
Editor's comments:

Enclosed are two referee reports on the paper you submitted to the *Journal of Risk and Uncertainty*, "Individual Subjective Discounting: Form, Context, Format, and Noise." One of the referees was an econometrically-sophisticated reader, while the second reader is less so but is well-versed in experimental analysis in this area. They raised a variety of concerns. Many of the concerns raised by each of the reviewers can potentially be addressed through expositional changes or through appropriate tests.

I thought the abstract was too long.

RESPONSE:

Also, assuming that you can overcome the concerns of the referees, which I hope you will, there is a need to position the paper appropriately for the *Journal of Risk and Uncertainty*. Right now the introduction really seems to be targeted toward a quite different audience. I thought that the material that you tackle around pages 6-9 does a very good job of casting it in the rationality of intertemporal choice literature, which is appropriate for the *Journal of Risk and Uncertainty*. I'm not sure what to do with the Weitzman material, but I think it deserves some mention. The conclusion of the paper seemed a bit weird in terms of what would be appropriate for a *JRU* article. There is much too much about Harrison et al., Warner and Pleeter, and Weitzman. Presumably these are hangovers from when this paper was submitted to the *AER*.

RESPONSE:

Referee # 1

This paper provides both an econometric and survey methodological approach and implementation for estimating individual level discount rates. The paper creatively embeds the approach in structures that allow for distinct forms of non-constant (or exponential discounting) as well as exponential discounting. Very interestingly, the paper links two previously distinct literatures. One proceeds from a view of homogenous discounting functions that allow for non-constant discounting which the authors nest within a "generalized hyperbolic form", while the second proceeds from the approach of Weitzman that considers how standard exponential discounting with heterogeneous discount rates can yield non-constant discounting. Overall I found the paper very interesting, well executed, and well written. However, I do have some suggestions regarding how the presentation might be streamlined (which is not surprising given that the paper tackles a number of issues). I also have a two primary modeling questions that bear on how strongly one should frame the results in terms of whether there is support for a generalized hyperbolic form over the exponential or not. I begin with these two questions and then provide a few editing suggestions.

Comments:

1) My reading of your results indicates that one can not reject the null hypothesis that one can restrict $h = 0$ in the generalized hyperbolic discounting model which yields the exponential discounting model (with a difference in the likelihoods of just 0.397). So why would one examine the results of the hyperbolic models? While you note that the discount rates between the exponential and generalized hyperbolic are indistinguishable, and the likelihoods are negligibly different (page 20), and your concluding paragraph on page 39 clearly states that generalized hyperbolic and the exponential explain the data equally well, you seem to be avoiding making a clear statement about which model to choose. The fact that the using standard model selection approaches one would not choose the generalized hyperbolic is interesting and I believe should be stated clearly (with the caveat as in comment (2) below). I think it is worth noting that the generalized hyperbolic parameter is practically zero (0.000000036718). To be clear, I feel that you have done a very careful job in portraying the wide variety of results in a clear manner and relating them to the literature, but it seems to me that the comparison of these three models deserves a stronger judgment about what model is most appropriate.

RESPONSE:

2) It seems to me that your inclusion of the time horizon variable as a covariate in the three discounting models may be confounding an essential discussion of whether discounting should be best viewed as exponential with heterogeneity, or as inherently non-constant (perhaps with heterogeneity as well). By including the time horizon as a covariate in exponential form in the exponential discounting model, one effectively scales the discount rate up or down for different time horizons (as you discuss on page 21). Thus the presumably constant exponential discounting model is effectively a nonconstant discounting model. I can see how one might wish to specify the model this way as you might view the time horizon as an essential part of the experimental design and you have gone to great lengths (to your credit) to incorporate context effects. However, this formulation means that the generalized hyperbolic and exponential model both allow for non-constant discounting. The generalized hyperbolic in fact allows for two distinct forms, one modeled as the time covariate and the other modeled through h . It doesn't seem too surprising that one does not pick an additional h effect. In summary, you estimate a rich array of models and whatever the results are, if it is possible to make a clear distinction- between inherent individual level non-constant discounting versus exponential discounting plus heterogeneity, I would like to see you provide it. I believe that this would provide a strong additional contribution from your paper.

RESPONSE:

Editing Comments

A) I believe that the Weitzman discussion is more than a digression, it provides a very interesting bridge across literatures which you very usefully exploit.

RESPONSE:

B) I would note in your econometric methodology section a brief reason that you will/can estimate ordered models for instance on page 13 (e.g., the binary case is the simplest but you will have some richer data).

RESPONSE:

C) I think the paper's flow would be improved if the public good component of your discussion in section 2 was moved to a footnote or appendix as it is not essential to the empirical analysis.

RESPONSE:

D) I think you could provide much shorter statement and a reference to how you compute confidence intervals in the middle paragraph on page 30.

RESPONSE:

Referee #2

This paper analyzes various models of discount rates based on an econometric analysis of responses to an internet-based survey. The major innovation of the paper seems to be the econometrics, but I'm not exactly sure what the authors have done. I had a number of questions pertaining to both the survey itself and the empirical results.

RESPONSE:

I'm not sure what we have learned from this paper. The authors estimate an econometric model of the implied discount rates based on a series of hypothetical choices in response to an internet experiment. As they indicate in the abstract, they test the exponential discounting model, a competing hyperbolic model, and a generalized hyperbolic model. What exactly did we learn on that front? The authors hint at a conclusion in the final paragraph of the paper, but if they really believe the conclusion, presumably it should go in the abstract.

