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Peer Review

Review of: "The Challenge of Economics"

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The paper suggests that economics has been neglecting planning horizons—defined as "the bounded, evolving scope of agents' foresight and knowledge"—and explores the consequences of that, as well as the reasons, and through those explorations, goes on to comment on academia and the pursuit of human knowledge more generally. The paper is particularly strong, in fact inspiring and at times even enlightened, in confronting how human knowledge has been pursued by academic research. In sum, I did learn something about "planning horizons," and I did take many moments to (re)think about the point of economics and the other behavioral sciences.

I am a professor of behavioral science at Southampton, with training in the mathematical methods of decision making (operations research) and the scientific approach of psychology (judgment and decision making); I do not have training as an economist but have engaged with behavioral economics and what is often called heterodox economics, as, for example, in Simon's and Gigerenzer's conceptions of bounded rationality. I have also done my time pondering the perils of academia, especially in the last decade. Hence, I read this paper as an informed generalist, or rather a stakeholder.

In the order they appear in the text (more or less), here are the more specific parts of the paper that stood out for me, strengths and weaknesses lumped together, in the remaining paragraphs of this review.

From the get-go (abstract), I was intrigued by the suggestion that economics is not horizontal. I was aware of research on things such as reinforcement learning and Bayesian updating that appear to try and capture the dynamism of human planning—albeit by reducing all uncertainty to probabilistic risk, contra Knight—but never thought they went far enough and outside the confines of lab experiments or too-neat theories (more on that to come), and the opening stanzas of the paper gave me some reasons to understand my own thoughts more deeply. On p. 2, I was glad to see nods to heterodox economics but was subsequently disappointed that there are no references to modern reviews of bounded rationality, as, for example, in the two recent handbooks (Routledge, 2021, and Edward Elgar, 2024) edited by Viale and

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colleagues. And the topic was not developed enough anyway, although this might be normal for a paper that takes such a broad scope. Sometimes the text fell back on traditional conceptions of bounded rationality (p. 11, p. 15) as first developed by eminent figures such as Arrow and Tversky and Kahneman, who, however, arguably, strayed from Simon's original meanings that do not see bounded rationality as flawed attempts to optimize (see my 2023 Palgrave Macmillan book).

Allusions to the shortcomings of academic research became obvious from p. 4 onwards, and I enjoyed them, despite the occasional sweeping assertions. In behavioral economics, I have experienced the almighty power of prospect theory (a neat theory for the lab), among professors and students, despite the fact that it takes little work to find out that its descriptive principles and assumptions have been mostly known all along from Smith and Bernoulli to Markowitz, and its predictive prowess is very much challenged by simple alternatives such as lexicographic heuristics. Celebrated concepts such as loss aversion and probability weighting might not be necessary, or pragmatic, to predict human choices under risk (these claims are supported in a 2014 Routledge book by D. Friedman and colleagues, and in my Palgrave Macmillan book). Güth wittingly and accurately saw that behavioral economics has been dominated by mathematical models that merely aspire to "repair" the subjective expected utility program from the fifties. Apparently, however, the authority of Google Scholar citation counts and of membership in elite journal lists is difficult to resist, despite warnings from funders and assessors against outsourcing our own judgment from first principles (my own experience as associate dean of research confirmed such fears). Later in the paper (pp. 23-25), the story of the "Hicksian gateway" and "Hirshleifer rescue" (neat theories again) in a marvellous way shows the complex of formal and psychological reasons that can create resistance to obvious empirical truths; this part is truly a gem (complete with an unexpected formal turn).

The paper is strong in emphasizing that ultimately great research requires real openness, the ignoring of comfort zones, and the like. Perhaps it is too strong, as already from p. 6, I had felt I had gotten the author's commitment to this, admittedly, very important point. I enjoy aphorisms as much as the next person, of course. And the pep talk that naturally follows from these lessons is indeed inspiring, as when on p. 23 we are reminded to "dare to be true." How true itself this point is. And I am satisfied that this is a true paper, worth reading and then thinking about and beyond it. Enjoy.

Declarations

Potential competing interests: No potential competing interests to declare.