Economics is converging with sociology but not with psychology

Don Ross

School of Society, Politics, and Ethics
University College Cork

School of Economics and Research Unit in Behavioural Economics and Neuroeconomics (RUBEN)
University of Cape Town

Center for the Economic Analysis of Risk (CEAR), Robinson College of Business
Georgia State University

don.ross931@gmail.com

July 2021

Abstract

The rise of behavioral economics since the 1980s led to richer mutual influence between economic and psychological theory and experimentation. However, as behavioral economics has become increasingly integrated into the main stream in economics, and as psychology has remained damagingly methodologically conservative, this convergence has recently gone into reverse. At the same time, growing appreciation among economists of the limitations of atomistic individualism, along with advantages in econometric modeling flexibility by comparison with psychometrics, is leading economists to become more pluralistic than psychologists about the ontology of behavioral causation and structures. This, combined with economists' growing interest in network models, is drawing economists closer in theory and practice to sociologists who use quantitative or mixed methods.

JEl codes: A12, A14, B41, C91, C92, D01, D85, D91, E71

Acknowledgment: Thanks to Keith Dowding, Glenn Harrison, Andre Hofmeyr, and Harold Kincaid for helpful comments on a previous draft.

1 Introduction

Since the wave of interest in behavioral economics picked up serious momentum in the first years of the current century, the interdisciplinary frontier between economics and psychology has been a zone of intense activity. When Thaler (1992) gathered up "anomalies" arising from comparison of economic models of rational agency with psychological experiments concerning human choices, this was framed as a challenge to the "mainstream" in economics. As data arising from new technologies for observations of behavior in neuroscience were introduced into the picture, this rhetorical theme was pressed more strongly: Camerer et al (2005) argued that microeconomics should have neuropsychological foundations (though at least two of the three authors have hedged and qualified this view more recently). A strident and highly salient backlash, denying that economists should pay attention to psychology or neuroscience at all, was circulated for several years by Gul and Pesendorfer (GP) before being published in 2008. The debate triggered by GP's *dictat* served mainly to show how much the sand had shifted – I am not aware of a single prominent economic theorist who expressed full agreement with them in print. Within the next decade, Chetty (2015) arguably spoke for orthodox opinion when, in a plenary lecture at an American Economic Association meeting, he urged "pragmatic" blending of economic and psychological theory and method in efforts to inform economic policy. Bibliometric studies (Geiger 2017, Rawat 2019) offer support to Erik Angner's (2019) widely retailed announcement of the triumph of the revolution under the banner 'We're all behavioral economists now'.

If economists are now paying more attention to results from psychology than they generally did in the in the middle and late years of the previous century, this is surely a good thing. GP's isolationist bluster is just that, remarkably unsupported by argument, as opposed to mere assertion of what economics should be about. I will argue, however, that to infer from the obvious expansion of behavioral and experimental economics over the past few decades that economics is becoming more integrated with psychology would be rash. I will suggest that the influence of psychological research in economics is in fact currently *shrinking*, for identifiable reasons, except within a specific research program focused on so-called social preferences (and even within that interdisciplinary area, the influence of other social sciences on economists is rising faster). Then I will argue that what we are mainly learning from the progress of that program, and others, is that psychology is the wrong discipline to turn to for either theoretical or methodological support in investigating the phenomena that interest behavioral economists (or, to the extent that Angner's rhetoric reflects reality, microeconomists in general). The more promising zone of integration lies with sociology.

I have previously (Ross 2014) defended at book length the general view that economics and sociology are more natural partners than economics and psychology. Even in that expansive frame, the claim is developed only generically and abstractly. Integrating the full suite of motivation for the thesis relies on both historicized philosophy of social science, and two technical arguments comparing, respectively, psychological game theory (Coleman 2003; Battigalli and Dufwenberg 2007; Azar 2018) with game theory based on revealed preference (Binmore 2007a, 2009), and techniques of psychometric with econometric data model identification and estimation. This all calls for another book-length treatment. The present short paper can attempt no such grand project. Thus I do not aim here to convert the skeptical reader to my opinion. My much more modest ambition for the moment is simply to tell an alternative story to the standard one, in hopes of recruiting well-informed participants to at least join a debate.

In the spirit of Rubinstein (2012), I will proceed by narrating three fables: a fable of psychology, a fable of sociology, and a fable of economics. Calling them 'fables' signifies that they are fictions of a certain sort. Specifically, they are idealizations of the methodological norms and broad background assumptions of each discipline, since the roughly coincidental emergence of cognitive science and the takeover of microeconomics by game theory in the 1980s. These idealizations are constructed in terms of philosophy of science. Use of this perspective does not reflect any idea to the effect that philosophy does or should regulate science or set a priori standards for it. One of the purposes of philosophy of science is to provide a synthesizing framework from which different disciplines can be examined for shared or conflicting assumptions about more general features of the one objective world they all aim to model and explain.

The fables abstract away from nuance and from large tangles of underlying details and qualifications. Sensible scientists do not self-consciously try to embody idealized conceptions of their disciplines; they try to understand actual empirical phenomena, which are generally not orderly. But science is organized into disciplines for sound practical reasons, mainly to facilitate *accumulation* of knowledge of principles, as distinct from mere curation of observations, and to minimize wasteful reinventions of exploratory technologies. Idealized disciplinary ambitions exert influence through the priorities of journal editors, the design of graduate school curricula, and the decisions of selection and tenure committees. Fables disguise some tensions in research programs and highlight others, which then come to be treated as the major challenges where scientific reputations can be made.

Section 2 constructs a fable of recent and current psychology. Sections 3 and 4 repeat this exercise for sociology and economics, respectively. Section 5 provides reasons for thinking that the fables of sociology and economics are currently showing stronger convergence than the fables of psychology and economics. Section 6, a fable of the near future, concludes with bolder speculation about emerging pathways by which sociologists and economists might become true collaborators.

2. A Fable of Psychology

Since psychology first emerged as a discipline distinct from philosophy, the representative psychologist has gyrated between emphasizing internal and external arenas of behavioral causation. Classical behaviorism, of the sober (Watson 1924) and grandiose (Skinner 1953) varieties, in promoting mechanistic accounts of brains as slaves of conditioning contingencies, pointed toward denial of the existence of mind. At its most abstract scale of description, what makes a psychological program behavioristic is attributing most *variation* in the causal nexus of behavior to an organism's environment, with the response principles engineered into nervous systems by natural selection providing constraints. The psychologist's most important mission is then to identify and generalize the constraints in question. Characterization of the social and functional objectives and targets of behavior is left to others – mainly anthropologists, sociologists, and philosophers.

Since, where humans are concerned, these goals apparently vary widely across cultures and circumstances, nervous-system mechanisms governing integrated actions tend to be characterized as highly functionally plastic in people, while the same mechanisms in non-humans are viewed as tropic (Hinde 1982).

