

## What can economics contribute to the study of human evolution?

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Abstract

The revised edition of Paul Seabright's *The Company of Strangers* is critically reviewed. Seabright aims to help non-economists participating in the cross-disciplinary study of the evolution of human sociality appreciate the potential value that can be added by economists. Though the book includes nicely constructed and vivid essays on a range of economic topics, in its main ambition it largely falls short. The most serious problem is endorsement of the so-called strong reciprocity hypothesis that has been promoted by several prominent economists, but does not pass muster with biologists.

Over the past decade or so, economists have become increasingly prominent in discussions of human evolution. This has had three main sources. First, the proliferation of evolutionary game theory has provided a formal modeling tool. Second, institutional economists have earned much more sympathetic regard from their mainstream colleagues than they have enjoyed at any time since before the First World War, and this coincides with a widespread acknowledgement that among the most economically influential institutions are unofficial social norms that develop by unintended cultural evolution. Third, many behavioral economists who promote a more socialized view of human motivations than the stance they associate with establishment 20<sup>th</sup>-century microeconomics found an intellectual alliance with evolutionary psychology to be useful to their cause, since the promoters of evolutionary psychology have been successful in persuading most scholars that debates over basic structures of human nature turn importantly on facts about the past environmental pressures and genetic constraints that selected it. The most aggressive single promoter of all three of these strands has been Herbert Gintis, whose 2009 book calls for a fresh "unification" of the social and behavioral sciences by evolutionary game theory, to be jointly deployed by a tandem of behavioral economists, evolutionary psychologists and comparative and historical anthropologists. Textbooks for economics students (Schmid 2004; Bowles 2004; Bowles et al 2005; Frank 2009) have appeared that include early chapters on the evolution of human social and moral norms, which serve as foundations for the introduction of institutional variables into the microeconomic and macroeconomic models developed later in the syllabi.

These developments signal a number of shifts that I expect will prove enduring in the way in which economists are coming to think about their relationships with other disciplines and about the range of phenomena to which they can contribute deeper understanding. Anthropological evidence will come to be generally recognized as important to economists. Sociology may collapse into economics altogether, as economists become comfortable with irreducible collective agency and ‘team reasoning’ (Bacharach 2006), and thereby encroach on territory they have traditionally left to sociology’s less rigorous methods. Contested relationships with psychology have always been important drivers, even when held off-stage, of the methodological opinions of economists, but in this area I suspect that the specific current consensus in the textbooks cited above will not turn out to have staying power.<sup>1</sup> On the other hand, economics will not again drift into the ahistorical unconcern with institutions that characterized it for a few decades in the previous century; and some of the most important of the relevant institutional histories indeed go back to the dawn of the human species and earlier. Thus evolutionary psychology, as a set of specific assumptions associated with classic texts by Boyd & Richerson (1985) and Barkow et al (1992), may disappear from future economics textbooks; but evolutionary theory and history will not.

In this environment, there is natural demand for surveys that are more readable and intellectually engaging than textbooks, but less parochially technical than economics monographs; that is, distillations that attract the critical attention of both economists and the specialists in neighboring disciplines with whom they increasingly engage. Paul Seabright’s 2004 book *The Company of Strangers*, subtitled *A Natural History of Economic Life*, was an early entrant into this niche. The book was generally well received and has been relatively heavily cited, though only rarely in economics journals. In 2010 a revised edition appeared. This takes into account some prominent related scholarship that has appeared since then – e.g. Clark (2007) and Boldrin & Levine (2008) – but its principal distinguishing feature is a thread of argument, emphasized in a new chapter, according to which the 2007-2008 financial crisis illustrates and is explained by the book’s “central thesis”. Given the substantial market for efforts to make sense of these traumatic recent events, cynics can readily diagnose opportunism here. But this would be uncharitable. Seabright no doubt thought the crisis was indeed the sort of phenomenon that he had in mind when he wrote about the negative consequences of what he calls “tunnel vision”, recognized that this was bound to occur to many readers, and therefore felt that, after 2008, the 2004 edition was likely to come across as *Hamlet* without the prince.

Thus the 2010 version ranges from the prehistory of urban civilization to recent international headlines. One might worry that this scale is bound to lead to superficiality and over-generalization. For the most part it doesn’t, but for a reason that implies a criticism. What Seabright has mainly produced here are engaging, insightful essays on a hodgepodge of important economic subjects – money,

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<sup>1</sup> Some of my reasons for thinking this will be indicated in the current essay.

banking, urban management, common pool resources (specifically water), the relationship between poverty and health, and mercantilism – that will strike few economists as generally controversial, but that are modern in their dress because they are lightly flavored by references to evolved human nature, and strongly influenced by a regard for institutional history that is altogether salutary. This explains why economists have neither complained about the book nor much cited it; it contains little to either challenge or enlighten them. Others will learn about economists' thinking on some socially central topics in a pleasant way, since the book is very nicely written. There is some risk that non-economists will think that Seabright has summarized for them the conventional professional wisdom on the causes of the recent crisis, when, for reasons to which I will return, he has done no such thing. Since this is the main difference between the first and second editions, I judge the former to be superior.

Nevertheless, what we have in two thirds of this book is a polished introduction to economists' style of reasoning, by means of timely topical essays, for non-economists. There is no reason not to welcome this contribution, while acknowledging its necessarily modest importance. Unfortunately, there is another third to the book, the opening chapters, in which Seabright describes the new evolutionary foundations of economics as sketched in my opening paragraph, along with the economic thread in human evolution. This effort does not reflect, or cite, the most rigorous scholarship, and will encourage entrenchment among non-economists of widespread but mistaken opinions about how economists with fresh cross-disciplinary enlightenment frame the course and significance of our species' early history. In my opinion, almost everything in these chapters sets cross-disciplinary understanding backwards rather than moving it forward.

I begin with Seabright's central theme, emphasized right through the book, on which he is not wrong but with respect to which he fails to emphasize what is distinctive about the economist's perspective. The theme in question is that humans are very different from other mammals, though not from the eusocial species, such as bees, termites and naked mole rats, in basing their ecology on elaborate division of labor. They are distinct from *all* other animals in extending this division of labour far beyond close relatives. To establish and maintain mutually beneficial relationships with unrelated strangers, people rely on rich signaling dynamics, which are stabilized by norms and institutions. Human interactions, outside of the rather minor sphere of near-competitive markets with symmetric information, are thus best modeled as non-parametric strategic situations – that is, games. This in turn implies that the interactions will tend, like all games, to produce outcomes that involve significant externalities – side-consequences relevant to the welfare of non-players – that players lack incentive to notice or try to control, and which will sometimes be beneficial to those bystanders but, in a world of entropy, will more often be harmful to them. Costs of externalities may often exceed the benefits players derive from use of the strategies that produce the externalities in question, and this is how we explain what has gone wrong when people ruin common resources by collectively over-exploiting them, or allow their productive surpluses

to be captured by rent-seekers, or annihilate their own assets in zero-sum conflicts – or participate in asset bubbles that end in wealth-destroying explosions. Dividers of labor, as Seabright puts it, are naturally prone to “tunnel vision”: with everyone specializing, and each specialist trusting all the other specialists to hold collective structures in place, no one may notice if the network as a whole lurches toward catastrophe.

These themes have become quite familiar to the contemporary well-read student of general social science. However, economists add special rigor to them in ways that Seabright doesn’t indicate. To begin with, they focus on *exchange* as the proxy for the loose and un-formalized idea of division of labor, because the former but not the latter is the basis for statistically testable models and hypotheses. Division of labor is a term of art more common among sociologists. Seabright might respond to this complaint by pointing out that division of labor, unlike exchange, draws our attention to social roles with which individuals self-identify, and he indeed spends a short chapter arguing that such role identification is an important reinforcer of tunnel vision. However, he does not explain how role-identification addresses anything more than what tunnel vision *feels like* to those in tunnels; positive and negative externalities of games, with all of their momentous economic and political consequences, follow directly from the specialization that is the essential concomitant to exchange. Thus economists see no benefit, and much cost to analytical clarity, in directly modeling division of labor. Seabright’s reader will not learn this.

