Ontic Structural Realism and Economics

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Ontic structural realism (OSR) is crucially motivated by empirical discoveries of fundamental physics. To this extent its potential to furnish a general metaphysics for science may appear limited. However, OSR also provides a good account of the progress that has been achieved over the decades in a formalized special science, economics. Furthermore, this has a basis in the ontology presupposed by economic theory, and is not just an artifact of formalization.

1. Introduction. Ontic structural realism (OSR) (Ladyman 1998, 2002; French and Ladyman 2003; Ladyman and Ross 2007) aims to resolve a decades-long impasse between Kuhnian skeptics about scientific progress and mainstream scientific realists who emphasize referential continuity across theory change. The impasse is most acute for naturalists. To them, skepticism distorts history, while realism freights the content of science with nonnatural transworld reference relations, essential natures, and other metaphysical exotica.

Naturalists should furthermore like OSR because it is based on physics, not conceptual intuitions. Its key claim is that the types featuring in fundamental physics (quarks, bosons, etc.) are not kinds of *entities* (D'Espagnat 2006; French and Krause 2006; Ladyman and Ross 2007), but recurrent aspects and/or modes of *structure*. Realists and Kuhnians share the assumption that if fundamental physical theory literally describes the world, this must be because the world is composed of entities corresponding to the theory's irreducible objects of predication. Saunders (2003, 130) gives the structural realist's reply: "There will always be a place for objects, understood as objects of predication; but I see no reason why objects of this sort should precisely line up with the constituents of reality." Achievement of this insight corresponds to what naturalists regard as the true form of philosophical progress: dissolution of antinomies

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in the light of scientifically inspired reform of background assumptions. OSR identifies one such reform by attention to the foundations of physics. Physics describes structural relations, not properties of little things from which reality is glued together. This allows putative Kuhnian discontinuities to be reanalyzed as cumulative improvements to mathematical models of physical reality, rather than as changes of the collective mind about what there is.

Current physical mathematics, especially models of quantum entanglement, are thus crucial to motivating OSR. It adds nothing to the evidence for this physics that it dissolves philosophical impasses; OSR begins with physical arguments and then welcomes the philosophical payoff as a happy consequence. This leaves its reach as a general philosophy of science unclear. Can OSR be more than a philosophy of physics? If all of science reduced to physics, this wouldn't matter. But it would be a dark day for naturalism if it were hostage to across-the-board ontological reductionism, which current trends in science don't support. OSR is a serious candidate for a general naturalistic philosophy of science just in case its positive content is motivated by mathematically formulated sciences besides physics, and thus by considerations of consilience and unification.

The caveat about mathematical formulation is important. OSR's very sense relies on the idea that the structures with which it identifies reality have definite content because they are described by mathematical models or (on a Platonist interpretation[1]) *are* mathematical structures. One should thus not expect to be able to motivate OSR by appeal to disciplines such as sociology that lack distinctive canonical formal theory for interpreting quantitatively parameterized models. Many sociologists would agree that their ultimate subjects of study are structures rather than individuals. Kincaid (1997) makes a general negative case for anti-individualism across the social and biological sciences. But the job of OSR as a positive metaphysic of science is to say something about what *does* exist (in general) as empirically interpreted mathematical structures. The philosopher cannot be expected to be able to do this in domains where scientists have not yet achieved the relevant identifications.

Among social sciences, economics has the requisite mathematical structure, in which it expresses a suite of standard theories. Periodically, as the suite is augmented, its explicit unification is updated; milestones are Samuelson (1947) and Debreu (1959). In opposition, so-called 'heterodox' economics questions the canonical formulations. This challenge falls broadly into two waves. First, there is a busy anti-economic literature based on resentment of economists' role in social and financial power

1. I think Bertrand Russell shows it is possible to be a naturalist and a Platonist. But I cannot go into this here.

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structures which has no genuine alternative theory to offer (Ross 2009). This political literature is of no relevance to metaphysics. Then there is what I will call 'nostalgic' economics that resists the way in which economics has matured through increased formalization. This version of heterodoxy, of which Sen (1977, 1987, 1999, and elsewhere) is a famous example,[2] is more interesting.