My fable is spun against the foil of a widely told competing one, which has been especially dominant among philosophers of mind (e.g. Flanagan 1984, Haugeland 1985, Johnson-Laird 1988). According to this narrative foil, the 'cognitive revolution', set off by Chomsky (1959) and inspired by computational models of biological information processing, eclipsed behaviorism and restored the mind, not so different from the old conceptions of the philosophers, as a fit object for scientific study (Brook 1994, 2007). Notwithstanding an initial residue of over-emphasis on pure cognition, floodgates soon opened to the rehabilitation of topics shunned by behaviorists: emotions (Damasio 1994) and consciousness (Baars 1988; Marcel and Bisiach 1988). Minds were modeled as complex and *endogenously* adaptable, supervenient on, rather than reducible to, the brain, and only loosely constrained by its physical structures.

This philosophers' story would have achieved no traction if it had not consolidated real theoretical and empirical advances. At the conceptual level it promoted two important insights. First, it forced the sciences of the mental away from ambivalence between ontological reductionism and eliminativism. *If* there are true generalizations about minds then these are not about brains. Notwithstanding a few recalcitrant voices such as Bickle (1998), the rise of cognitive science put an end to mind-brain identity theory, of both type and token forms. The two-part ontological issue that matters to psychological methodology is whether minds supervene on brains in some relatively strict sense, and if they do not whether the concept of mind should or should not be abandoned as an inheritance from folk proto-science. Second, the core founding idea of cognitive science, that brains are information-processing systems, as are minds if they exist in any scientifically objective sense, has become a standing principle of behavioral modeling.

What is frequently obscured by the philosophers' fable is that neither of these conceptual consolidations threatens behaviorist methodology in psychology. Notwithstanding Skinner's grandiose philosophical agenda, behaviorism is not essentially an ontological idea. It is, rather, the methodological package implied by the hypothesis that the preponderance of the variation that drives both developmental and cross-sectional response plasticity arises in the external environment. If this hypothesis is false, because the lion's share of the causal action in the deep dynamics of intelligent behavior is endogenous to nervous systems, then psychologists' methods should be closely entangled with those of computer science and generative linguistics. If the hypothesis is true then the boundaries between psychology and other sciences that concentrate on features of organisms' environments are implicitly up for grabs.

Cognitive science of the past (roughly) 35 years has developed in directions that are highly favorable to behaviorism as understood above. The rise of connectionism in

AI, which idealizes neural organization as dynamic adaptation under pressure from perceived patterns (Clark 1989, Caudill and Butler 1990, Bechtel and Abrahamsen 1991)[1]; the steadily increasing emphasis, particularly in robotics and emotion research, on cognition as crucially *embodied* (Varela et al 1991, Steels and Brooks 1995, Gibbs 2005, Clark 2008); and increasing emphasis on continuities between human and non-human animal cognition as reactive to and supported by environmental contingencies (Caporael et al 2014), jointly motivate cognitive scientists to look for causal drivers of both cognitive structures and cognitive dynamics in external environments (including somatic structures outside nervous systems). At least as important as these grand shifts in theory, behaviorist methodology for the experimental lab, with its emphasis on relatively direct observability, never ceased to be taught to apprentice psychologists as fundamental. Certainly, since the rise of cognitivist models of perception, language, and attention, psychologists typically aim to use their traditional methods to test hypotheses about internal representational structures. But, as reflected in the absence of structural modeling in psychometrics, these hypotheses are typically qualitative interpretations (Oberauer and Lewandowsky 2019; Yarkoni 2020), while the literal (quantitative) elements of low-level data models remain continuous with their avowedly behaviorist histories. This is reflected in the retention of t-testing and F-testing as the core approach to analysis of experimental results, notwithstanding Ward Edwards's (1961) unequivocal warning six decades ago that psychologists needed to move beyond reliance on such low-information inferences.[2] Throughout his career, Paul Meehl was another insufficiently heeded evangelist for reform of inferential methods (Meehl 2006).

In general, the dominant philosophy of mind of the late twentieth century, built around the core idea of the mind as a functionally interpreted description of computational process carried out by the brain (with Cummins [1984] and Pylyshyn [1985] as classic expositions of subtly different varieties) has largely collapsed. Increasingly there are two live options open to current practitioners of mainstream experimental psychology with respect to the philosophical question: "What is the mind?".[3]

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[1] As an example of nuance here that fabulism suppresses, see Hinton (1991). On the other hand, the history of AI over the past three decades has substantiated the alarm expressed by Fodor and Pylyshyn (1988) that connectionism threatened the general paradigm in cognitive science that they championed.

[2] Edwards said "Unless someone … leads psychological statistics out of the wilderness of t and F tests, psychological experiments may find themselves still seeking the magic 0.05 level when to statisticians and other applied scientists the notion of significance level is only a historical curiosity" (1961, p. 474).

[3] Many, perhaps most, psychologists see little practical advantage in taking *any* philosophical stand here, just as many or most economists avoid having a clear opinion on the equally philosophical question "What is practical rationality?". Science sometimes solves philosophical questions by ignoring them until they

One option is to follow Dennett (1978, 1987, 1996, 1998, 2017) and a growing flotilla of his direct and indirect followers (McClamrock 1995; Clark 1997; Bogdan 1997, 2000, 2009, 2010, 2013; Hutto 2008; Zawidzki 2013) in conceiving of minds as *socially* sculpted virtual structures that integrate observed behavioral patterns with normative expectations about relative dynamic stability and functional intelligibility. On this kind of account, minds are naturally arising social constructs[4] that coordinate social interaction, facilitate joint planning, support division of labor, and normatively regulate competition. Shared mental attributions allow agents to find strategic equilibria in situations modeled as games. In people, minds primarily take the form of personal narratives that individuals cultivate, display, and use to try to influence other minds. People do not so much *read* one another's minds – since they are not hidden internal causes of behavior – as mutually *shape* one another's minds through implicitly negotiated ascriptions of biographical details (Zawidzki 2013). The basis of personhood, on this account, is self-attribution of mind, which children learn under active parental reinforcement (McGeer 2001, 2009, 2020).

The other option is to deny that minds are ultimately sound scientific constructs, favoring research programs aimed at eliminating these constructs from psychology in favor of elements of an emerging behavioral neurobiology. This is seldom semantically understood, at least by psychologists, as *eliminativism* about the mental in the sense of Churchland (1981). It is, rather, interpreted as *reconstruction* of the idea of mental information processing without use of folk ideas such as beliefs and intentions.[5] This seems to be the view urged on experimental psychologists by Chater (2018).