The complaint might be regarded as just so much parochial disciplinary fussing. However, if Seabright’s distinctive self-assigned job is to show how economic reasoning tightens a picture that every social scientist shares, it is hard not to see blurring conceptual resolution as anything but regressive. Furthermore, this fuzzing contributes directly to substantive failures of explanation on Seabright’s part. I will concentrate on two of these: first, his confused presentation of the actual role of exchange in human evolution, and, second, his failure to show best-practice economic logic at work in promoting understanding of the recent financial crisis.

In neither his 2004 nor his 2010 editions does Seabright cite Haim Ofek’s (2001) work that explores, with much greater economic *and* paleontological rigor, the actual role of exchange as a coevolutionary factor in the descent of early *H. sapiens* from *H. erectus* and *H. habilis*. Ofek argues persuasively that specialization and exchange were a precondition rather than a consequence of this trajectory, and of the massive brain growth that was its most salient evolutionary feature. He assembles physical evidence from paleontology to build his case for this highly portentous hypothesis, but surveys it with an economist’s close attention to opportunity costs of the alternative behavioral strategies that were available to our ancestors at each key evolutionary stage. Since Seabright does not even scratch the surface of this sort of inquiry, he fails to put the economist’s distinctive analytical capacities to work in contributing to our knowledge of our origins. At least as seriously, by focusing on the importance of extended social networks only during

and after the very recent establishment of agriculture, he misses its vital role in facilitating lifestyles based on sharing yields from hunting and gathering; but as Ofek shows, this was the true breakthrough that we need economic analysis to properly understand. In choosing his foreshortened frame, Seabright lops about two million years off the history of exchange relations in human evolution – and the very two million years in which it made its most revolutionary impact until the coming of industrialization. This is a bit like writing a history of physics that begins after Newton and never mentions him.

It is worth saying more about Ofek's reconstruction of human history in order to capture some of the flavor of what Seabright misses. The reader of Seabright's book who doesn't already know economics won't learn that the analysis is based on opportunity costs, measured as a function of budget constraints given locally fixed technology for resource exploitation. Evolution itself faces such constraints. In the case of our hominid ancestors, these included the metabolic demands of the larger brain. The only comparably expensive organ that could be traded off to support it was the complex gut needed to digest raw plant food. Thus Ofek argues, in company with Wrangham *et al* (1999) and Wrangham (2009), that mastery of fire was a specific precondition for at least the later and most rapid stage of human encephalization. He then marshals reasons to believe that fire-keeping was the first specialized occupation in the hominid social ecology. This involves interpretation of paleontological evidence in light of an economic analysis according to which, for *H. erectus* and his immediate successors, it was much more efficient for specialists to maintain fires, upon which bands of local hunter-gatherers could draw in exchange for food and pelts, than for each small band of hunter-gatherers to search for suitable kindling each day – which would have severely restricted their foraging ranges – and then endure the high-risk, failure-prone ordeal of starting a nightly fire without modern ignition technology. Caves, Ofek argues, were not primarily used as homes by early humans, as popular imagination supposes, but as fire service stations. This naturally leads one to speculate, though he does not, that cave art, exploiting early humans' mimetic dispositions (which, according to Donald 1991, were already crucially in place), might have had the intended function of attracting customers. Since fire maintenance requires steady presence but not steady labor, once fire service centers were established, it would be natural for their operators to diversify into making hand axes, body ornaments and other products that would be of value to hunter-gatherers but are not most efficiently manufactured *while* one is moving around to follow prey and locate fruit and vegetable patches. The pattern of human colonization in challenging environments such as ice-age Europe, Ofek argues, was constrained by considerations of economies of scale: hunter-gatherers could not begin to productively work a new territory until there were enough of them to support a local fire station. The expansion of frontiers of settlement in modern times according to this pattern – on far faster time scales, but obeying the same economic principle – is a familiar one to economic historians.