Professional economics has both scientific and engineering wings (Mankiw 2006). As with physical engineering, attempts to use economics to promote human flourishing generally need to engage with folk ontologies that, for reasons to be addressed, remain atomistic. But by reading *scientific* economics through the lens of OSR we can shed light on some institutional features of professional economics that arise from the cohabitation of the two wings.

2. The Objects of Economics. The impasse between realism and instrumentalism in physics that OSR aims to resolve arises from the fact that the putative basic objects of physics are 'theoretical entities'. By contrast, the apparent basic objects of economics look directly observable and pretheoretical: consumers, producers, and exchanges of goods and services. Mäki (1992) argues in a well-known paper that Milton Friedman, the supposed champion of instrumentalism in economic methodology, doesn't resemble philosophers who are agnostic about the theory-independent existence of particles in fundamental physics because he doesn't doubt the reality of everyday economic objects (types or tokens). Rather, he denies that economic theory aims to truly describe general relations among these objects. If economics is mainly in the business of predicting behavior of types of everyday entities, then it seems that OSR can't characterize the ontology of economics unless consumers, producers, and acts of exchange reduce to structures that figure in fundamental physics. As noted above, there is no naturalistic reason (i.e., a reason based in the current content of science) for thinking that they do.

Stigum (1990, 550) reminds us not to confuse a concept's 'everydayness' with its independence from theory. "We have knowledge by acquaintance," he points out, "of the salary we received last year, but we have knowledge by description only of what our income was, i.e., of the maximum amount of money we could have spent last year and been as wealthy at the end of the year as we had been at the beginning of the year." Economic theory, Stigum suggests, traffics mainly in such kinds, which nontheorists regularly evoke but which have no definite referents outside of a theoretical framework. So, again, "we have knowledge by acquaintance of the price

2. Other examples include Heibroner and Milberg (1995) and Hollis and Nell (1975).

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of our house, but only knowledge by description of its current market value" (ibid.). This point does not impress nostalgic economists such as Sen, who insist that economics has an obligation (the source of which is obscure) to be *directly* 'about' people, which (for some reason) should not be constructed, as a physicist would construct an everyday rock or table, from theoretical entities.

The idea that economic science, as opposed to economic engineering, is directly about people and their properties strains interpretation of the history of economic theory. Or, at least, it does so on a progressive interpretation of that history. This is of course just the sort of story that OSR helps us to tell about the history of physics in the face of Kuhnian skepticism. I do not mean to argue that the history of economics must come out as a history of progress and to then defend OSR because it supports such a history. My claim is more modest: insofar as economics has made progress, this has consisted in deepening our knowledge of abstract structures. By contrast, though economists have borrowed new knowledge about people and their properties from neighboring disciplines such as psychology and anthropology, they have contributed relatively little to this knowledge themselves.[3] Adam Smith had a deeper understanding of human behavior than many modern psychologists, but he did not know much economics by contemporary standards.

I will contrast two conceptions of economics that have contested for the discipline's soul during its modern history, but using contemporary terminology not found in the earlier stages of this history. I will then justify the claim that this anachronistic usage well characterizes implicit targets of the kind of progress at which historical economists successfully aimed. OSR makes sense of this claim.

On the first conception, economics is any body of theory or application of theory that generalizes over maximizing, optimizing, or meliorating relationships among (i) utility functions, (ii) scarce production inputs, and (iii) reallocations of (ii) with reference to (i). This characterization makes no reference to people or groups of them. It is equivalent in spirit to what Rosenberg (2009) calls a "nightmare" view of economics[4] as consisting in any model or application of linear or dynamic programming—I say "in spirit" because we must add game theory to the fundamental tool kit, but Rosenberg leaves it out. Call this 'Debrevian' economics. What contrasting pleasant dream might someone who considers it a nightmare have in mind? Here is an alternative conception (consistent with Rosenberg's suggestion): economics is any body or application of theory that gener-

3. The new 'neuroeconomics' may offer an exception.

4. Rosenberg hasn't always regarded conceptions of this general sort as nightmarish; see Rosenberg 1992.

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alizes over the behavior of some specified class of people or their aggregates when they take actions to optimize or improve their well-being with respect to recruitment of scarce assets. Nostalgic economists including Sen stand on this conception when they criticize Debrevian economics, usually referred to less perspicuously as 'neoclassical' or 'Walrasian' economics.

The recent polarization of debate in metaeconomics (see, e.g., Ormerod 1994; Lawson 1997; Gintis 2000) that pits 'anti-neoclassical' rebels against 'neoclassical' sticks-in-the-mud has a self-conscious air of (pop) Kuhnianism. However, it distorts history and misrepresents most economists' day-to-day practice. The reference of 'neoclassical' in these critiques is highly variable. The term always denotes 'establishment' figures, but not always the same ones. Often it just vaguely means what was taught in top American, Western European, and Indian economics departments in the 1950s through 1980s. Depending on whether the commentator thinks that game theory is an extension of mainstream theory or the basis of its overthrow—both views are common—the period of neoclassical hegemony in microeconomics is sometimes extended to the present. Despite typically presenting their broad vision and special constructs as 'cutting edge', anti-neoclassicists often identify with a lost golden age in the discipline. Adam Smith is regularly interpreted as a prescient post-Walrasian whose social-psychological emphasis was later neglected. Anti-neoclassicists are usually warm toward Keynes for obvious reasons, and often, because he resisted the hegemony of general equilibrium reasoning, toward Marshall. Hayek is appreciated for emphasizing learning and dynamics and for dismissing the significance of general equilibrium. Ricardo, once a favorite with promoters of a new paradigm because he inspired Marx, Sraffa, and Robinson, seems to have fallen into relative disfavor, partly due to the downgrading of Marx and partly because Marshall criticized him for using overly abstract mathematics.

This Kuhnian tale is on its best ground concerning Smith. (Historical narratives of sciences emphasizing discontinuity naturally look best when describing the earliest developments.) Influenced by Locke and Hume, Smith indeed tended to explain properties of economies in terms of personal psychological properties of traders (even though his theory of the social formation of the person was two centuries ahead of its time). His nineteenth-century successors—Bentham, Mill, Jevons, Walras—followed him in this. Yet from the moment when theorists began self-consciously building a distinct discipline of political economy, we observe consistently diminishing reliance on psychological hypotheses. First Jevons reduced the psychology of the economic agent to diminishing marginal utility in consumption, the mechanism of which he sealed in a black box. Then Edgeworth and Fisher taught economists how to eliminate utility as a hypothetical quantum, to concentrate on inferring price equilibria from

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gradients of indifference curves. Pareto argued for, but did not complete, reducing inferred utility to observable demand. Hicks and Allen then showed in 1934 that the convexity of the demand function does not require the substantive psychological hypothesis of diminishing marginal utility. Samuelson brought proper mathematics to bear on the mission bequeathed by Pareto and Hicks, as a result of which, in his 1947 foundational work, the individual agent disappeared from the core of microeconomics altogether: in Samuelson the elementary subject matter, taking the mathematics at face value, consists in schedules of aggregate demand and supply and their resulting interaction as flows of consumption and production. Debreu (1959) finished the job by producing an elegant formalization of general economic valuation under certainty and its computation in general equilibrium, which Arrow (1965) showed how to extend to generate predictions of demand for asset classes given distributions of consumer life spans and attitudes toward risk. Akerlof introduced information asymmetries as sources of market imperfections on the basis of game-theoretic insights, but this became a core part of standard theory when Stiglitz built it onto transparently Arrowian foundations. Stigum (1990) shows how to consistently axiomatize all of this in first-order logic and set theory.

This is a familiar story, especially when sketched so broadly. I rehearse it here in order to set up contrasting Whiggish and Kuhnian spins one can put on it. I have just outlined the Whiggish version according to which one technical advance is smoothly taken up into the next, leading to the steady growth of a more powerful, more unified edifice of constantly increasing scope. In opposition, the standard Kuhnian account reads the abandonment of psychological utility as the great discontinuity in the history of modern economics. Contemporary behavioral economists presuppose this account when they claim to have recovered the lost paradigm by reintroducing psychology into microeconomics. This is needed, they say, because the 'amnesiac' theory incorporates an empirically false model of the human agent. On that view economics must indeed struggle to claim a history of general progress. Among the more polemically aggressive of the 'Kuhnians', Gintis routinely takes care to remind us how much representational power and rigorous technique were enhanced when economics was formalized, so he does not regret the long Debrevian episode. However, this is not the sort of progress that is relevant to either scientific realists or their critics among philosophers of science. From their point of view, if the new behavioral anti-neoclassicists are right in both their science and their history of economics, the discipline spent decades digging a hole—no less of a hole if the digging built stronger muscles.

The Kuhnian history is selective. A leading theme of the anti-neoclas-

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sical revolt is that neoclassical economics is atomistic, insisting on building models of markets and whole economies out of entities that selfishly maximize their own narrow welfare. Sen explicitly insists that this aspect of the 'degenerating paradigm' is built into the Debrevian formalism through the required separability of the utility function. Is this true? Suppose one accepts my view of economics as the study of attempted optimization in systems that respond differentially to scarce possible states that are hierarchically organized with respect to those states' contributions to the systems' stability. The systems in question must be assigned boundaries, so are to that extent individual optimizers. But Sen, despite his voluminous output on the matter, ignores the fact that no mathematical work in the foundations of postwar economic theory is done by any assumption to the effect that maximization or optimization is the typical target of a person or even of any individual entity in the folk sense. A 'consumer' in microeconomic theory is a triple consisting of a constant, a set of scarce states that constitute improvements on some metric ('commodities'), and a set of achieved states that can be exchanged for elements from the first set ('a budget'). What are literally maximized in equilibrium are variables in simultaneous field equations interpreted as values on preference fields, which may or may not correspond to consumption agendas of actual people. If we let the mathematics speak for itself, there is nothing in economic theory that advises people to be selfish, to be foolishly rational, or, indeed, to be or to do anything in particular at all.

The critic objects: economists *interpret* their formalism, and if they didn't interpret it in certain ways, people would resist calling it 'economics' (as opposed to the mathematics of linear and dynamic programming and game theory). Practically, we might then wonder why we should pay them to give advice on how to make people wealthier. Here, however, is where the distinction between economic science and economic engineering needs emphasis. Economic *science* has been evolving steadily and cumulatively since at least Walras's time into the current theory of linear and dynamic programming in (increasing) interanimation with game theory. The properties studied in economic models are not best regarded as properties of people or as proxies for them. They are typically aggregate properties of stylized markets and stylized populations, not individuals, and, contrary to what people often suppose on the basis of one year of university economics with its tiny toy systems of shoppers and apple growers, these aggregate properties are almost never assembled out of expressions that map properties to individuals in engineering applications. Admittedly, models of small-n games do distinguish idiosyncratic features of single utility functions; but in abstract game theory there is no presumption that such functions are best thought of as models of the preferences of people,

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as opposed to behavioral patterns—'revealed preferences'—of firms or countries or complexes of genes.