I refer to these as 'options' not in the sense of philosophical doctrines between which psychologists explicitly choose (see note 3 above), but as interpretive tendencies reflected in the opening and closing sections of psychological research articles. Within cognitive science as a whole, both options are flourishing in the sense of driving active research programs. My sense is that the majority of philosophers of cognitive science have come to favor the first option, and they are joined in this by evolutionary theorists in various contributing disciplines, especially anthropology (Henrich 2016), but also evolutionary psychology (Heyes 2018). However, the second orientation is the more natural interpretation of most of the literature in experimental psychology proper. The reason is practical. If minds are elements of social contexts, then it is not clear that they can readily be studied by

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become obsolete; sometimes it quarantines them by replacing thick concepts with thinner ones. Much activity in philosophy and history of science involves keeping track of these subtle and complicated dynamics.

[4] 'Naturally arising' merits emphasis. This signifies two aspects of this social construction: all normally functioning humans participate in it, and have done so for millions of years; and almost none have been aware of it *as* construction, partly because it is communal and partly because it is natural.

[5] I pass over the question of whether there is any disagreement between philosophers and psychologists here that is not *merely* semantic. I suspect not.

presenting tasks to isolated subjects. The emphasis in standard psychometrics on trying to reject null hypotheses about effects of hypothesized internal mechanisms requires rigorous control of 'confounds', that is, environmental features that trigger activation of multiple such mechanisms. The point here is not the familiar (though fallacious) one that field experiments enjoy a standing general validity advantage over lab experiments.[6] The point is rather that if minds are devices for coordinating people with and in shared environments, then what is left to study after 'confounds' are excluded is generally an artifact that is literally constructed in the lab. Chater (2018) argues that this radical critique extends even to studies of perception.

Yarkoni (2020) argues that this isolating experimental methodology is a main source of the widely recognized 'replication crisis' in psychology (Oberauer and Lewandowsky 2019; Pashler and Wagenmakers 2012; Ortmann 2021), because there are simply too many potential confounds, interacting in too many situation-specific ways, to effectively control. In response he argues that either psychometrics should be abandoned in favor of a purely qualitative approach, or that standard experimental psychology should be given up.[7]

Both Yarkoni's diagnosis of the replication crisis and his drastic remedies for it reflect, I suggest, a general conception among psychologists that their disciplinary mission is to *isolate* the distinctive contribution of individual minds or functionally modeled nervous systems to behavior. Environmental, including social, factors are then implicitly or explicitly treated as exogenous triggers, just as in traditional behaviorism. The relevant internal mechanisms are now understood as complicated, rather than as conditioned reflexes or black-boxed 'drives'; indeed, generating inferences about these mechanisms is the primary objective. But the history of psychological research methodology displays general continuity from the days of James, Mach, Thorndike, and Watson to contemporary experimental researchers. This continuity is expressed in the fable.

Here, then, is a summary of the fable. The natural world includes systems with brains. These organs are control units for the bodies in which they are installed, operating under physical constraints. There are generalizations about the nature of this control that go beyond what can be deduced from the physical structures of brains, and also beyond what can be deduced from the basic challenges to survival

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[6] For a critical discussion of the more familiar point, and the reasons why it is unsound, see Harrison and List (2004). Experimental economists have ways of working around the criticism based on over-isolation that almost no psychologists avail themselves of; and too many behavioral economists follow the psychologists' example. I will return to these distinctions at various later points.

[7] Yarkoni's radical diagnosis ignores the possibility that psychologists could borrow quantitative methods from econometricians and try to estimate structural models of data obtained by trying to reproduce social factors across varying experimental treatment groups (Andersen et al 2010; Ross 2021a). I will consider this contrast later.

and stability that arise in their external environments. The concept of mind abstracts away from *some* physical features of brains, though it is an open empirical question just which ones can be bracketed by the science, psychology, which studies minds. At one limit, if eternal environmental contingencies largely determine what brains do, psychologists might reduce away the scientific value of the concept of mind. At the other limit, if the mental products of brains can neutralize environmental threats and manage environmental resources with sufficient flexibility, minds can display levels of complexity that far exceed the physical complexity of the brains that produce them, and psychology can proceed autonomously from brain science. At the current moment, theory and method are trending toward the first one. To accumulate progress, psychologists must operate an empirically and theoretically motivated distinction between endogenous (= mental) and exogenous (= environmental, including extra-mental somatic) sources of behavioral causation. The general boundary between endogenous and exogenous causal factors will naturally shift over time with empirical discoveries and theoretical insights. But any given psychological research project will begin from a well-theorized set of priors about the boundary. "Well-theorized" need not imply that the boundary isn't fuzzy or porous, but merely that that theoretical concepts used for measurement are clearly anchored on one side of it or the other.

3. A Fable of Sociology

If our fable of psychology finds continuity even through an alleged paradigm shift (from behaviorism to cognitive science), the best-known wide-angle views of sociology describe the opposite: failure to achieve a single paradigm due to competing visions among the recognized founders, which has led to ever-increasing fragmentation (Collins 1994; Horowitz 1994). Collins worries that the absence of unifying theory has led policy-makers to ignore sociologists, especially by contrast with an economics discipline that has been successful at maintaining a salient, even if dynamically labile, distinction between mainstream and heterodox theory.[8] Some survey authors, sharing the view that theoretical cacophony undermines sociologists' claims to be engaged in social science, respond by identifying traitors within their ranks: Bruce (1999, pp. 83-100) calls advocates of strong relativism and ideological advocacy-driven theory "imposters" of sociology.

The deep historical source of facture in sociology, between Durkheim's 'positivism' and Weber's 'anti-positivism', is sometimes said to find contemporary expression in competing valorizations of quantitative and qualitative research. These have rival geographical hubs, with quantitative work dominating the profession in the US and qualitative studies being symmetrically pre-eminent in Europe, particularly the UK. Such methodological variation, however, is sufficiently obviously (especially to sociologists!) a response to varying institutional settings that few contemporary

[8] As many have pointed out, this is almost certainly a case of bidirectional causation: the direct and indirect rewards that economists earn from commissioned policy work incentivizes them to aggressively maintain clear borders.

sociologists seriously endorse the quantitative and qualitative traditions as normative rivals. In the current climate, almost all rhetoric supports 'mixed methods' (Pearce 2012).

The more intractable source of theoretical fracture in sociology arises from the fact that the discipline hosts *both* an ontological perspective in which individual agents are pushed into and out of social roles – these days, usually represented as nodes in network models – by social forces over which they exert little or no control, *and* an ontological perspective in which attention is paid to maximally thick descriptions of subjectivity, described by Cassell (1993) as "a baffling variety of competing perspectives: phenomenologically inspired approaches, critical theory, ethnomethodology, symbolic interactionism, structuralism, post-structuralism, and theories written in the tradition of hermeneutics and ordinary language philosophy". There isn't a direct contradiction between thinking that people are powerless as individuals despite being accurately characterisable only by accounts that are sensitive to all the nuances of their special experiences. That is, thick descriptions of subjectivity do not necessarily imply effective subjective *agency*. To take a simple example: a White American male who resentfully feels excluded from powerful institutions might express this by singing country-and-western songs about drinking on the job and seducing the boss's wife, and if this turns out to be relatively widespread a sociologist borrowing methods from anthropology might drill into the pattern. However, the goal of inquiry is the opposite of the economist's usual one, that is, trying to measure the marginal effects of choices on frequencies of equilibria. This is what agency amounts to in economics. Sociologists trying to explain structural patterns and changes have more in common with the economist here than with their hermeneutically focused disciplinary colleagues. The working ontologies are logically compatible, but that is merely the minimal requirement for integration. The ontologies resist useful interaction in general theory, because thick description by its nature is the enemy of seeking generalizations using sparse sets of theoretical elements.

The mid-1990's hand-wringing over this seems to have largely faded away. The pragmatism now dominant in sociological methodology emphasizes a relaxed attitude to theoretical unification. There is not much evident inter-penetration of the literatures of mainstream philosophy of science and sociological methodology. But if we pull them together, we can construct a plausible fable centered on a shared retreat from ontological parsimony.

Institutionally, sociology is a microcosm of the whole set of social sciences, because any topic that interests any social scientist potentially interests sociologists. In this respect, sociological theorists who try to operate at grand scales resemble philosophers of science who aim to say things that are enlightening about science in general (e.g. Miller 1987, Kitcher 1993, Hoyningen-Huene 2013). Both literatures have converged on extremely tolerant methodological and – my particular point of emphasis – ontological pluralism. The logical positivists and logical empiricists had some good reasons for believing that all of science might find unifying foundations in physics, mathematical logic, and set theory, but the empire of science has developed in the opposite direction to these reductionistic ambitions. Ladyman and Ross (2007) argue that a naturalistic general metaphysics is still possible, but entails what they call "rainforest realism" (as contrasted with Quine's [1969] "desert landscape" ontological ideal) in which reality is understood as a scale-relative kaleidoscope of "real patterns" (Dennett 1991) that aren't stacked in "levels".[9] An associated ontological slogan urged by Ladyman and Ross (2012) is "the world is the totality of non-redundant statistics", accompanied by an argument that such non-redundancy is not correlated with distance from fundamental physics.[10] Thus we should not expect to find general structural models that apply across the sciences on the basis of shared ontological structure. Such general cross-disciplinary model isomporphism as we find is usually attributable to shared mathematical resources. Consequently, theoretical unity is particularly unlikely in less quantitative sciences.

On this picture, the ambition of sociologists, considered as a disciplinary tribe, to be able to shed at least some light on any large-scale social patterns, is systematically traded off against the possibility of a single general theoretical style. Some sociologists can usefully borrow modeling constraints and principles from economics and game theory, as promoted in the magnum opus of James Coleman (1990). But Coleman's ambition to supply "foundations" for social theory on the basis of such borrowing was naïve (Martin 2009), at least if by "economics and game theory" we mean to refer, as Coleman did, to a modeling program that takes utility functions of individual agents, ontologically associated with actual individual people, as basic building blocks.

The closest thing to a general, core modeling technology for sociology is network theory. This naturally accommodates most sociologists' conception of social structure and dynamics as residing in relationships between institutional roles,

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[9] For non-philosophers: the basic contrast here is between a model of reality according to which 'higher-level' processes and objects decompose, across contexts, into smaller sets of 'lower-level' ones, and a model of reality that denies that such structure is to be expected, though it might be found in rare cases.

[10] Again, for the non-philosopher: if complex processes and objects generally decompose into simpler ones, then higher-level, more stochastic, processes should generally be explicable by lower-level models of less stochastic, more fundamental processes. Then the advance of knowledge would consist in making the higher-level, less general, models redundant with respect to optimally efficient prediction and explanation. If the decompositional picture is generally false, then many 'high-level' processes are irreducibly complex and sensitive to stochastically varying parameters. Ladyman and Ross (2007), in defending the anti-reductionist picture, emphasize that this point applies as much to non-fundamental parts of physics as to traditionally identified 'special sciences'. Furthermore, there is no compelling reason to believe that fundamental physical models, particularly the quantum statistics, express underlying deterministic structure (Ladyman and Ross 2012).

which individual people move into and out of over time and space. Thanks to the explosion of inexpensive computational power that allows large networks to be easily simulated, this tool is now exploited to a far greater degree than was possible for the architects of sociological theory. However, network theory is an elaboration of graph theory, and as Martin (2009) points out, there has been an increasing tendency for sociological models that were traditionally represented graphically to be expressed using matrix notation. The main reason for this, according to Martin, is that it allows absent social influences to be represented along with actually occurring (positive or negative) ones. In this respect, sociology moves closer to economics at a more abstract level than that emphasized by Coleman. Economic explanations have always relied heavily on the shadow of what *would* happen far from equilibrium, which accounts for the resilience of both markets and social-statistical trends.[11]

I have suggested that the main source of the remarkable theoretical pluralism in sociology is the scale-relativity of its domain. Economists face the same problem, as expressed in a history of recurrent methodological problems around aggregation (see below). The most salient response to this in economics has been strong separation between microeconomics and macroeconomics. We might thus wonder why sociology is not similarly cloven into Goffman-style 'microsociology' and Marxist-inspired 'macrosociology'. The fact that we can easily associate iconic theorists with this distinction indicates that it in fact *is* present to some extent. But in the fable I am telling, one of the strongest commitments shared by most sociologists is to the idea that social roles are stabilized and reinforced by distinctive subjective perceptions and symbolic tokens of prestige. Thus the continuous interaction between micro- and macro-scale phenomena is basic to understanding social structures and dynamics.

The key feature of the sociological world-view, then, is the *combination* of emphasis on subjectivity and de-emphasis on the causal significance of individual agency. Consider Marx's model of the dynamics of revolution: it is delayed by workers' false consciousness – so, by the specific nature of their subjectivity – and the only mechanism that overcomes this are alleged economic "contradictions" that cause the capitalists to finally go bankrupt. The workers do not make their own revolution. Economists before the rise of behavioral economics handled this tension in social explanation by promoting its exact mirror image: avoidance of subjectivity and overwhelming emphasis on individual agency. But whereas sociologists ultimately settled on an attitude of 'stop worrying and love the tension', increasingly many economists have aimed to dissolve it. A prominent strategy for this has been to embrace the fable of psychology. I will argue that this is not a promising choice.

4. A Fable of Economics

[11] It should be noted that extensive-form games *do* include (all) off-equilibrium paths through game trees, which are directed graphs. Martin's explanation invites critical scrutiny.

I presented a fable of psychology in which an apparent, and rhetorically celebrated, paradigm shift from behaviorism conceals deep methodological continuity. My fable of sociology describes a discipline in which theoretical fracture was initially perceived as a crisis and is now welcomed as a strength – but which still unites around intuitions about social causation that do not comfortably make space for individual agency. My fable of economics is the narrative inverse of the fable of psychology: in economics there was a true intellectual revolution, brought about by the massive importation of game theory from the 1980s onwards, the extent of which tends to be underplayed in economists' stories of their discipline's evolution.

Whereas sociologists finessed the problem of aggregation in social science by ignoring individual agency, my fable of economics is a story of recurrent confrontation with that problem. This has been somewhat obscured by the strong division of labor between microeconomists and macroeconomists. But great economists who have been most influential in the wider empire of social science, for example Adam Smith, Veblen, J.M Keynes, Schumpeter, and Hirschman took large-scale social phenomena as their explicit explicanda and inferred hypotheses about individual incentives that would at least be compatible with the policies they recommended, and which are empirically evaluable.[12] Some equally influential names are associated with evasion: Walras theoretically refined Smith's ideas about market efficiency using incredible idealizations about micro-scale information processing and indifference to long-run profitability by firms; and Samuelson (1947) produced a 'synthesis' of marginalist microeconomics and Keynesian macro policy that amounted to simply stapling them together, since the elements of Keynes's micro-scale story (liquidity preference and animal spirits) had been removed.[13] But economic theorists have repeatedly torn off these patches.

Standard histories of economic theory in the second half of the twentieth century rightly frame two developments as pivotal.

First, proof of the existence of general welfare equilibrium under conditions of perfect competition by Arrow and Debreu (1954) provided what has often been interpreted as a theoretical foundation for policy analysis. In principle – though in light of limitations swiftly exposed by Lipsey and Lancaster (1956), *only* 'in principle' – general equilibrium (GE) theory can be used to identify efficiency targets for policy planners. This has encouraged some philosophers of economics, notably Hausman (1992), to interpret GE as the theoretical core of contemporary

[12] Smith and Wilson (2019) transform Adam Smith's micro-theory into experimental protocols. Shiller (2019) proposes bibliometric investigations of Keynesian liquidity preference and animal spirits.

[13] The absence of Arrow from this monument park of economists is deliberate. Fable-spinning should respect some limits. One of them is not to represent theorists like Arrow, who consistently resisted complacency about what had and hadn't actually been settled, as contributors to simplification. I regard Arrow as the greatest economist of the twentieth century, by a substantial margin.

economics. It is important to distinguish between static GE as an idealization of efficiency, and computable general equilibrium (CGE) models of market adjustment, which are all about respecting the empirical fact that specific markets are not isolated from one another. CGE is and will remain useful for modeling large-scale policy problems such as arise in macroeconomics, public finance, and international trade. However, Hausman focuses on static GE as a putative foundation for general economic conceptions of efficiency. This encourages the idea that economists can only give policy advice by using perfect competition models as benchmarks. But this idea is entirely false where the vast majority of contemporary work in applied microeconomics is concerned.[14] There, the game theory revolution of the 1980s led theory in essentially the opposite direction.

Actual businesses of course do not operate in environments approximating perfect competition, and would not want to. Economists relying on GE, or Walrasian foundations more generally, thus remove themselves from Marshall's (1890) project of providing guidance for "the ordinary business of life". Prior to the game-theoretic remaking of industrial organization (IO) theory in the 1980s, where many areas of business strategy were concerned microeconomists could offer advice only to monopolists and to producers of commodities with no market power; but the majority of large firms are either oligopolists or trying to become oligopolists. Game theory allowed economists to study equilibria in specific markets, and furnished insights in diverse areas of commercial operations including strategic pricing, mergers and acquisitions, supply chain design and management, employment and human capital strategy, corporate governance, process and product innovation, intellectual property development and protection, auctions, and internationalization. The fingerprints of game-theoretic microeconomists are everywhere in the 21st-century capitalist infrastructure. By any reasonable measure they have been far more successful and influential than their macroeconomist colleagues, though much less visible to the general public.

The relative preponderance and prestige of game-theoretic microeconomics in the profession as a whole has, I suggest, contributed at least as much as any direct methodological (let alone philosophical) reflection by economists to the majority conviction that all economic models should ultimately be compatible with incentive-compatible vectors of choices by individual agents. These can be micro-scale aggregates of people, such as firms and households. They can be allowed to be boundedly rational in specific ways (i.e., fail to respect some axioms of Savage [1954]), or can have less than fully rational expectations (i.e., fail to attend to some relevant information that is costly to acquire or process). But they must be incentive-sensitive decision makers. This is why, for example, post-Keynesian macroeconomics is institutionally treated as 'heterodox', even though it is directly descended from work by economists, such as Joan Robinson and James

[14] In fairness to Hausman, this was much less obvious at the time his book was published than it is now.

Duesenberry, who were unequivocally members of the club of leading theorists in their time.

In light of this recent history, it is fair, at least within the liberal tolerance of fable-making, to attribute a specific meta-theoretical inconsistency to economists as a group.

On the one hand, the prevailing culture since at least the 1950s has been anti-philosophical. Trying to explain this involves a constellation of factors. Blaug (2002) is not entirely off-target in accusing economists of paying lip service to Popperian falsificationism while (rightly, in my view) failing to practice it. But a deeper reason is that philosophers have tended to under-appreciate the divergence of objectives between the normative decision theory they explore and the microeconomic theory of choice under uncertainty that grew from shared axiomatic foundations (Ross 2021b). Economists consequently tend to read philosophers, with substantial justice, as typically not understanding them.[15] This has prompted them to walk away from conversation.

On the other hand, many leading economists have, at least in casual rhetoric, defended methodological individualism on the basis of ontological atomism (Udehn 2001),[16] which is a very strong, and dubious, metaphysical doctrine (Kincaid 1997; Ladyman and Ross 2007).[17] An economist who endorses it thereby goes much further than inserting a mere toe into deep philosophical waters.

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[15] The main history of refinements to utility theory has been driven not by philosophical preoccupations with relationships between utility maximization and rational choice, but with making the theory flexible enough to accommodate heterogeneous marginal elasticities of substitution amongst different baskets of commodities; see Chung (1994).

[16] By no means has this *all* been casual rhetoric. For example, Stigler (1980) is explicit and insistent.

[17] See notes 9 and 10. Atomism is the hypothesis that reality is ordered into levels, that higher-level processes and entities decompose additively into lower-level ones, that properties of higher-level systems are explicable in terms of properties of lower-level systems, and (in some formulations) that all causal relations among aggregates are in principle reducible to causal relations among their constituent atoms. Atomists typically maintain that we maintain special sciences only because we lack the epistemic capacity to model everything at the scale of fundamental physics. Some atomists are therefore instrumentalists about higher-level entities, viewing them as convenient fictions rather than genuine elements of reality. Economists are not generally professionally interested in the metaphysical aspects of atomism, but many casually transfer the general picture to the relationship between social groups and individual people. Margaret Thatcher expressed this kind of atomism in her famous claim that society doesn't exist. Having cited that well-worn example, it is worth adding that Thatcher's favorite source of intellectual inspiration, Hayek, was not an atomist in any sense and indeed produced original

There is work to be done by historians of economics, which to my knowledge none have yet directly taken up, in weighing the influence of four roots of this collective schizophrenia:

(1) The dominant tradition in Western metaphysics is atomistic, and economists who are not interested in critical philosophy culturally inherit it without much reflection.
(2) Most mainstream economists are liberals, and normative individualism is a standard element of liberalism. Economists, again mainly due to impatience with and indifference toward philosophy, fail to ask themselves whether normative individualism depends on descriptive individualism as a view about social causation. I have argued elsewhere (Ross 2013, 2014) that it does not.
(3) Standard welfare economics relies on the concept of consumer sovereignty, and this concept comes under significant stress if separable preferences of individuals aren't causally privileged in explaining economic choice (Lecouteux 2021).
(4) In game-theoretic models, agents are defined by utility functions, and although one agent's utility function can implicitly take another agent's utility function as an argument, in this can only be represented as a coincidence in their rankings of outcomes.[18] Technically, then, only individuals or collective agents (e.g. firms, households) treated as perfectly fused can be 'players'.

(3) and (4) above, because they involve *explicit* issues in economic modeling, have at least been subject to some critical reflection and direct influence on theory. Economists have been open to efforts to work around the limitations stated by (3) and (4), (for example, Bernheim [2016] on [3], and Bacharach [2006] on [4]), implying that they are at least recognized as limitations. Anti-philosophy, generating blind spots, is a more natural cause of philosophical dogmatism, and this is the common causal pattern represented by (1) and (2).

My fable emphasizes two major consequences of unreflective atomism in the institutional ecology of economics.

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arguments against it. It does not exaggerate matters to say that atomism as described here is an extinct view in philosophy of science, though some metaphysicians who don't attend to science continue to endorse it. One of the more influential ones, Merricks (2001), holds that there are two kinds of atoms: whatever (imaginary) physicists declare to be the smallest indivisible things, plus people. This may sound like lunacy to non-philosophers. I think it is indeed a ridiculous doctrine, though based on complicated arguments.

[18] As I will discuss in Section 6, in Conditional Game Theory this remains true only for games in extensive form (Ross et al 2021b).

The first is the strange history of the relationship between macroeconomics and microeconomics. It became an institutionally entrenched fundamental division in the profession – that is, the basis for career-determining binary choice for every economics graduate student – only in the 1950s, *after* Samuelson had pronounced a synthesis. Then, because microeconomic theory was supplied with stable axiomatic theory through the work of Samuelson, Houthakker (1950), von Neumann and Morgenstern (1944), and Savage (1954), while macroeconomic 'foundations' were merely reasoning heuristics such as the IS-LM model, the status of microeconomics as foundational could be motivated technically, without any need to explicitly invoke philosophical atomism. This in turn exposed the brief 'Keynesian' (really Hicksian) consensus in macroeconomics to the critique of Lucas (1976) that it lacked consistent 'microfoundations'. Officially, the microfoundations of post-Lucas macroeconomics (Kydland and Prescott 1982; Long and Plosser 1983) were not ontological, but practical, based on the reasonable idea that sustainable macro policy could not rely on assumptions that individuals would indefinitely fail to notice that that they were expected to ignore opportunities to optimize their welfare.[19] Philosophers have frequently criticized the leading microfoundational construct – the optimizing infinitely-lived representative agent – on the basis of its obvious ontological absurdity. This has reinforced economists' perceptions that philosophers do not understand the point of what they are doing, and these perceptions are often correct. But philosophers also accurately detect some disingenuousness in the economists' response, because their rhetoric often reveals intuitions that economies decompose into individual agents, and this certainly shores up commitment to microfoundations.

There is, and always has been, widespread awareness among economists of the conceptual problems in post-Lucas macroeconomic theory. Many microeconomists regard macroeconomics in general as unscientific. I have already described a main driver of this attitude: microeconomists have a far more defensible track record of policy success than macroeconomists. However, another source of *recent* complacency among microeconomists stems from a second expression of their naïve atomism: increasingly many think that importing concepts from psychology and combining these with more sophisticated statistical estimation is the path to achieving a tight grip on the real causes of economic behavior (Chetty 2015, Thaler 2015, Dhami 2017).

As Ortmann (2021) argues, one might reasonably think that economists would be very cautious about importing concepts and methods from a discipline that is in the grip of an existential crisis over reproducibility of empirical results (Yarkoni 2020).

Some economists no doubt believe, at least off the record, that they can avoid psychologists' problems by conducting the psychology of valuation and choice more

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[19] The key evasion in macroeconomic theory is around 'indefinitely'. The reasonable Lucas critique has usually been 'idealized' as the *un*reasonable assumption that optimal strategies are discovered instantaneously.

rigorously than psychologists usually do. I think it is true that economists studying psychological phenomena gain considerably improved leverage through commitment to incentivizing experimental subjects. However, the convenience of recruiting subjects through sources such as MTurk leads increasingly many economists to achieve incentivization in ways that undermine good control of sample selection bias, because they do not include incentive variations as treatments (Harrison et al 2009; Harrison et al 2019). Furthermore, many experimenters pay lip service to incentivization by using tiny stakes in expectation. But my main concern, addressed at length in Ross (2014), is not with economists' interest in addressing topics traditionally regarded as lying within psychologists' domain. It is, rather, a tendency of many behavioral economists' experimental designs to import, due to their philosophical blind spot about atomism, the fable of psychology as I sketched it earlier. That is, the majority of work in behavioral economics assumes that choices of individual people isolated from 'social confounds' are at least good approximate models of economic behavior *in situ*. As I will discuss later, however, there are also methodological currents in experimental economics that have closer affinity to the fable of sociology.

5. Economic methodologies at turning points

I refer to 'methodologies' and 'turning points', in plural form, to reflect the fact that microeconomists and macroeconomists inherit very different situations and confront different choices. Though contingencies in the history of thought played a dominant role in the initial separation of the sub-disciplines, it is increasingly reinforced now by the emphasis on experimentation in microeconomics. Macroeconomists can sometimes employ similar logic of model assessment with regard to natural experiments. Even there, however, scope is limited by the fact that there are only 200 sovereign policy units in the world, and only a handful of them are large enough to have any significant discretion when most economies are open. The methodology of cross-country regressions is largely a failure (Kincaid 2009, 2021) because there are simply not enough independent observations to sustain it.

I will first characterize the situation in microeconomics in the terms of my three fables. As indicated above, the dominant experimental tradition, stimulated by the methodological perspective of Thaler (1992), reinforced by the prominence of cumulative prospect theory (CPT) (Tversky and Kahneman 1992, Wakker 2010), and Camerer's (2003) magisterial consolidation of 'behavioral game theory', has been to borrow experimental design strategies from psychology, with special improvements. The improvements in question are real and important: use of incentives, as indicated above, but also application of structural theory to support quantitative identification of variables and parameters. This has sometimes allowed models to be decisively tested and rejected: for example, Harrison and Swarthout (2022) demonstrate that CPT is empirically inferior to some specifications of Rank Dependent Utility Theory (Quiggin 1982, 1993) that drop the special CPT loss-aversion parameter by rejecting sign-dependent preferences; and see Bourgeois-Gironde (2020) for other examples and discussion. Close alignment between theoretically constrained identification, interacting with prior consideration of specific econometric tests to guide experimental designs, is the best strategy for mitigating replication failure (Harrison et al 2015; Harrison 2019) – a far superior approach to the blunt instrument of hypothesis pre-registration, a cure at least as bad as the disease because of its tacit acceptance of the logic of the *t*-test, combined with wasteful commitment to ignoring potentially interesting data (Deaton 2020).

Tightening the grip of theory for filtering observations, however, offers limited scope for general epistemic improvement if the theory itself is based on ontological blind spots. This risk is *greater* the more structure is built into theoretical specification, so anyone who supports Harrison's strategy, as I do, has enhanced motivation to be attentive to background ontology. Here is where implicit adherence by economists to the fable of psychology can have pernicious effects. According to that fable, the psychologist's job is to try to isolate the contributions of individuals' cognitive processing mechanisms and capacities to behavioral patterns. Let us accept that that is a job someone should do, even if it requires idealizing away much of the deep entanglement, particularly in humans, between individual cognition and social scaffolding. This job is *not* continuous with gaining best purchase on the core intellectual challenges of economics or, especially, on the kinds of practical problems that motivate this policy science (Sutton 2000; Leamer 2014; Colander and Su 2018; Ross and Townshend 2021).

Binmore (see especially 2007b) has drawn persistent attention over the years to the problem at issue. It is nicely illustrated by Plott and Zeiler's (2003) well-known review of the so-called 'endowment effect' that looms as big as the sun in popular manifestos by behavioral economists gormlessly calling for the overthrow of establishment 'neoclassicism' (e.g. Ariely 2008). The 'effect', Plott and Zeiler found, appears and disappears, in ways that are rationalizable by standard exercises in economic modeling, across experimental contexts. The populist rebels also give pride of place to hyperbolic discounting of future rewards, which supposedly applies as a default tendency across incentive domains. But, if they are to be accurate, discount rates and functional forms need to be estimated under control for risk preferences (Andersen et al 2014), and mountains of accumulated evidence show that human risk preferences are not constant across (for example) choices over money, health-related contingencies, and food prospects.[20]

The currently most intense topic of research in behavioral economics is the cluster of behaviors influenced by 'norms' or 'social preferences' (Truc 2021). Here the contrast between approaches that reflect the fable of psychology and approaches that do not is sharp and explicitly debated in critical literature (Binmore 2010; Kimbrough and Vostroknutov 2016, 2020a, 2020b; Ross et al 2021; Botelho et al

[20] Ainslie (1992, 2001), the theorist who most strongly promotes the hypothesis that hyperbolic discounting is a basic Darwinian heritage in all animals, has always recognized that people tend to discount monetary outcomes exponentially. See also Andersen et al (2014).

2021). A dominant thread of literature (e.g. Fehr and Gächter 2000, 2002; Gintis et al 2005) models norms as strictly emergent from individuals' preferences over distributional social properties such as equality and frequency of reciprocated public contributions. This encourages a psychological style of experimental design, in which researchers set participants in games featuring equilibria that vary in these distributional properties, and seek to identify individual characteristics that predict preferences over them – so-called social preferences. An alternative approach models norms as conventions that societies inherit from their own history, with associated costs and benefits contingent on the extent to which individuals or networked sub-communities choose to conform to them in interactions (Kuran 1995; Binmore 2005; Kessler and Leider 2012; Krupka and Weber 2013; Kimbrough and Vostroknutov 2016, 2020a, 2020b; Ross et al 2021). Experimental paradigms are only beginning to emerge in this more recently active research program. Some authors collect evidence about subjects' perceptions of prevailing norms prior to embedding them in games where these expectations condition expected payoffs (e.g. Kimbrough and Vostroknutov 2016; Smith and Wilson 2019). Another approach presents subjects with choices to join communities governed by varying norms, entrenched in rules as 'institutions' (e.g. Botelho et al 2021). These design strategies are sociological in flavor, in that they implement norms as exogenous 'social facts' (Gilbert 1989).[21]

Macroeconomic theorists confront a similar methodological turning point. The years since the post-2008 global financial crisis have featured mounting dissatisfaction with the dominant modeling approach based on Lucasian microfoundations, dynamic stochastic general equilibrium (DSGE) (Romer 2016). It would not be accurate to claim that this dissatisfaction is with microfoundations themselves; it is rather with the treatment in DSGE models of technology as purely exogenous and money as a veil. However, post-Keynesians are making a concerted effort to reinsert themselves into the mainstream conversation (Taylor 2010), and the defensive barriers consigning them to heterodox status lack evident good reasons if loss of faith in DSGE as a complete model of the real economy[22] spreads doubt that macroeconomists should expect to maintain any single core model (Lehtinen 2021). Leamer (2009, 2021), who no economist would regard as an outsider, directly argues that evidence-based macroeconomic policy guidance is best informed by causal stories disciplined by graphical expression of mechanisms and identification of quantitative tests. Where methodology is concerned, he advocates pragmatic

[21] One influential norm theorist, Cristina Bicchieri (2006, 2017) equivocates between the two conceptions, in the sense that her theory is closer to the fable of sociology, but her usual experimental practice follows the social preferences approach (Ross et al 2021a).

[22] DSGE models coupled with 'bolted on' models of the financial sector likely have a secure continuing future as abstract frameworks for the specific policy choices that confront central banks. But these cease to be plausibly regarded as general models of economies as limitations on the controlling reach of monetary policy are increasingly acknowledged.

empiricism combined with rigorously critical statistics, but promotes no core theory or microfoundational restrictions. His general attitude would be familiar to contemporary sociologists.