Ofek's project is not merely to explain the origins of markets. Rather, his thesis is that market exchange was the basic behavioral adaptation that allowed humans to

construct a distinctive ecological niche, and the only such niche that tends by its own endogenous dynamic to expand indefinitely. Like Seabright, but without any need for the tendentious hypothesis of 'strong reciprocity' (see below), Ofek observes that the progress of cross-band exchange in turn required the partial displacement of natural xenophobic violence by diplomacy, thus promoting the enhanced strategic competence in which all social intelligence, including that of humans, largely consists.

Let us now review a second instance in which the limited real economics in Seabright's early chapters contributes to impoverished explanation. His account of the financial crisis blames it, as one might anticipate, on tunnel vision, in this instance that of bankers. Each senior bank executive, he explains, appreciated a responsibility to attend to his or her own bank's level of relative risk against the market, but no banker had responsibility to attend to so-called systemic risk – the possibility that all banks might simultaneously face demand for liquidity, which implies each bank's sudden insolvency.<sup>2</sup> Seabright then appeals to a fatuous<sup>3</sup> popular story propounded by two famous economists, Akerlof & Shiller (2009), to explain why the prospects of all banks wanting liquid reassurance at the same time was always greater than would be implied by the statistical analysis of hypothetically perfectly rational market participants. The relevant decision makers, according to Akerlof & Shiller, are not rational, but are locked into correlated cycles of mutually reinforcing irrational exuberance (an Alan Greenspan coinage) and irrational gloom.

Seabright's appeal to tunnel vision here is not false, but it is also not economics; it is at best a high-level metaphorical gloss that sweeps the specific economic details under the rug. *Obviously* any asset bubble is an externality; it cannot, as a matter of logic, be an intended outcome of any but a tiny minority of investors. Calling this sort of externality a result of 'tunnel vision', however, has a potentially misleading semantic connotation, for it suggests that participants were necessarily unaware of asset over-pricing. In the case of the American and European housing bubble that burst in 2006, such unawareness no doubt characterized many home-buyers, or at least the sub-prime ones. However, these buyers' actions are not Seabright's focus, probably for the defensible reason that they were responses to incentives offered by finance companies. Now, the senior managers of these companies were *not* unaware of the bubble. The primary shared sources of business news in their culture, including *The Economist*, *The Wall Street Journal* and *The Financial Times*, wondered repeatedly and with front-cover alarm for *four years* when the bubble would burst;

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<sup>2</sup> Seabright recognizes that regulators *did* have exactly this responsibility, but were discouraged by inductively induced complacency from taking it seriously. I think it is more accurate to say that they had been disempowered for ideological reasons. On neither of these stories, however, does their failure clearly stem from tunnel vision as Seabright loosely defines it.

<sup>3</sup> See Ross (2010) for the justification for this adjective.

these journalists were also nearly unanimous in fearing that correction of inflated real estate values would be of sufficient magnitude to drag the rest of the US economy into crisis (Posner 2009; Ross 2010). The problem was that the *rational* strategy for each investment bank was to profit from the bubble in hopes of jumping off early; indeed, the shareholders in large finance houses, and the directors of pension funds, would reasonably have tolerated no *other* strategy. (The handful of small funds that profited handsomely from correctly timing the bubble's implosion took enormous risks in order to do so, risks possible only for nimble minnows using the resources of very aggressive investors, as brilliantly related by Lewis [2010].) Furthermore, the link between systemic financial-sector risk and the main economy *lowered* the proprietary risk for individual banks because it made government bail-out more likely in the event that a company's timing gambit failed. This was rational ex-ante, and turned out to be generally correct ex-post; the fate of Lehman Brothers was the exception, not the rule, among the main makers of the market for derivatives based on mortgages.

Read literally, Seabright's appeal to tunnel vision is merely uninformative, even if its connotations are misleading. His appeal to Akerlof and Shiller's 'animal spirits' account is, however, outright *bad* economics. No economic logic related to asset bubbles calls for resort to emotional contagion as a causal driver, and there is no econometrically tested evidence for it. Investors in financial assets condition their estimates on their observations of the behavior of other investors. Under some circumstances, which empirical studies and experiments conducted by economists have carefully investigated, they will rationally ignore private information that contradicts their inferences from such observations (Bannerjee 1992; Anderson & Holt 1997; Hung & Plott 2001; SgROI 2003; Chamley 2004). The loss of this information in markets characterized by such 'herding' is an inefficiency, to be sure; but, contrary to many popular complaints which it is unfortunate to find Seabright abetting, only a minority of economists discount the possibility of such inefficiencies in real markets. No doubt investors suffer enervating losses of brio when they realize that their wealth is shrinking or about to shrink; but it is old-fashioned casuistry to mistake this epiphenomenon – the origins of which are mysterious even according to Akerlof & Shiller – for the main motor in the causal engine. Ironically, business cycles were originally *defined*, outside the economic literature, as waves of rising and falling market confidence. Economists, in wondering whether business cycles were 'real', answered the question in the affirmative precisely because they *did* find explanations of their dynamics that didn't simply appeal to brute psychological changes.<sup>4</sup>

I have so far concentrated on ways in which Seabright misses opportunities to demonstrate the distinctive value of economic analysis for his largely non-

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<sup>4</sup> The same comment applies to the main alternative literature in the business cycle debate, even though it postulates less rational use of information and borrows the Keynesian phrase that Akerlof & Shiller do; see Farmer & Guo (1994).

economist audience. However, the soft spot in his exposition that seems to me most serious, and also most interesting with respect to the larger state of the dialectic in the interdisciplinary study of social evolution, is his endorsement of the so-called 'strong reciprocity' hypothesis associated with the economists Ernst Fehr, Herbert Gintis, Samuel Bowles, and Colin Camerer, and the anthropologists Joseph Henrich and Robert Boyd. According to these scientists, strong reciprocity was a crucial mechanism in human evolution that was suggested by economic logic and has been confirmed by economic and neuroeconomic experiments. In fact, there is no evidence that it plays a role in human behavior outside of the very laboratory setups designed to induce it. It is outright inconsistent with modern, formal Darwinian theory, and it is contra-indicated by the relevant ethnographic literature.

Strong reciprocity is defined by contrast with weak reciprocity. The latter refers to a tendency to reward cooperation and punish defection in non-zero-sum games, which maximizes the expected inclusive fitness of the player whose strategic choices reflect the tendency. Strong reciprocity, on the other hand, is a postulated disposition to reward cooperators and punish defectors that *reduces* the strong reciprocator's fitness. Since this is incompatible with Hamilton's rule, it would be a stunning discovery indeed if it were empirically confirmed. The group of economists whose experiments have hoodwinked Seabright, and through Seabright his readers, claim all of the following: that they routinely observe strong reciprocity in their behavioral laboratories (Fehr & Gächter 2000, 2002);<sup>5</sup> that this observation is supported by ethnographic evidence (Henrich & Henrich 2007); that mathematical models and computer simulations show it to be consistent with Darwinian processes (Gintis 2000; Gintis et al 2003; Falk & Fischbacher 2005); and that fMRI work has identified its neural correlates (Sanfey et al 2003; de Quervain et al 2004).

None of these claims hold up well under careful scrutiny. It is true that people can be induced to pay monetary costs to retaliate against perceived unfairness in constrained experimental circumstances, especially where their capacity to costlessly signal is restricted (though even this strongest link in the theoretical edifice of strong reciprocity frequently rests on dubious inferences, as shown by Binmore & Shaked 2010). However, if such behavior does not violate Hamilton's rule then it simply expresses weak reciprocity plus learning lags (Burnham & Johnson 2005). The promoters of the strong reciprocity hypothesis are consistently ambiguous about whether they do or don't intend to promote the rejection of Hamilton's rule as the full generalization of Darwinian theory (Ibid; West et al 2010). As West et al explain, the general logic of the strong reciprocity lobby begins with equivocation about the entities that are taken to maximize fitness, which then motivates postulated effects designed to reconcile the hypothesis with orthodox