RESPONSE:

Here I give comments by paper Section.

The authors review the standard discounting models in section B of this section. I thought this was fine. However, if we couple these models with the subsequent empirical results, I'm not sure where this leads us. As I read the final sentence of this section, the exponential model is the special case where γ equals zero and the simple hyperbolic model is a special case where γ equals 1. Turning now to the empirical results in Table 2, we find that the estimate of the

generalized hyperbolic parameter γ is -17.12. This value is clearly not zero or 1, or even close to these numbers. If we plug -17.12 into equation 3, which is the generalized hyperbolic discount function, does this make any sense in terms of the implications? Perhaps I have misunderstood what the authors have estimated, though it seems that the label they have given the variable in Table 2 is quite clear. If I have not misunderstood what is going on, then there seems to be a fundamental problem of reconciling the empirical results with sensible models of discounting.

RESPONSE:

I wonder why we have the digression on Weitzman. This could have been done at the end somewhere to compare the authors' results to the survey findings of Weitzman.

RESPONSE:

I am also not sure of what section D accomplishes. The authors are aware of the JEL survey that they cite, which pretty much covers all of this literature. Unless the authors are going to relate what they are doing in this paper to these studies, I don't see the point.

II. Here the authors develop their econometric model. So what they are going to be estimating is a series of ordered logits. For the readers such as myself who are less econometrically sophisticated, what exactly are we estimating in Table 2? Are these logits of some sort?

RESPONSE:

As I understand the bottom of page 16, what we have is the individual specific discount rate as an exponential function of a vector of coefficients times the explanatory variables—were these the various measures in Table 2? In general, I think it would be helpful if the authors could make the econometric analysis more transparent.

RESPONSE:

III. The sample for the survey seems to be both weird and possibly troubling. Where did this web-based internet panel come from? How are the panelists selected? They are supposedly from classes at universities, but how do we end up with so many "old" adults in the panel, including some over age 65? While I agree that it is not essential to have a nationally representative sample, one wonders whether the sample members have particularly weird characteristics. The only sample information we have concerns the age distribution and the income distribution. We know in that regard that the largest sample group consists of many who have family income in excess of \$100,000. What about the gender mix of the sample, the educational mix, and so on?

RESPONSE:

While the sample may not be worse than that used in classroom surveys, classroom surveys are not particularly good either. In contrast, the experimental literature may use student subjects but provides financial incentives, so at least we know that the subjects are dealing with real decisions that have real consequences, as opposed to hypothetical choices.

RESPONSE:

I understand that there is a literature dealing with contingent valuation surveys, and this study seems to be in that same general vein. However, there is also a series of tests that can be applied to provide some sense that their results are meaningful. The authors should show that their survey meets the types of tests that emerged after the Exxon-Valdez debate.

RESPONSE:

We know very little about the survey. All we see is Figure 1. Is that the only thing of consequence that was in the survey? The dollar values were intended to reflect the typical cost of 'a public good,' but was that a context that was provided in any way to respondents? The time horizons varied from 20 to 40 years. This seems like a really long time, especially for 60 year olds, most of whom will be long gone before they can recoup their hypothetical payoffs. I will return to this below in reference to the empirical results. Here I would like to see lots more raw data of mean values for different choices and time horizons. The paper strikes me as really abstract and such data would be a major improvement.

RESPONSE:

Looking at Figure 1, how did the authors handle these answers? Is "definitely yes" really different from "probably yes"? The authors indicate on page 19 that there are multiple ways of answering in terms of levels, but they don't tell us what they did with these multiple levels. Why not just use yes and no?

RESPONSE:

IV. The effect of the annual payments is the opposite of the experimental results that have been found in the previous literature, as noted on page 21. From my vantage point, this contradiction with established empirical results may cast doubt on the validity of the study. People dealing with real choices in experimental contexts express quite different preferences than those resulting from the hypothetical choices. The authors have to do a lot more than they have already to justify why hypothetical choices should be taken seriously.

RESPONSE:

I am not sure if I understand how to interpret the empirical results. Take, for example, the variable "age". On page 22, the authors state that "discount rates appear to be *larger* for individuals in this sample who are older ..." Appear to be? Looking at the log-age coefficients in Table 2, they are all strongly significant, so doesn't that mean that they are all larger? Unfortunately, they are not really larger for everybody. If you look at the female interaction with the log-age variable, it essentially knocks out the age effect. So rather than saying that discount rates appear to be larger as you get older, isn't it more correct to say that discount rates are significantly higher for older males, but there is no statistically significant relationship for older

females? More generally, the authors should do a much better job of explaining what the tables mean, just to assist readers such as myself in interpreting what's there.

RESPONSE:

V. Here the authors summarize the implications of their results, where the fitted individual discount rates range from 2 percent to 20 percent. Let's see what a 20 percent discount rate means for their time period. With a 30-year delay, \$1000 paid then is worth \$4 today. With a 40-year delay, \$1000 paid then is worth \$0.70 today. Going back to the types of tradeoffs shown to me in Figure 1, I unfortunately don't see an option that has such a low order of magnitude. More generally, is it possible to get precise estimate discount rates over a 40-year time horizon that make sense?

RESPONSE: