Report:
"Experimental Test of Utility Maximization"
Economics MS 758

Summary

I have had difficulties understanding the paper, so it is not easy for me to summarize it. I will explain this below, but I would already like to point out that I do not believe that this is my fault.

The paper sets out to show how the hypothesis of cardinal utility maximization can be tested, and suggests a new "psychophysical-econometric paradigm". It mentions "perception utility" and "emotion utility" in terms of Bentham's work and suggests that these concepts are somehow important in economics and need to be treated separately. While these concepts might be useful, they are not explained in a way understandable to a contemporary economist, and they are not put into the context of the latest research in behavioral economics. The paper presents results from a controlled experiment. The obtained data are used to estimate parameters and are claimed to confirm the author's theory.

Given the presentation of the results, I find it difficult to judge the potential significance of the paper.

General Comments

I have had major difficulties understanding the paper. First, the manuscript is written in rather poor English. Some sentences are incomprehensible. Second, and language issues aside, the author could have done a much better job describing what the paper actually does and what it tries to achieve. Third, the author completely ignores the innumerable papers produced by economic theorists and experimental economists in recent years and does not put the results into any context understandable to a contemporary mainstream experimentalist or theorist. The author's claims, some of which I think are wrong, even seem to indicate that the author is completely unaware of the experimental economics literature.

Should the author decide to revisit the paper, fixing the first two issues is mandatory. The author seems to be convinced that the results are important for economics. If they really are (and they might be, but I'm not sure), the paper would have benefited a lot from considering the third point, because it would have made the paper a lot more interesting and accessible to most economics. This would have increased its potential impact. I would like to stress that this is merely about accessibility and not about changing the paper's paradigm. If the author wants to establish a new paradigm, it should be explained why it is better or complementary and how it fits or does not fit into the established body of previous studies.

I do not fully understand the author's ideas about the psychophysical approach. I furthermore do not understand if the author actually estimates individual utility functions or aggregate index functions representing some sort of average preferences. It seems that the results are for average demand, which I think it not the right approach to testing the utility maximization hypothesis, especially when experimental data on individual demand is available. The model on page 5 is rather confusing; without prior knowledge of the Klein-Rubin or Stone-Geary approach and linear expenditure systems, it is very difficult to understand what is going on. It is furthermore not clear what the connection between U_{LES} and U_{Est} is. It is only a minor point that the author uses the same index, k, for a specific price and commodity and in the same equation as an index for a sum.

The experimental results presented by the author are to a large extent incomprehensible to me. I also have severe doubts about the part of the experimental design which I think I understand. Subjects are only asked to make three choices. The author does not tell us anything about the probability that, say, random choices also fit the theory. An analysis in the spirit of Bronars (1987, Econometrica) or Andreoni and Harbough (2006, "Power Indices for Revealed Preference Tests", working paper) would have helped to shed light on this question. Furthermore, while the author tries to explain "validity", I am not quite sure what is meant. The paper states that less than forty percent of the subjects "delivered valid data". As I said, I am not sure how to interpret this, but it seems to indicate that the experiment could have been designed better. I would expect at least half of the subjects to deliver valid data.

What I found bemusing in particular is the author's apparent dismissal of ordinal utility theory based on what I think are wrong claims. I do not want to conceal that my thinking is more rooted in the ordinal approach, but I do not think that this is important here; in particular, I do acknowledge that proper research by behavioral economics has shown that the cardinal approach can be useful and important (the author does not really explain the advantage of the cardinal approach). What is important is that the author claims that ordinal utility theory is not testable or falsifiable and that there is no experimental verification. If I do not completely misunderstand the author's usage of ordinal utility, this is plainly wrong. Beginning with Samuelson (1938, Economica), and extended by Houthakker (1950, Economica), Richter (1966, Econometrica; 1979, Journal of Economic Theory), Mas-Colell (1982, Samuelson and Neoclassical Economics; 1978, Review of Economic Studies), Matzkin (1991, Econometrica), Richter and Wong (1999, Journal of Mathematical Economics), Matzkin and Richter (1991, Journal of Economic Theory), Afriat (1967, International Economic Review), Varian (1982, Econometrica; 1983, Review of Economic Studies; 1988, Journal of Economic Theory), Chambers et al. (2010, Caltech Social Science 1317), and many, many others, there have been thorough theoretical papers which show how one can test the ordinal utility maximization hypothesis. In recent years, many experimental studies have tested subjects' choices for utility maximization (e.g., Sippel (1997, Economic Journal); Harbaugh and Krause (2000, Economic Inquiry); Mattei (2000, Journal of Economic Behavior & Organization); Harbaugh et al. (2001, American Economic Review); Andreoni and Miller (2002, Econometrica); Fevrier and Visser (2004, Experimental Economics); Choi et al.

(2007, American Economic Review); Fisman et al. (2007, American Economic Review); Dickinson (2009, Journal of Socio-Economics); Banerjee and Murphy (2011, Applied Economics); Dawes et al. (2011, Journal of Politics)). Given the contributions already made by these papers, it is difficult to tell what exactly the original contribution of the author is.