Increasing recognition by microeconomists and macroeconomists that they face choices that hinge on relationships between decomposed and aggregated scales of analysis might portend the opening of economists' historical blind spot around the importance of ontological issues in theory specification. The suggestion here is not that economists as a group are likely to 'decide' that atomistic individualism should be set aside or, in the case of macroeconomists generally, that microfoundations are irrelevant. The prospect is rather that economists might come to resemble sociologists in recognizing explicitly that (a) good theory makes ontologies explicit, and this is a relevant dimension in motivating their conditions of application; and that (b) both traditional thoughtless individualism and insouciant emergentism are strong philosophical assumptions, so methodological pragmatism is not well served by philosophical know-nothingism.

This emerging affinity between the philosophical attitudes of economists and sociologists might be regarded as too abstract and diffuse to be very interesting. However, it finds expression in shifts of emphasis in the distribution of research effort. In his bibliometric analysis of behavioral economics, Truc (2021) finds large declines since the 1980s in publications by behavioral economists in psychology journals, and in citations of psychology journal publications by behavioral economists. Publications by behavioral economists in other social sciences and humanities (SSH) journals, and citations of articles in such journals by economists, have risen during that period, from a low base, with a sharp very recent uptick. At the same time, the interdisciplinarity of economics as a whole, as proxied by cross-disciplinary publication and citation, has increased. Finally, Truc finds that social preferences have risen recently to become the most studied topic in behavioral economics. Most behavioral economists writing on this topic have published their work in economics journals. But of those who have published in journals from other disciplines, very few have gone to psychology journals. Indeed, this dominant growth area in behavioral economics is the only major topic cluster in which economists publish in and cite more articles in other SSH journals than in psychology journals.

Truc does not speculate on underlying intellectual shifts that might jointly explain these changing publication and citation patterns. But they are consistent with the hypothesis that as behavioral economics becomes better integrated into economics as a whole, this increases the interdisciplinarity of the overall profession. If behavioral economics is a vanguard in this sense, then we should expect to see lags in cross-disciplinary citation patterns, since economists who are not behavioral economists tend to cite older behavioral research than behavioral economists do. Thus the shift away from psychology publications and citations, and toward other SSH journals, by behavioral economists predicts a similar lagged effect for economics in general.

If this hypothesis is borne out by additional data and future trends, then it will be correct to summarize the interdisciplinary ecology of economics by saying that although early behavioral economists generated a significant beachhead for cross-disciplinary influence between economics and psychology, subsequent decades have seen relative shrinkage of that beachhead at the same time as economists have followed their behavioral economist colleagues into reduced disciplinary insularity.

6. Conclusion: A Fable of the Future Marriage of Economics and Sociology

Two main mechanisms may carry economists away from psychology, at least as it is characterized in my fable, and toward increasing affinity with sociology.

First, as argued in Ross (2014), many behavioral (and other experimental) economists handle what psychologists regard as confounding variables in a way that is crucially different from standard practice in psychology. According to my fable, psychologists are preoccupied with trying to *isolate* the influence of essentially 'inboard' information-processing mechanisms and processes. Consequently, when they identify possible confounding influences from external environments, psychologists look for ways to wall them out of the lab. As documented by Cartwright (1983, 1989) in application to high-energy physics, this is a sensible strategy for isolation of causal effects of mechanisms that transport with relative internal integrity across settings. Cartwright describes this as 'shielding' the lab from the world. By contrast, the increasingly dominant norm in behavioral economics is to respond to hypothesized 'confounds' by creating treatment groups of subjects, each of which is exposed to a different confound in a measurable way. This strategy amounts to bringing more of the world into the lab (Harrison and List 2004). When it is adopted in a context where researchers estimate structural models, reference to 'confounds' becomes misleading, because these influences must be integrated into the models, with parameters for relative effect magnitudes. Methodological difference thus flows upward into theoretical difference in a profound way. This then flows back down to influence another aspect of practice: divergences between psychometric and econometric toolkits are amplified, to accommodate the differences in theoretical forms. This is Harrison's three-part alignment (see above) in action.

Second, economists are beginning to try to build explicit bridges between economic and sociological theory. I do *not* refer here to campaigns by heterodox economists to describe effects of symbolic rituals and thick identities on patterns of material production and consumption. Such work has a long track record, including some moments of brilliance such as Simmel (1900/1978). But mainstream economists have ignored it and will almost certainly continue to do so (though see below for an important qualification of this point). I refer, rather, to efforts by economists to use network theory to integrate asymmetric social power and institutionalized roles into the economist's primary working technologies of utility and production functions and non-cooperative games.

Ioannides (2013) is a particularly ambitious and pioneering theoretical review. A major technical barrier to be surmounted is the need for preservation of agents' utility functions across extensive-form games, which conflicts directly with the fundamental sociological conception of people as temporary occupants of social roles that specify and constrain their action sets (Coleman 1990). The conditional game theory (CGT) of Stirling (2012, 2016) reconciles strategically labile utility with standard non-cooperative game solution concepts by exploiting constraints from the axioms of probability theory and Markov processes. However, this accomplishment is limited to the severely restricted space of ordinal preference rankings. An exactly complementary approach, with the correspondingly reversed limitation, is Ross's (2006) overlapping-generations model of his previously developed (Ross 2005) concept of 'game determination',[23] that is, strategic interaction involving some pre-socialized agents who produce socialized 'descendant' players. This model is fully flexible with respect to utility specification, but gains this at the price of several limitations: there is no mechanism for coalition formation or in-group / out-group dynamics, mindshaping is at best implicit, and the parameters that fix n-generation players' interest in the welfare of their $n + 1$ and $n + 2$ –generations descendants (in the model's 3-generation cycles) are exogenously chosen by the modeler. Ross, Stirling, and Tummolini (RST) (2021b) preserve the concept of game determination but model it using an enriched version of Stirling's CGT that places no special restrictions on utility functions beyond conformity with rank-dependent utility theory (Quiggin 1982, 1993). RST frame their model as capturing long-run "sociological" processes that are formal duals of shorter-run "economic" interactions among players with fixed utility functions, and they demonstrate this duality using simulations of the public goods games used in experiments reported by Kimbrough and Vostroknutov (2016). RST's work thus amounts to a proposed partial theory of the relationship between sociological and microeconomic models.

I noted above that economists are unlikely to be influenced by heterodox economists who seek to transform economic theory using sociological concepts. However, these writers might charitably be interpreted as philosophical commentators on the more empirically grounded research program of economic sociology (Swedberg 2003; Smelser and Swedberg 2005; Granovetter 2017). Bridge-building modeling such as that of Ioannides and RST may help economists to more efficiently mine the rich economic sociology literature for both empirical phenomena and proposed causal mechanisms to incorporate into economic theory. I regard this as the most promising pathway by which sociology and economics may draw progressively closer together, a development I have argued to be both desirable and probable.

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[23] Ross develops the concept from informal suggestions in Binmore (1994, 1998).

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