

Dear Editor,

We have now taken the time to revise our paper in light of the referee reports. Below we reproduce their comments in italics and follow with our response in plain text. Both referees also had overarching positive comments on our paper; we omit these below as they require no changes to the manuscript.

Response to Referee 1

Around line 221 there is a phrase about "normalizing by unit change in consumption." This is awkward terminology and might be clarified.

Phrase has been edited.

Around line 221 it is stated that "IAMs are built around the deterministic Ramsey model." This is true of Nordhaus's DICE model, but most of the others lack a full optimal growth framework.

Yes, thank you. We have changed to wording to make this clear.

Around line 342, one might elaborate a little further on the dual role of η as a coefficient of relative risk aversion and the intertemporal elasticity of substitution. For little uncertainty the discounting aspect dominates. For a lot of uncertainty, the risk premium aspect can dominate. It might be useful to go over this briefly.

We have added discussion on this point.

Response to Referee 2

First, the focus on the social cost of carbon is interesting and appropriate. The paper's arguments and literature review, however, apply in equal measure to evaluating the benefits generated by non-marginal changes in greenhouse gas emissions. This is a point that might be emphasized at various points in the text.

Yes, we have added more text emphasizing this.

Second, I like the fact that the paper explains in a straightforward way how some very basic ideas from the theory of decision-making under risk can be applied to the economics of climate change. This is necessary because some of the leading climate economists – notably Nordhaus and Tol – have pushed models that (methodologically) are premised on the assumption of intertemporal optimization under perfect foresight. It's true that Nordhaus and Tol have used their models to conduct Monte Carlo simulations concerning uncertainties in the parameters. But the preference parameters invoked by Nordhaus and Tol assume levels of risk aversion that are plainly inconsistent with people's observed economic decisions. That highlights the rather urgent need to bring climate economics and integrated assessment models back into touch with the basic economics of decision-making under uncertainty. That in a nutshell explains the value of the present contribution.

Thank you; we agree!

Third, I like the fact that the paper emphasizes basic expected utility theory while also touching on the emerging literature on ambiguity aversion. I wonder if it would be possible to expand the paper somewhat to attach more weight to this latter topic. I could be persuaded that a proper exposition would render the paper much more technical, while at this stage there is not that much work published on ambiguity aversion and climate change. So I will leave this issue in the hands of the authors and the editor.

We have had internal discussions on this point and conflicting opinions from readers. For instance, another economist who commented on our paper privately to us suggested we remove the discussion of ambiguity premiums entirely. We feel that the current discussion is enough to introduce the topic and discuss its relevance to our topic for those interested readers, but expanding it might move the paper in a different direction that some readers may not appreciate. Further, as the referee notes, there is very limited work on this topic to date and this review is not the place to develop new theory. Given all this, we have decided to leave the ambiguity discussion as is.

Finally, my take is that a fair interpretation of the recent literature suggests that the risk premium is likely to be numerically quite large, both because the level of risk aversion is much higher than previously thought, and because uncertainties about climate sensitivity and the damage function are much, much larger than assumed by models such as FUND, DICE, and even PAGE. The authors may or may not agree with me on this, and I understand in part while their conclusions are a bit equivocal, mainly (and rightly) pointing to the need for more research. Still, the paper provides a nice exposition and literature review without generating much in the way of interesting and striking conclusions. It's as though the authors don't want to risk putting their cards on the table and telling the reader what lessons they (tentatively) perceive. In other words it would be nice if the abstract and conclusions section were at least somewhat more conclusive.

We have now strengthened the conclusions on this point.