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<sup>5</sup> Confusingly, they like to say that they also observe it in field experiments (Henrich et al 2004); but an overt manipulation of subjects' incentives is not turned into a true field experiment merely by conducting it outdoors in an exotic country (Harrison & List 2004; Guala 2010).



evolutionary theory. All of this is outright formal confusion. The next problem, with the claims that field and ethnographic evidence confirm strong reciprocity, is most decisively exposed by Guala (2010). In a careful review of the anthropological literature, he finds that outside of the lab people enforce their social contracts with spontaneous low-cost punishments like gossiping, ostracism and moving away from offenders, or rely on third-party punishment institutions. As Guala further demonstrates, this empirical failure has been obscured by repeated citations of ethnographic counter-evidence as though it were favorable! Finally, the supposed support for strong reciprocity from neuroimaging work depends on repeated invocation of fallacious ‘reverse inference’ (Poldrack 2006; Harrison 2008), in which brain activity correlated with costly punishment is interpreted as specially adapted to subserve such punishment, in the absence of independent grounds for supposing that the brain regions in question are *specially* adapted to *any* strategic function.

In light of all this, it is most unfortunate that Seabright gives explicit pride of place to strong reciprocity as the primary special behavioral disposition that maintains cooperation among networks of human non-relatives. The evidence he cites is precisely that briefly reviewed above, which, in light of its failure to connect with any phenomena observed outside of the lab, should *not* be interpreted as showing anything significant about actual human evolution. To his credit Seabright establishes distance from the two aspects of the strong reciprocity literature that excite the most allergic responses from biologists such as West et al (2010).<sup>6</sup> First, with respect to the biologists’ concern, Seabright is careful to ground strong reciprocity in kin selection, and thereby to imply that he doesn’t take it to violate maximization of expected inclusive fitness. People, as he puts it, treat non-relatives – or, at least, relatively materially successful fellow citizens and co-religionists – as “honorary relatives”. Since this is usually a *sound* biological strategy for humans, dangers arising from “tunnel vision” notwithstanding, it is unclear why what Seabright calls “strong reciprocity” isn’t what his sources of putative evidence like to dismiss as mere boring, old-fashioned “weak reciprocity”. Second, Seabright backs away from endorsing the view that strong reciprocity is based on a *genetic* discontinuity in primate evolution that distinguishes modern humans from their ancestors and nearest living relatives; all the behavioral patterns associated with strong reciprocity, he concedes, might be based on cultural development and normative conditioning in childhood (p. 87). This leaves the reader asking the following question, to which Seabright’s text gives no answer: Does he think that humans have uniquely escaped from the constraints of Hamilton’s rule by cultural bootstrapping, or does he simply not recognize the radicalism of the strong reciprocity hypothesis as its defenders intend it?

To conclude that this question about Seabright’s view is left open by his book is to imply that its theses in both directions – i.e., concerning the impact of economic

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<sup>6</sup> For alarms on these same points from non-biologists, see Binmore (2005) and Ross (2006).

factors on human evolution, and of human biological origins on economic behavior – are fundamentally unclear. This is a rather dismal verdict on a book that purports to help non-economists interested in the evolution of human sociality get a handle on the distinctive importance of economics to this subject. They are likely to conclude instead that economists have a shaky grasp of biology and paleontology, or, worse, will acquire some muddled beliefs about all three disciplines. Let us note in mitigation, therefore, that in backing away from the *profound* errors associated with the strong reciprocity hypothesis, Seabright shows more reliable scientific judgment than the *majority* of his economist colleagues who have tried to clarify the general interdisciplinary frontiers. This is faint praise, of course. Seabright's book stands as a reminder that until such time as we see more citations of such economists as Ofek and Binmore in *all* parts of the literature on the evolution of sociality, the integration of the full power of economic reasoning into that science remains incomplete.

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