly. A different way to say all this is that agents do not interact *directly* with

one another in neoclassical models. Rather, they interact only *indirectly*, through economic variables.²

The situation is different in game theory, where agents are considered to be in direct interaction with one another, with payoffs determined by the actions taken by *both* agents. However, interactions in game theory are typically treated as anonymous, and so in population games, say, agents assume they will interact with a typical or representative agent in the so-called “mean field” sense. Such direct interactions can lead to rich outcomes, as demonstrated by the recent literature on the statistical mechanics of games (e.g., Blume 1993, 1995), but quickly leads to deep problems in probability theory (Liggett 1985).

This leads to a curious conclusion. Despite economists’ commitment to methodological individualism, it is not the norm for individual agents to treat one another as individuals in deciding how to behave. This is a peculiar kind of atomized individualism, where each person is completely powerless against the ‘representative agent’ and completely isolated socially, unable to do anything but send economic signals to those with whom it anonymously interacts. It is as if the Earth were inhabited by 100 billion or a trillion people, and every person had lost every family member and previous acquaintance in the mass of humanity, and therefore each and every interaction a person has is with someone he/she has never met before and are unlikely to meet again. Clearly, this is a peculiar perspective on which to base social science.

Against this well-mixed, anonymous, mean field view of interactions there has grown up in the past decade a view of economic interactions mediated by networks. These can be social networks in the case of interactions between individuals, or production networks representing supply chains and related flows of commodities between firms. From an economist’s point of view, there are two distinct streams to this research, the older and more well-developed being economic behavior on exogenous networks, while newer work focuses on the economic incentives in the formation of networks in the first place (a.k.a. endogenous networks). For our purposes, it is of little use to distinguish these in what follows, so we lump both into the subsequent discussion of networks.

The main reason why the network approach is so important is because it challenges the neoclassical status quo on multiple fronts. First and foremost, methodologically, it permits truly direct, non-anonymous agent–agent interactions in which the identities of individuals can be made known to agents, at least if there is (economic) rationale for doing so. Second, the network perspective potentially breaks the mean field view of the economic environment in which agents interact. It makes information local, leading to locally purposive behaviors that can be either reinforced or not by the global environment, as when disparate social norms obtain and have to be mediated at some boundary (think driving on the left side of the road versus the right). Third, it is antagonistic of all manner of global rationality postulates, because agents only have local information and cannot optimize over all states of the world (i.e., globally), either statically or dynamically. In fact, the network perspective mixes temporal perspectives in a mathematically nasty yet realistic way, for there may be little value to locally optimizing even static decision problems when there are large-scale changes afoot in the network that will lead to secular and potentially costly strategy changes later; and any attempt to solve the large-scale, network-wide dynamic optimization problem is pure fancy. Fourth, it is equally antagonistic of equilibrium for essentially these same reasons, and because equilibrium in a network is a much more

² Buchanan (1964) makes essentially the same point in discussing Robinson Crusoe’s solitary allocation problem that becomes ‘symbiotic’ once Friday arrives and the two are brought into association with one another.

delicate affair than in soup (a.k.a. mean field interactions). Fifth, and finally, the network perspective is eminently realistic for wide swaths of economic affairs, and economists neglect it only at their peril. We next consider an example in which some of these features of networks come into play.

Example 1: Establishment and maintenance of cooperation in repeated games The difficulty of establishing high levels of cooperation repeated in games has been the subject of a voluminous literature play of prisoner's dilemma and related. Results with both population (well-mixed) games and spatial models (Huberman and Glance 1993) lead to the same negative conclusions. However, permitting agents to directly interact with other agents in some non-anonymous way, for example by reinforcing those interactions with agents whose behavior is non-exploitative and breaking off interactions with exploitative agents (Ashlock et al. 1996), can lead to high levels of cooperation. A similar effect can be achieved by signaling one's disposition ahead of play through otherwise meaningless tags (Holland 1995, 1998; Hales 2001; Riolo et al. 2001; Hales 2002). Both of these mechanisms create local environments hospitable to the "good" Pareto-dominant outcomes and guard against exploitative mutants or invaders.

