

Ackerman and Stanton response to reviewers:

Thanks to both reviewers for helpful comments that have led to a much-improved, revised version of the paper. We have made two major changes – corrected our use of estimates based on the Hanemann paper, and added an extended discussion of the ways in which our estimates would change on a low-emissions trajectory. In addition, we have made a number of smaller changes in wording and citations, as requested by the reviewers. (We have also added a clarification to the abstract, as requested by the one additional comment posted on the website.)

Response to Reviewer 1

Reviewer 1 offered several detailed comments and one bigger issue. In response to the detailed comments we have:

- Clarified that SCC values mentioned in the abstract are per ton of CO₂.
- Added a footnote on p.3 with citations to descriptions of DICE, PAGE, and FUND.
- Noted on p.3 that the models ran in Monte Carlo mode, examining multiple uncertainties in the case of PAGE and FUND.
- Added a citation to the EMF model comparisons.
- Softened and shortened language about potential effect of emissions trajectories on SCC estimates, and referenced more extended discussion of the topic, added later in the article.
- Explained that the 95th percentile results for PAGE and FUND refer to variation in all Monte Carlo variables, not simply climate sensitivity, on p.6.
- Clarified that the risk-free rate referred to on p.10 is a long-term average (implying that the citation in footnote 12 to the multi-decade averages in DeLong and Magin (2009) is appropriate, and a newer source is not needed).
- Confirmed, in a footnote, that marginal abatement costs discussed from other studies are in 2005 dollars, while our SCC estimates are in 2007 dollars (a difference in value of 6 percent, which does not seem to warrant inflation adjustment on such rough estimates).

The bigger issue raised by reviewer 1 is that we compared marginal abatement costs for aggressive abatement scenarios to SCC estimates based on emissions trajectories assuming little or no abatement. If such aggressive abatement occurred, wouldn't the SCC be lower?

This question is partially but not entirely answered by another point from reviewer 1, citing Hope, "The social cost of carbon: what does it actually depend on?" (2006). That paper argues that the SCC is insensitive to changes in emissions trajectories, because two opposite effects roughly cancel each other: at lower concentrations, a given increase in emissions causes a greater change in forcing, and therefore in temperature; but at higher concentrations (and therefore higher temperatures), a given increase in temperature causes a greater increase in damages.

To test the significance of emissions trajectories for our results, we repeated our calculations, replacing the CO₂ emissions and non-CO₂ forcings in all five DICE scenarios with the corresponding data from the IPCC's new RCP2.6 (or RCP3-PD) scenario. This rapid-reduction scenario reaches negative net emissions of CO₂ by 2080. As shown in Figure 6 in our revised paper, this lowers most of our SCC estimates but leaves our qualitative conclusion unchanged.

The effect of the recalculation on our estimates is noticeable but much less than proportional to the change in emissions – probably for the reasons discussed in Hope (2006).

Response to Reviewer 2

Reviewer 2 raises four major comments. The first of these is the same as the bigger issue raised by reviewer 1; see response above.

Comment 2 expresses three reservations about our alternative damage functions.

- The Hanemann and Weitzman working papers which we cite have not been subject to peer review, should they be used as inputs in a paper in a refereed journal? In fact, the Nordhaus damage function in DICE-2007 also has not appeared in a peer-reviewed journal article. While mentioned in a recent Nordhaus book, it is supported in detail only by a memo on the author's website describing the calculations. Nonetheless, it seems appropriate to discuss the latest Nordhaus damage function now, without waiting for him to get it published in a peer-reviewed journal. The status of the Hanemann and Weitzman working papers, relative to peer review and academic discourse, is exactly the same as the Nordhaus calculations memo: they are recent writings by leading researchers in the field, which are worth discussing now while they are at the cutting edge of debate, rather than waiting (perhaps years) until they are published elsewhere.
- The Hanemann analysis which we cite was comparing the author's calculations to DICE-1999, not DICE-2007, estimates. The reviewer is correct; thank you for pointing this out. Both the Nordhaus damage function (1999 and 2007 versions) and the Hanemann damage calculation are based on point estimates at 2.5°C. Hanemann's estimate is 4.0 times as large as the DICE-1999 estimate, or 2.4 times as large as the DICE-2007 estimate. We have revised all our calculations accordingly; our Hanemann-based estimate is now correctly calibrated to 2.4 times the DICE-2007 estimate at 2.5°C.
- Is the Weitzman estimate of serious damages at 6°C and 12°C of warming merely a casual characterization of the last time global temperatures were at or near those levels? Our response is two-fold. First, it is surely of interest that the last time global temperatures were at or near those levels, more than 30 million years ago, sea levels were tens of meters higher than at present (since ice sheets do not survive such temperatures); adaptation to or defense against that much sea level rise is difficult to imagine. Second, Weitzman does in fact cite an important piece of recent science about damages at these temperatures: the study by Sherwood and Huber (also cited in our paper), finding that at 12°C of warming, areas where half the world's population now lives would, at least once a year, reach temperatures that human beings cannot survive.

The third comment asks for discussion of the fact that the U.S. government based its SCC estimates, and hence policy resting on those estimates, on global rather than domestic actions. It also objects to our statement that the low estimate is “difficult to reconcile with the belief that it is urgent to take action...”, noting that IPCC reports cannot be the basis for this since they do not prescribe policy objectives. We have addressed this with two changes to that section of the text:

- We have added the statement that the Working Group analysis is also significant “for its recognition that policy should be based on global, rather than domestic impacts (unlike most national environmental policies).”
- We have replaced the “difficult to reconcile...” text with the statement that a low SCC estimate “seems to suggest that only modest, inexpensive measures are needed to address climate risk.” We have also eliminated the citation to the IPCC at that point.

The fourth comment asks why we are “comparing a handful of new SCC estimates with the distribution based on thousands of model runs” in the U.S. government’s analysis, and asks for “a formal representation of the probabilities for the states of the world” implicit in our estimates.

Regarding our “handful” of estimates, the scope of our effort may not have been clear: the results we report are based on 40,000 runs of DICE for each of five emissions scenarios; we did 10,000 iterations of the Monte Carlo analysis of climate sensitivity, for each of our four damage functions. (We have added a sentence making this explicit.) The low-emissions trajectory sensitivity analysis, discussed above and added in revisions, involves another 40,000 runs.

Regarding formal representation of probabilities, this is possible for only one of the parameters that we vary in our analysis.

- For climate sensitivity, the Working Group introduced a formal representation of probability, which we accepted, leading to the identification of average and 95th percentile values.
- For discount rates, there are sharp differences of theoretical frameworks and interpretations; it does not make sense to talk about the probability that the Stern Review, or its critics, were correct or incorrect about the appropriate choice of a discount rate. We believe that our two choices span the most active range of the climate economics debate – roughly speaking, from Stern to Nordhaus-style discount rates. As noted in the article, our two choices approximate the range that the Working Group described as supportable under the prescriptive approach to discount rates.
- For climate damages, the integrated assessment literature is relatively weak, and lags far behind research on sectoral impacts and damages. The alternatives we examined represent two important poles in the discussion: first, a re-estimate of non-catastrophic low-temperature damages to be higher than is currently assumed in many IAMs; second, a way of modeling catastrophic damages at somewhat higher temperatures. Based on the available research, we believe these are the two most important new directions for climate damage modeling; we are not aware of any body of research suggesting that damages could be much lower than assumed in current integrated assessment models.

Thus among our parameters, only climate sensitivity is amenable to formal representation of probability. The others are inevitably (for us, or anyone in the field) judgments about the range of uncertain values that should be considered. We have attempted to make this clear in rewording the conclusion to the article.