

Let me first thank you for taking the time to read my article. I took the time to look over the criticisms and noted and looked over the articles that were discussed in the review. As well as conducted a literature search to support adding additional sections to the review, should it be needed. While the language in the review did not seem to favor the paper, it has opened the dialog for reasonable discussion.

Since then, the literature on non-expected utility models has grown enormously. Models have been proposed, qualified, developed, and applied in several contexts. Prospect theory itself has changed substantially from the earlier formulation of the eighties (Tversky and Kahneman 1992, Wakker 2010). Several surveys are available for the literature (Starmers 2000). A first major problem with this survey is that the many developments are ignored. In fact, more strongly a reader not acquainted with the literature could be induced to believe that many developments never occurred.

1.) Yes, theories have expanded a great deal, but the focus of the paper was on the Certainty Effect and not prospect theory, utility theory or any other choice theory per se. I touched on EU and PT being as these two theories were the starting place for what would be called the Certainty Effect.

I am not clear on why it would be relevant to include cumulative prospect theory when the revised theory does not have empirical findings to support predictions or alterations to the certainty effect in regards to classical prospect theory or offer new insight into the outcomes for repeated gambles or gambles guided by feedback. If the modified version had experimental research that is research using human participants and their choices rather than a model to support predicted new outcomes, or had something else to contribute, I would agree, but being as the focus of the paper is the certainty effect and the experimental methods that have been used to test for this specific effect and not choice theory I made a judgment call not to include the theory. As you stated in your review, there already exist surveys on choice theory and if this paper were focusing on choice theory, it would defeat the unique contribution that it makes for the field of psychology as well as economics.

If noting that there have been alterations to prospect theory and noting some of the changes would satisfy the reviewer doing so could be accomplished, but to include a section about it would require replicated experimental research supporting outcomes predict by the CPT that are not already predicted by the original prospect theory.

Note:

* L'Haridon (2009) Publication that attempts to demonstrate cumulative prospect theories ability to predict risk-seeking for repeated gambles in the loss domain, but is merely theoretical without scientific experimentation with human subjects to support said claims.

<http://dx.doi.org/10.1016/j.joep.2009.03.007>.

Also note Dr. Levine and mine's paper focused on the gain domain because the experimental research demonstrates replicable shifts in choice preference counter to what the original prospect theory predicted. I have searched for literature to support why it would be relevant to include cumulative prospect theory (CPT) and there are many theoretical papers available, but I have failed to find experimental studies using human participants to support doing so and of the papers I have found none focus on the certainty effect or the different experimental methods used to test for the effect.

* Starmers 2000, focuses on choice theories, but does not focus on certainty effect. Additionally, the gambles guided by feedback paradigm was published a few years after

Starmers review meaning that this work would not have needed to address the results of Barron and Erev 2003.

* Kahneman & Lovallo, (1993). Timid Choices and Bold Forecasts: A Cognitive Perspective on Risk Taking, *Management Science*, has noted that repeated gambles do not follow the outcomes as a single gamble. See footnote pg 20. This article did not appear to use any of the two modified version of the certainty effect (repeated gambles or gambles guided by feedback). This is the only reference to risk-seeking in the gain domain, but does not have experimentation to support the findings.

<http://www.jstor.org/discover/10.2307/2661517?uid=374399021&uid=3739920&uid=18828728&uid=2&uid=3&uid=67&uid=62&uid=3739256&sid=21103004972117>

Kahneman and Lovallo note that the issue with single and repeated gambles exist and has been explored for a long time. The authors also note that the Allais paradox (which is what the paper has looked at primarily) is only one of many paradoxes to explore.

My paper does not focus on why individual shift preference per se, but that the different paradigms do have choice preference shifts and that information processing (FTT) may offer a window into why.

*Redelmeier & Tversky (1992). On the Framing of Multiple Prospects. *Psychological Science*, 191-193.

<http://search.ebscohost.com.ezproxy.uta.edu/login.aspx?direct=true&db=psyh&AN=1992-41785-001&site=ehost-live>

This publication addressed repeated gambles and did not offer a different outcome but more that people should be trained to view problems differently so not to fall into a decision-making trap that can be created by repeated gambles.

Even very critical refinements of Prospect theory are ignored. For example, the probability weighting function in Figure 1 is a very old one, having in fact several problems. Some statements are very misleading. For example, what do the authors mean with the following (p. 18): "... As a result of these findings, questions have arisen about the universal application of prospect theory outside of single gamble scenarios". Do the authors mean that Prospect Theory cannot be used outside the single gamble scenario? This is false.

2.) The interpretation of the results for the repeated gambles paradigm does not, and has not follow what would be predicted by prospect theory as in the cited publications in the review, there is and has been debate as stated by (Keren & Wagenaar, 1987; Redelmeier & Tversky 1992). This is true also for the gambles guided by feedback because the outcomes in these experimental paradigms do not follow prospect theory. In this case resulting in what Baron and Erev, 2004 refer to as the **reverse** certainty effect. These outcomes have been replicated by individuals such as Ralph Hertwig and Tim Rakow as well as in unpublished studies in the Levine laboratory. Once again this is not new; the paradigm has repeatedly shown these results for nearly a decade now. Again, keep in mind this is in reference to the "certainty effect" and not examining other type of decision-making phenomenon, which would be well beyond the scope of this paper. If changing language in the submitted review about choice preference for repeated gambles and gambles guided by feedback would help to sway the reviewers opinion to refer to the outcomes associated with the certainty effect and not others doing so would be easily accomplished.

* When I investigate work by Wakker, his research does not touch on either two of the certainty effect paradigms (repeated gambles or gambles guided by feedback) which is the focus of this paper.

A second major problem of the survey is that it is difficult to understand. Statements are often too vague and so are descriptions of theories. Even very simple points may be difficult to understand for readers not familiar with the literature.

3.) As far as the amount of detail in the article for many of the well-known axioms and fundamental theories, prior to being published the editor requested that I remove much of the information being as the majority of readers will have a developed understanding of much of the materials. The editor felt that including all the detailed information would make a reader feel as though I were insulting their intelligence. This is not only the opinion of the editor but also the first reviewer.

...how does Fuzzy trace theory work in practice? The long description of the theory between 26-30 pages is not satisfactory and it is useless for a reader who may want to check what the theory predicts in a given example.

4.) As far as the complaint about the use of Dr. Valerie Reyna's Fuzzy Trace Theory (FTT), the theory is a psychological theory explaining how an individual acquires information and develops a form of expertise with the information over time. In terms of practice it would explain why an expert decision-maker is able to quickly solve a problem versus a novice being forced to take a step by step method to come to a reasonable solution. The use of FTT was an attempt to provide a psychological explanation about why choice preference may differ depending on the experimental paradigms. The paradigms themselves are resulting in different forms of mental calculation which may explain the conflicting outcomes when testing for the Certainty effect. I would like to request more clarity on how the theory is not clear in order to a.) Better word or b.) Further elaborate on FTT and attempt to determine if it is possible to improve the ability to understand the psychological theory.

The book by Von Neumann and Morgenstern quoted in the survey is *Theory of Games and Economic Behavior*. It is one of the most important books in Economics. It was written in 1943 and the first edition was published in 1944. A second edition came in 1947 and a third in 1953. There was no new edition in 1964. It is possible that John Wiley Science Editions published the book in 1964 (as reported in the references to the survey). But this is not normally the way in which books are quoted.

5.) As far as the problem with the citation of Von Neumann and Morgenstern. I cited the addition that I had in my hand that I read and worked with. I am not aware of any APA guideline that states it is poor practice to cite the version of a text that a writer is using. This is an easy problem to fix should it be deemed necessary.

Reading the paragraph, a reader may be induced to believe that there is an axiom of expected utility called "invariance" used by, inter alia, Von Neumann & Morgenstern in 1964. But this is not true. First of all, "invariance" is not a formal axiom of expected utility, but it is a general principle assumed by several theories developed by economists (not only by expected utility). In this sense, it has a different status with respect to cancellation. The term was introduced by Tversky and Kahneman in their papers written in the eighties (for example, Tversky and Kahneman 1986), and never used by Von Neumann & Morgenstern.

6.) While the term cancellation is not always used, the axiom is synonymous with the axiom of independence which Von Neumann and Morgenstern noted, but did not go into detail about and was indeed touched on by Savage. This citation is a simple and easy correction to make. As far as the citation for invariance, once again this is a simple error to correct. Considering the quantity of material reviewed and considered to include or exclude in the article it is reasonable that a source error may occur once in great while, but can easily be corrected.

Concluding remarks:

I believed I addressed the issues that were raised by reviewer #2 and hope to eliminate any of the misunderstandings about the paper and the focus of this survey. While the prospective is more from the psychological end of the certainty effect, it keeps in the spirit of what was being requested by the special issues call for submission.

Thank you,

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