In summary, taking seriously the idea of modeling agent interactions through networks leads to a kind of unraveling of the neoclassical "sweet spot," for once rationality and equilibrium postulates are either unworkable or of little utility then there is little left of the conventional perspective. I next investigate the extent to which these ideas lead to new perspectives about the aggregate economy.

3 The social level

Kohn is on the mark in framing the "value paradigm" as a top-down perspective, while the "exchange paradigm" offers a bottom-up orientation. The latter is a more open-ended and capable of novelty, innovation. In this section, we suggest general mechanisms at work in such systems. Specifically, agent interactions lead to emergent, multi-agent structures having potentially novel properties. To the extent that these structures are long-lived and give rise to subsequent generations of derivative structures, they embody the evolution of complexity.

3.1 Emergence

There is a large and growing literature on the idea of emergence in physical, biological, and social systems (cf. Haken 1987; Baas 1994; Morowitz 1998; Howitt and Clower 2000; Johnson 2001; Sawyer 2001; Morowitz 2002; Sawyer 2002). In systems that display emergence, the interaction of autonomous or quasi-autonomous components of the system leads to higher level functionality that is not present in any of the individual components. Examples of emergence in computational systems include:

- (a) "glider guns" and other structures in the Game of Life: super-cellular patterns that survive for observationally significant periods of time (Faith 1998);
- (b) self-reproducing structures like Langton loops in artificial life (Langton 1995);
- (c) autocatalytic arrangements of chemical species that are self-sustaining in artificial biochemical systems (Fontana and Buss 1994);

- (d) high degrees of segregation in spatial models composed of agents who have mildly integrationist preferences (Schelling 1971, 1978);
- (e) networks of skills in proto-economic environments (Padgett 1997);
- (f) skew distributions of income and wealth (e.g., Epstein and Axtell 1996);

It is a common mistake to confuse the emergentist position with an anti-reductionist stance. Few modern advocates of emergence deny reductionism in principle, but rather argue that it is simply not useful in practice (Faith 1998)—a kind of pragmatic anti-reductionism.

Elsewhere (Axtell 2006) I have argued that there is a close relationship between Austrian economists' ideas of *spontaneous order* (Hayek 1945), physicists' ideas about *self-organization* (Laughlin and Pines 2000), and the modern, computationally enabled view of *emergence* (Darley 1994). Self-organization refers to collective phenomena in which aggregate structures emerge from component interactions. Spontaneous order has the same character, although the components are nominally human actors (as opposed to inanimate particles) and there is an implicit welfare property associated with the emergent order—for example, when a path in the woods that emerges from many individuals' actions has the quality of being relatively shorter than alternative routes. Emergence is the most general notion, covering both these ideas (and not usually requiring a positive welfare property, e.g., unintended consequences), but also including the case of novel functionality arising at lower levels due to extant functionality at higher levels, a phenomenon sometimes termed “downward causation” in sociology and related fields (Sawyer 2001).

Although Kohn does not use the term “emergence” explicitly, clearly he means very similar things in his discussion of the “exchange paradigm”. Specifically, he argues for an interpretation of Adam Smith's invisible hand metaphor at variance with the usual one encountered in the “value paradigm”. He asserts that the conventional picture is sorely lacking, as it does not include any mechanism by which prices are formed. I have made a similar point in models of market processes that are more decentralized than neoclassical ones (Axtell 2005). In these non-Walrasian models an explicit price formation process is proposed that yields market-clearing prices asymptotically, while also giving rise to phenomena at variance with the neoclassical depiction of markets (e.g., price dispersion, wealth effects).