So whence comes the widespread presumption, shared by many economists, that microeconomic theory requires methodological individualism? The detailed history of atomistic individualism as it relates to economics has yet to be written, with twists more interesting than the prevailing Kuhnian narrative of a lost Adam Smith whose *Wealth of Nations* got detached from his *Theory of the Moral Sentiments* during a long night of neoclassical antihumanism. The Victorians *were* high atomists, both metaphysically and ideologically, and so the familiar history handles Jevons tolerably. But the version that lionizes Keynes while turning Samuelson and Arrow into high priests of neoclassicism is bizarre; Keynes's influence was immensely expanded by the Samuelsonian (and more generally postwar) aim to infer welfare properties from aggregate demand properties. And simultaneously with this emphasis on the aggregate in supposed High Neoclassicism came the rise of game theory. Cooperative game theory, which briefly flourished first, is obviously not individualistic. But even the more important noncooperative game theory takes an individualist cast only if applications insert it. The solution to a noncooperative game, after all, is a vector of strategies, and distributions of outcomes in sets of games, intended by no one, are structural properties par excellence, not qualitative properties of individuals or of anything else. These are overwhelmingly the principal sorts of properties that interest applied game theorists; while the properties that interest pure game theorists are (unsurprisingly) strictly mathematical (typically, properties of vector spaces).

Two factors are mainly responsible for the amnesiac conviction that contemporary economics is the product of a neoclassical tradition built around individualism (and therefore ripe for a Kuhnian revolution). The first is the way in which economics is taught: one begins by interpreting simple utility and production functions as solutions to resource-allocation problems facing individuals, and then spends several semesters adding elements to these closed scenarios. This pedagogical model is locked in and would be difficult to change (though see Bowles et al. 2005 for an effort). However, it has little relationship to anything that either economic scientists or economic engineers do outside the classroom. In 10 years of devoting half my professional time to microeconomic policy consulting I have never estimated a personal utility function on the job, and I know of no one who has. I earlier acknowledged that microeconomic *theory* has the theory of the consumer at its core. But this 'consumer' is merely the operator of a linear optimization program for computing a certain class of functions; there is no basis in the mathematics for interpreting

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the 'consumer' as a model of a person roaming a grocery store.[5] Economists in fact do not give the idea this interpretation except in the undergraduate classroom. But the undergraduate classroom is where most noneconomists who learn any economics at all pick up acquaintance with the discipline.

The second basis for the conviction that economics is dominated by atomistic neoclassicism is the successful promotion by the Chicago School of libertarian ideology as if this were part of economic science. It is not: monetarism is an approach to macroeconomic engineering, just as privatization of highways is an approach to microeconomic engineering. Though several were great theorists, the Chicago economists were generally proud of being at least as motivated by policy concerns as by scientific ones, and their relative disinterest in computational economics was an expression of this. Through their public roles, Friedman, Stigler, Coase, and colleagues became the main face of postwar establishment economics. And then Friedman produced the single most influential contribution on economic methodology ever written. In its celebration of prediction and disdain for explanation and generalization, Friedman's (1953) methodological essay is transparently the manifesto of an engineer rather than of a scientist.

Clearly, the anti-neoclassical revolt at least often has ideological motivations. I endorse the view of any critic who stresses that libertarianism is political philosophy, not economics, and who therefore seeks to drive a wedge between the economics brand and the Chicago brand. My first point above, however, is that confusion of the neoclassical brand with the Chicago brand in the public mind is equally in need of undoing. My second point is that in formulating theses about the ontology of economic science we should ignore work that is mainly economic engineering, which is what preoccupied Chicago economists. (This is not a jib at their status: I think that good economic engineering is more important than economic science, and that the former probably wouldn't shrivel in the absence of the latter. But my topic in this essay is economic science.)

3. OSR as the Ontology of Economics. Economic engineers address problems confronted by people or groups of people in their roles as producers and consumers of goods and services. It is natural that they conduct their business in the terms of the everyday folk ontology of their clients. Philosophers sometimes forget that engineering is overwhelmingly the largest part of the activity of most economists. This partly explains the stub-

5. Thus Sen is right to point out that consumers are misleading models of people. But he assumes without argument that this must indicate a profound mistake in the foundations of economic theory.

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bornness of the traditional pedagogy: we know our students expect to hear about shoppers and workers and their choices in economics classes, and would be nonplussed if we instead talked about nothing but functions and fields and computations.

This should not obscure the fact, however, that what economics has accumulated is understanding of patterns in optimization, maximization, and melioration by systems of elements for which some states of affairs are more valuable than others. The basic objects of economic theory are optimization problems. Of these, the most interesting, both mathematically and in application, are nonparametric ones—that is, games. These include the small-*n* games familiar to philosophers, but also the large-*n* investment, savings, and consumption games that will soon dominate macroeconomics the way small-*n* games dominate industrial-organization theory. Games are mathematical structures, networks of relationships whose relata are distinguished as such by the mathematical representations of the structures in question—just like quarks and bosons. OSR thus predicts the right sort of ontology for economic science, and the sort under which the conviction that of course we have learned vastly more economics than Smith or Jevons knew is ratified.

A worry dangles. Does the ontology of economics as interpreted by OSR have any metaphysical significance in the way the ontology of physics as interpreted by OSR does? According to OSR, individual objects in physics are heuristics, bookkeeping devices that help investigators manipulate partial models of reality so as to stay focused on common regions of measurement from one probe to the next. I contend that the everyday objects in terms of which economic theory is interpreted also have this character. Elsewhere (Ross 2005, 2006) I have defended the view that people as these figure in social sciences are not identical to *H. sapiens* organisms. The latter are products of genetic evolution. People, on the other hand, are normatively regulated virtual constructs that arise out of complex dynamics operating on multiple interacting scales of measurement resolution. These scales include genetic-evolutionary dynamics, but also cultural-evolutionary dynamics and information-processing dynamics at micro timescales both within the complex *H. sapiens* brain and at the level of social signaling. Summarizing drastically, people are stabilization devices in social-evolutionary dynamics and, simultaneously, stabilizers of the processing dynamics of their own brains through their representation, to themselves, of socially coded and learned expectations. It is because individuals are constructed to be reference points for keeping the books in the networks of reciprocity and norm enforcement that hold societies together that individuality is so important to people. This in turn is why folk social explanation typically starts and ends with individual

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motives and individual behavior. For practical purposes, we have no other option.

Economic theory takes a more objective perspective. The thin concept of agency in economic science identifies agents with the gravitational centers of consistent preference fields. The theory incorporates no thesis about which empirical entities implement such roles. Nor does it entail anything about how long their embodiment typically persists. Agents may be as transient as a modeler likes; so although agents may not change their preferences and remain the same agents, people may do so and can simply be modeled as successions of economically related agents. Davis (2003), assuming that people should be the paradigmatic economic agents, puts these points to work in mounting a complaint against mainstream (as usual, 'neoclassical') economic theory that echoes Sen's. We can endorse his concern up to a point: we would have grounds for disquiet if economic theory turned out to have no useful applications at all to human behavior. However, as argued in Ross 2005, it is a non sequitur to jump to such disquiet from the weaker claim, endorsed in my book, that (among organisms) insects, because their preferences don't change, are better exemplars of basic economic agency than people (while people approximate such agency from time to time in something like the way that countries do), or from the claim that economic theory attributes no distinctive properties to people. The relationship between everyday individuals and economic agents is flexible so far as the modeling technology is concerned. People are to economic theory roughly as tables and rocks are to physical theory. OSR as applied to physics doesn't deny that there are tables or rocks, and it explains how physical theory can provide (improving) explanations of their behavior despite denying that they are good models of fundamental reality. (That is, it denies that fundamental reality is a collection of objects like rocks, but smaller.) Similarly, economic theory is not a set of propositions about entities like people, but meaner. In this respect, economic theory exactly resembles physical theory, just as Jevons and Walras hoped it would.

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