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Economics, cognitive science and social cognition

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Abstract

I discuss the role of economics in the study of social cognition. A currently popular view is that microeconomics should collapse into psychology partly because cognitive science has shown that valuation is constitutively social, whereas non-psychological economics insists that it is not. In the paper I resist this view, partly by reference to the relevant history of economic theory, and partly by reference to an alternative model of the way in which that theory complements, without reducing to, psychological accounts of social cognition.

Keywords: Economic theory; Behavioral economics; Sociality; Atomism; Aggregation

1. Introduction

This essay concerns the role of economics in the interdisciplinary study of social cognition. Increasingly many economists believe that economics has such a role. Most who hold this opinion do so because they think that, to some extent, important parts of microeconomics should collapse into psychology. They think this in part *because* they are convinced that most human motivation has turned out to be irreducibly social, whereas traditional microeconomics depended for maintenance of its distance from psychology on modeling people as if their social relations were incidental rather than constitutive. Bruni (2005) is a representative instance of the newer view.

The perspective I will defend here agrees that economics can and should contribute to the understanding of social cognition. Economics is an important part of a complementary suite of cognitive and behavioral sciences that accomplish more together than they could do in isolation. However, I do not believe that any part of economics should be collapsed into psychology, and I reject the widespread opinion that economics for much of its history 'went wrong' by ignoring the social dimension of value.

I will aim to do three things in the paper. First, I will describe the origins of the widespread misperception. As will be seen, both the cognitive revolution of the 1960s and the later interanimation of cognitive science and social theory are important parts of this story (see also Angner & Loewenstein, forthcoming). Then I will explain why I think the perception is confused. Finally I will indicate why all of this matters: economics can make its distinctive and important contribution to our understanding of social cognition (and social behavior) only if it is recognized to have a *different* role from that of psychology. Economics is not equivalent to the psychology of valuation, though there is (of course) such a psychology, which is partly social, and economics helpfully informs it. It would reduce confusion, I believe, if much of what is now called 'behavioral economics' were referred to as 'psychology of valuation' instead.

The confusions I aim to dispel are historical in origin. Thus much of the essay will be about what economists call

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'history of thought'. Let me therefore note that my *motives* are not the historian's. That is, I am not concerned *per se* with the way in which historical thinkers represented their own intentions and views to themselves. I am instead concerned with how we should critically regard previous episodes of reasoning in light of what we think we have learned that participants in these past episodes did not know. To illustrate with a simple example: the question 'Why did Copernicus set the scientific revolution in motion?' is not a question for the intellectual biographer, because Copernicus never imagined he was doing any such thing; but it is a perfectly good question for the historian of science who knows how events turned out and what led to what.

2. The supposed chastening of economics by cognitive science

It is doubtful that many economists were ever unaware, as one might suppose from reading some critics, that social relations are very important to people and to their economic activity. Nevertheless, it is noteworthy that an increasing number of economists have recently been motivated to produce models of socialization and reciprocal interpersonal preference modulation themselves,[1] instead of simply acting as consumers of such models produced by their colleagues in psychology, sociology, anthropology, ethology and neuroscience. Most of the economists in question think that this reflects a discovery that in their modeling practice they had been neglecting important considerations. Often this is expressed in tones suggesting that economists should feel chastened by discoveries coming from cognitive science generally and cognitive neuroscience particularly (Camerer, Loewenstein, & Prelec, 2005).

The discoveries in question can be broadly sorted into four sets: (1) findings that people don't reason about uncertainty in accordance with sound statistical and other inductive principles; (2) findings that people behave inconsistently from one choice problem to another as a result of various kinds of framing influences; (3) findings that people systematically reverse preferences over time because they discount the future hyperbolically instead of exponentially; and (4) findings that people don't act so as to optimize their personal expected utility, but are heavily influenced by their beliefs about the prospective utility of other people, and by relations between other peoples' utility and their own. All of these are taken to threaten the supposed 'dogma' of mainstream (typically called 'neoclassical' or 'Walrasian') economics that people are rational and self-interested. Note that the findings in sets (1)–(3) undermine assumptions about practical (means-ends) rationality, while set (4) undermines assumptions about narrow self-interest. It is thus not obvious that worries about conventional theory motivated by (1)–(3) are directly related to sociality. I will later indicate why I think that they are. That (4) is directly related to sociality will be evident.

Economists, like other scientists busy accumulating *new* knowledge, often rely for their sense of the history of their discipline on textbooks. Textbook writers tend to like to introduce their subjects by spinning whiggish narratives about their histories, thus recruiting students to purposeful, progressive projects. This understandable aim requires no bad faith, because whiggish histories are not necessarily *false.* Perfectly reasonable narratives can interpret past cul-de-sacs in theory development as valuable lessons that inspired later progress, and therefore, at the relevant level of abstraction, as not really cul-de-sacs at all. So, let us give two cheers for whiggish history. We should withhold the third cheer only because relentless whiggery makes it difficult to comparatively evaluate the extent to which earlier shifts in doctrine, methods or modeling techniques represented important discontinuities, from which we can inductively learn now. In addition, whiggish attitudes cry out to be taken down a peg for their inevitable simplifications and air of self-congratulation; and this encourages counter-narratives of renunciation and self-chastening.

The view of economics as chastened by cognitive science is in part rebellion against bewitchment by a standard whiggish narrative on postwar economic theory, which celebrates some climactic technical accomplishments that resulted from mathematicization. In 1954 Kenneth Arrow and Gerard Debreu established that, under certain restrictions, perfectly competitive economies have uniquely stable, optimally efficient equilibria. They thereby completed an idea and an argument that had animated Adam Smith, a few predecessors, and most economists after him. Formulating this 'general equilibrium problem' in terms clear enough to permit technical solution required strong general constraints on the nature of the participants in economies. In particular, it was necessary to assume that the participants could rank all possible states of the world with respect to value, and that they never changed their minds about these rankings. Exact specification of these constraints on *economic agency* also had a long history in economic thought, and Debreu, once again, triumphantly completed this arc of progress (in Debreu, 1959) by axiomatizing the required formal account of subjective valuation.

Against this background, the basis of the later humbling of economics was the 'insight' that Debrevian economic agents are not plausible models of whole people over the course of their natural lives. The truth of this can be made evident before any considerations having to do with sociality are introduced. On pain of failure of the mathematics, an economic agent cannot change its preference ranking over complete states of the world and yet remain the same agent. Yet no person's identity as an organism, *or* as a social entity defined partly in terms of obligations to particular institutions and people, is threatened by her discovery that (e.g.) she just does not like sweet salad dressing anymore. On the other hand, a person's identity *is* compromised by a discovery that she no longer likes her spouse,

[1] For examples from an exponentially growing literature see the following collections: Durlauf and Young (2001), Gintis, Bowles, Boyd, and Fehr (2005), and Gui and Sugden (2005).