Example 2: Local price formation leading to the emergence of market clearing prices and market-generated inequality The mathematical existence of Walrasian equilibria (market clearing prices and allocations) is treated in neoclassical theory as a key result. However, the attainment of such equilibria is largely left untheorized. That is, mechanisms sufficient to yield such prices are not part of the usual story, except through the invocation of a mythical “auctioneer” who assembles agent demand and supply schedules and mysteriously comes up with the right price. As has been previously noted, both by economists critical of the neoclassical conception of general equilibrium as well as (and more recently) by computer scientists who have an interest in applying economic mechanisms to their systems, the actual computation of such prices is a difficult problem in general (Papadimitriou 1994). It is formally among a well-known class of “hard” problems in the theory of computational complexity. The intrinsically static character of the Walrasian equilibrium notion is evidenced by the somewhat peculiar requirement of the theory—and stipulation on agent behavior—that no trade be permitted before the market-clearing price has been computed. This is a necessary assumption of the static theory because if it were not maintained then trade at non-equilibrium prices would lead to wealth effects—some agents gaining wealth and some agents losing with respect to the (eventual) market clearing price—thus corrupting the notion of Walrasian equilibria from initial endow-

ments. Models that permit trade out of equilibrium were first developed a generation ago (Negishi 1961; Hahn 1962; Uzawa 1962) and fall under the non-descriptive moniker “non-Walrasian”. Recently, I have studied the computational complexity of such processes and discovered that they have some nicer properties (e.g., less complexity) in comparison with the Walrasian model. Indeed, the existence of price heterogeneity breaks the complexity barrier. However, it also leads to wealth effects of the kind alluded to above, which seems to make neoclassical economists very uncomfortable, but which, I believe, should be viewed as an asset of this perspective and not a liability. Indeed, Foley has argued that any statistically realistic theory of markets must be capable of producing such “horizontal inequality” among market participants—agents with identical endowments and preferences will, in general, wind up in different welfare states (Foley 1994). These two features of distributed, decentralized exchange (market) processes—price dispersion and wealth effects—lead to a further non-Walrasian feature of such market outcomes: market indeterminacy. That is, in general there will exist vast numbers of equilibria, and the particular history of trade relationships that obtain in a market “selects” the final outcome. Indeed, this aspect of these non-Walrasian models was seized upon by neoclassical critics in the 1960s as an important flaw in the approach, where today it might credibly be viewed as strength.

The kinds of decentralized market models just described yield, in an amount of time that is a polynomial in the number of agents and commodities, near equilibrium configurations—non-Walrasian equilibria (from initial endowments) but equilibria nonetheless (in the sense the no further gains from trade are possible). But earlier I argued that Kohn’s “exchange paradigm” was best described as a dynamic perspective on economics. How can these disparate views be squared?

One way to think about Walrasian equilibrium is that it is “thoroughly static” in the sense that von Neumann and Morgenstern use this phrase in the quote from *Games and Economic Behavior* at the start of this paper. Equilibrium in von Neumann and Morgenstern, as in Walras–Arrow–Debreu, is a configuration in which no agent has any incentive to unilaterally change. It is not the rest point of some dynamic process. However, the non-Walrasian market process described in Example 2 is intrinsically dynamic, with trades occurring among agents that move the market closer to some equilibrium, asymptotically approaching a time beyond which there does not exist a single further mutually beneficial trade. So conceiving of economic processes as dynamic may yield equilibrium configurations, but the path that the economy takes toward the equilibrium can matter as in the above where it generates horizontal inequality. It is also possible that the path does not settle down to an equilibrium configuration of agents as the next example illustrates.

Example 3: Agent-based models of financial markets Over a little more than a decade there has grown up a reasonably thorough-going treatment of financial markets from an interacting agents perspective (Palmer et al. 1994; Arthur et al. 1997; Lux 1998; LeBaron 2001a, b, 2002). The first authentic model of this kind is known as the Santa Fe Institute Artificial Stock Market and was created by an interdisciplinary team of economists, computer scientists, and physicists. In such models, agents allocate resources between a risky asset and riskless one. Agents have heterogeneous forecasts for the price of the risky asset and make money based on the accuracy of their forecasting functions. Furthermore, each agent has some mechanism for updating its forecasting function, whether through explicit learning (e.g., neural network training), recombination of previously successful forecasting rules (e.g., evolutionary algorithms), or perhaps simply by copying successful agents. By now something like a couple of generations of such agent-based financial

market models have appeared, and so the forecasting and learning rules have become richer, and the current generation of models have deep connections to empirical data on financial markets (e.g., Cont et al. 2005; Cont 2006). What is notable about such models from our present perspective is that while at each instant in time the artificial financial markets clear so that there is no excess demand, it is not the case that anything like a fixed-point equilibrium at the agent level obtains. That is, agents do not stop adjusting their behavior, even if they are making handsome profits. In these models all profit opportunities are transient and, if one waits long enough, all forecasting functions eventually become obsolete. There is perpetual adaptation by agents at the micro level as they try to “outsmart” one another. At the aggregate level there may emerge some kind of stationary state, but at the agent level the only constant is change, in which agents continuously co-evolve their strategies in response to the actions of the other agents.

What is clearly evident in these examples is populations of agents in which individuals are all making decisions purposively, to improve their welfare locally, but are not fully rational in the sense of being able to deduce the future history of their world and act optimally with respect to the expected future history. In a definite sense, there exists a *complexity barrier* to anything like rational expectations obtaining in such worlds. We explore this in the next subsection.

3.2 Complexity

The real economic world almost certainly shares much more in common with computational worlds of boundedly rational agents co-evolving with one another than it does to worlds of fully rational agents in fixed-point-equilibrium configurations. This is so because no agent ever knows enough to figure out how it should behave from here to eternity, whether a human or a software agent. There is a *veil of complexity* that exists over the future—we know the future will arrive, but its exact character is opaque to us now (Albin 1975). So instead of globally optimal decisions, purposive agents act relatively myopically and seek local optima, at best, probably preferring robust and resilient solutions to optimal ones, and certainly so in environments where it is costly to switch between (brittle) policies in response to exogenous events. To illustrate an economic environment where the veil of complexity is operational, consider the following.

Example 4: Agent-based models of firm formation and evolution For coalition formation problems in team production environments (increasing returns to scale and non-cooperative behavior, with agent input being costly and output being allocated on the basis of some imperfect compensation system), it can be shown that the pure strategy Nash equilibria are (dynamically) unstable once teams exceed some critical size (Axtell 2002). That is, for sufficiently large teams, perturbations in agent inputs lead to individual adjustments that do not settle down but rather produce an environment where agents perpetually find the optimal input levels computed in a previous time to be no longer valid and it is welfare-improving to alter their input level. Furthermore, if agents are permitted to leave their current team and join another team, there results a flux of agents between firms—at any instant, some leaving particular firms, others joining—such that all agents eventually discover that they can gain welfare by migrating between teams. In such an environment of perpetually changing coalitions, it is essentially impossible for an agent to accurately

forecast its input contributions and income more than a few periods out. An accurate forecast would involve having detailed knowledge of which agents are leaving one's team and which others are going to join. Anything short of having a multi-agent model of the economic environment in each agent's head is essentially guaranteed to yield inaccurate results. And even if it were possible to program software agents to each "run" multi-agent simulations of their local environments in their heads, this is probably not a good model for how humans make decisions in such circumstances (Davies and Stone 1995). The "second best" response to such a veil of complexity is to be adaptive. Agents join a team and do the best they can. They periodically see what they might achieve in the world outside their team, and when the performance of their team falls below their outside options, they change jobs. Such adaptive behavior seems quite natural, especially with respect to alternative models in which agents are required to have rational expectations, for example.

Boundedly rational agents interacting directly with one another in networks, out of equilibrium, create patterns and structures not of their own conscious planning (e.g., coalitions). When such emergent forms have functionality, they alter the (economic) environment in which the agents live, creating sufficient complexity that no individual agent can perfectly forecast the future.

Neoclassical mathematical economics abstracts from this real-world complexity with a series of assumptions—rationality, equilibrium, homogeneity—that makes models analytically tractable and the results easily summarized through comparison of fixed point end states. It is a "thoroughly static" approach, yielding simple outcomes suitable for drawing uncomplicated conclusions. This method—working static problems first and only turning to dynamics once the simpler problem is understood—is one that has served well the physical sciences. But controlled experiments are possible and often easy in such disciplines, and so laboratory situations can be arranged to first work out the statics, then later modified to address dynamical considerations. Neoclassical economics sometimes feels like a scientific enterprise stuck in its early, static phase. It has been without the facility of laboratory experiments (until recently) to reach such a state of development as to move beyond the first phase, and so it languishes. It tries to treat all economic phenomena, whether static or dynamic, with its static apparatus, leading to great technical machinations but modest empirical power.

So it seems that Kohn's "exchange paradigm" is a worthwhile program, although certainly difficult technically. For how will we, who see the foibles and limitations of the "value paradigm," vanquish the formal, technical difficulties associated with building models in which adaptive, purposive agents seek utility gains in networks away from equilibrium? On this question Kohn does not offer a coherent proposal. Rather, as too often the case, a resounding critique yields a powerful vacuum into which the critic pours only desiderata, which turn out to be an ineffectual bulwark against the nearly immediate reencroachment of the banished formalism. Nature abhors a vacuum and the continued use of the criticized methodology occurs not because the critique was impotent but rather because there is no alternative. If "it takes a theory to beat a theory," at the impressive level of abstraction that Kohn operates, where *all* extant theory has been subsumed into the "exchange paradigm," it is surely going to take a powerful methodology to supplant the whole of neoclassical economics!

To Kohn's credit, in a subsection curiously titled "The Relationship Between the Two Theoretical Approaches" he does apparently suggest an alternative to mathematical expression, one based on so-called cellular automata (Wolfram 2002). On one hand, this is strange—what does a tome from a rather remote branch of computer science, containing but

a few physical science examples and only the most stylized attempt at a social science application (a finance model), have to tell us about a methodology that is to replace mathematical economics? However, there is a certain sense in which this suggestion is right on the money, although not for the reasons given by Kohn. For in a little more than a decade there has grown up a set of computational techniques suitable for dealing with economic and other social systems, some of which have been alluded to above. In the next section we provide some background.

4 Multi-agent systems and economics

Distributed artificial intelligence (DAI) grew out of conventional, top-down artificial intelligence (AI). Instead of designing a single cognitive engine as in AI, DAI focuses on building many small, smart components and having higher performance emerge from the bottom up through *interactions* of the components. Conveniently implemented using object-oriented programming techniques, DAI morphed into multi-agent systems (MAS) in the 1990s, by giving each component a well-defined sense of self-interest. Given this history, MAS is usually viewed as a subfield of AI within computer science.

A somewhat parallel development occurred within the study of complex systems. From sandpiles to fluid mechanical turbulence, to immune system models and evolutionary biology, complexity grew out of the mantra of “simple components, complex systems.” Indeed, in each of these domains crucial progress was made by building computational models in which the state of the system was faithfully represented in distributed fashion and permitted to change over time according to relatively simple rules or algorithms. Early models of this type were cellular automata (CA), of the kind described by Wolfram (1994) and others (e.g., Codd 1968; Toffoli and Margolus 1987; Gutowitz 1990, 1991; Ermentrout and Edelstein-Keshet 1993). However, when biologists and ecologists expressed their desire to apply such models to their fields in the late 1980s and early 1990s, to model ant colonies and other social insects and animals (Grimm 1999; Grimm and Railsback 2005), the CA paradigm gave way to the MAS perspective that was growing up in computer science. Indeed, insofar as a CA is usually composed of identical copies of a discrete state automaton having nearest neighbor interactions and connected via a regular graph, it is a very special case of a MAS. This is so because the latter are nominally represented as either continuous or discrete automata having potentially long-range interactions (e.g., social networks) and connected via arbitrary graphs (including lattices).

It would prove only a matter of time before social scientists jumped on the individual agent modeling bandwagon (Gilbert and Doran 1994; Gilbert and Conte 1995; Bousquet 1996; Gilbert and Troitzsch 1999). Many early adopters of MAS techniques within economics were motivated to do so because it permits one to systematically move away from the “sweet spot” of neoclassical assumptions, in the direction of more descriptive realism (Axtell 2000).

4.1 Agent-based Computational Economics (ACE)

So the application of agent computing techniques within economics has been around for about a decade, and has been surveyed (Tsfatsion 1997, 2002, 2003). Kohn’s failure to mention any of this means that he either does not know this emerging literature or does not believe, as I do believe, that this is the obvious methodological successor to mathematical expression in economics—the natural way to realize his “exchange paradigm”.

Because I have written about the strengths and limitations of agents at some length (Axtell 2000), I will make no attempt to repeat that discussion here. Rather, here I shall comment, from an agent computing perspective, on Kohn's assertions concerning normative implications of the "exchange paradigm" and possible problems with "hybrid theory". While I find much to agree with him about, I also think that results obtained so far with agent-based models in economics point in a rather different direction than what one would conclude from reading Kohn.

On the matter of normative implications, it is certainly not the case that agent models of markets are incapable of shedding any light on the performance of markets, market failure, or market intervention.³ Consider agent-based financial market models of the type described in Example 3 above. The mere fact that trades happen is not in and of itself indicative that "gains from trade" are being had—perhaps this is true in consumer markets but certainly not in financial markets where speculative behavior matters and people are trading on the basis of beliefs, not preferences. In financial markets there are "objective" measures of market performance, although these may not be tied directly to agent welfare. Consider price volatility, an easily computed quantity and one that investors watch closely. In real financial markets, volatility levels depend on many things, including the rules a market operates under. Changing the rules—modifying institutional arrangements—can (presumably) have dramatic effects on volatility. Although normatively it is difficult to say whether all volatility is bad, certainly too much is counter-productive insofar as it impedes price discovery. Therefore, market rules that produce modest volatility are to be preferred to ones that yield high volatility. Government intervention in markets (e.g., by the SEC), as an overseer and regulator, can indeed have positive effects if it forces markets to adopt policies that are good for market performance and may not be implementable by the management of a market due to vested or competing interests. To be very concrete, imagine a market where trading in 1/8s and 1/16s is the norm, and there is pressure from dealers not to change this long-lived feature of the market. If a regulatory agency can show that volatility will be systematically reduced by decimalization and price discovery improved accordingly, then such a policy could have welfare benefits, even if the assessment of such benefits may be difficult to quantify exactly (Darley et al. 2001).

It is similar for market failure. A market is a multi-agent system and there is no reason to believe, *a priori*, that it is working as well as it might. Agents have competing interests, information is distributed, knowledge is tacit. It is easy to imagine, as in the case of financial markets, circumstances under which market behavior that emerges has very problematic properties. Remember, it is spontaneous-order arguments that are burdened by affirmative welfare properties, while merely emergent outcomes can have either positive or negative implications for the individuals involved. For example, in certain circumstances it may be possible to come up with real-world interventions by regulators or others tasked with managing markets that make such markets run better (e.g., transparency measures). Consider the production of externalities. Recently, there have been a large number of distinct market designs for trading such externalities. All the designs do not have the same properties, as clearly evidenced in the record of trading to date—some markets are thin, some are dominated by a few agents, while others seem to be too volatile (e.g., Hahn 1989). New designs try to remedy these problems and have differential success doing so. Usually, it is costly to actually implement policy changes in the field, but the newfound capabilities of agent modeling permit economists to experiment with such changes with a high degree of fidelity.

³ "For the exchange paradigm, the concept of market failure is meaningless" (Kohn 2004: 325).

4.2 Interactive computing

If there is a new scientific perspective implicit in the “exchange paradigm,” it is not CAs and pretty patterns on two-dimensional lattices. Rather, the novel computational paradigm that underlies agents has to do with what has come to be known as *interactive computing* (Wegner 1997; Wegner and Goldin 2003). Traditional computer science, as well as scientific computing, views *algorithms* as the way to turn exogenous *data* into answers. Solving equations, hashing data, approximating functions, efficiently storing internal computations—these are the guts of *numerical* computing. But in interactive computing there is no single data set, no one algorithm, and certainly no final answer. Rather, there are multiple data *streams* that autonomous *agents* inspect and glean information from while *interacting* with one another directly, possibly to make predictions about the content of future data streams, maybe to modify the structure, content, format, or function of such streams. There may be answers to be sought, but whole chains of interdependent answers, not single objective truths. The agents in such systems have some shared states, but are potentially quite heterogeneous and, when their histories are considered, completely unique and idiosyncratic. Such *interaction machines* are known to be at least as powerful as Turing machines, suggesting that it may be possible to redo the foundations of computer science from an interactionist perspective.

This view of interaction seems quite natural to social scientists, used to trying to model human behavior, as we accustomed are. Once again, think about a financial market. There exists a finite data stream, but it is sufficiently large as to be practically infinite. There is no one, right answer, but simply the goal of making money. There is no best way to do this—or if there is a best way it will not stay that way for long—but a lot of guesswork, heuristics, and rules of thumb about how to trade. The interactionist perspective in economics is sufficiently new that it has few adherents today, save those who have been consistently ahead of their time in thinking about interacting agent social science (e.g., Albin 1998).

4.3 What remains of economic principles?

If theorists can no longer count on any of the usual neoclassical assumptions, and must try to model bounded rationality and agent heterogeneity along with direct agent–agent interactions and non-equilibrium outcomes, is it possible to formulate any economic statements that have wide, perhaps universal applicability? To state this in a different way, if the only foundational principle that we will admit is purposive behavior between autonomous individuals interacting on arbitrary graphs, what can we say about economic and social processes *in general*?

In economics, we have lots of *qualitative* rules of thumb that seem to be empirically valid in specific circumstances—Gresham’s law and “competition is good for the economy” are two. But we have no “exact” laws as in the natural sciences—statements that are true always and everywhere, although perhaps overshadowed by other effects depending on the context (Cartwright 1983). Do price and quantity controls always lead to rationing (e.g., Bradburd et al. 2006)? Do minimum wage laws always have to generate higher unemployment? While these claims may be empirically tenable in specific environments, can they be always true independent of the environment? One of the features of the neoclassical knot of assumptions is that it dictates so much structure that large numbers of “principles” directly flow from them, from the law of one price to no arbitrage conditions at equilibrium to the very existence of economic equilibria and comparative advantage. These

“principles” are logical consequences of the assumptions, not empirically-validated statements of how real economies work! In relaxing these assumptions, we are certainly left with many fewer results that are universally true, but also with many fewer results that are empirically false although logically consistent with unfounded assumptions.

I have agreed with Kohn that the economics profession is either on the cusp or in the midst of significant methodological innovation. I further agree with him that an empirically accurate depiction of human behavior is a critical piece of economics, a research stream naturally pursued by economic psychologists and cognitive scientists, while the ability to ramify such specifications to the social level is a core job for the remainder of the economics profession.

What I disagree with Kohn about is the way such progress will be accomplished. Indeed, I have argued that rapid progress is evident at present, and the main methodological innovation is a suite of computationally oriented techniques that go under the rubric of “agent-based computational economics,” amounting to mixing modern computer science ideas (objects, agents) with specifications of individual economic behavior. Such a perspective leads naturally to the study of emergent form and function in the guise of multi-agent institutions, not emergent in the sense of being inexplicable, but rather in the sense of being phenomena that are in some sense above and beyond—or supervene on—the specific motivations of the individual agents in the economic system. Once we know what economic agents do, what economists should do follows directly: study the emergent economy in all its tangled, networked, dynamic diversity, using tools dictated by the character of the economy, not those that dictate the economy’s character.

Acknowledgement The author thank the organizers of the symposium for their helpful comments.

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