Response to referee report on 'The treatment of risk and uncertainty in the US social cost of carbon for regulatory impact analysis'

I would like to thank the referee for his/her useful comments on my paper, which I would like to respond to here. For clarity of presentation, the referee's comments are italicised.

Referee's general comments on the paper

One of the referee's main points is that *"the author does not distinguish between critiques of the models themselves, which should be directed at the model developers, and how the results are used by the WG. In other words, the WG is a consumer of the IAMs and is not in a position to change damage functions or parameter distributions."*

Apportioning responsibility for the analysis of the Working Group between members of the group from the federal government on the one hand and modellers on the other is indeed an important issue. Moreover, I accept the contention that, in general, the modellers were in a better position to run their models than the Working Group was. In the paper, I mean to implicate both parties in the results of the analysis. I accept the case for clarifying this in a revised version of the paper, and acknowledging the fundamental difficulty faced by government policy-makers in conducting analysis with integrated assessment models (I have personal experience of this).

However, I do not fully agree with the statement *"the WG is a consumer of the IAMs and is not in a position to change damage functions or parameter distributions."* Above all, the veracity of the statement is contradicted by the WG's own analysis. In particular, the WG did indeed intervene to change parameter distributions; e.g. there is a detailed account on pages 13-15 of the WG report of how the group specified the climate sensitivity parameter distribution for the modellers to go away and implement. Similarly, in the pages that follow, WG assumptions about baseline socio-economic change and discount rates are detailed. It would thus have been in keeping with the WG's otherwise close engagement in the modelling issues to extend their considerations to the damage function.

The referee's other main point, namely that there are *"several unsubstantiated or arbitrary claims that require either citations or more explanation in order to be useful"*, will be addressed in my responses to his/her specific comments below.

Referee's specific comments on the paper

The author's first claim is that the WG did not go far enough in its exploration of uncertainty about the parameters of the damage functions in the IAMs.

- *This is an example of a critique that should be directed at the developers of FUND, DICE, and PAGE rather than the WG.*
- *As the author later acknowledges, FUND and PAGE treat many damage function parameters probabilistically and the author correctly points out that the model developers are forced to extrapolate the damage functions to temperatures for which we have no data. If the WG were to arbitrarily alter the damage function to allow for higher damages at the same temperatures they would be doing so under the same lack of data but without the peer review process that the IAMs have survived.*

- *Another point worth mentioning here is that while it is true that we do not have data on damages at high temperatures we don't know what modern economies and other human systems can adapt to. Adaptation is another area of tremendous uncertainty that cuts in the other direction but does not receive a single mention in this paper.*

The first bullet point is covered by my response above. On the second, the key point is that, given the total lack of data on which the extrapolation of damage functions to high levels of warming could be made (see also Tol, 2011), any functional form is arbitrary, and a deviation from the modellers' default choices is no more or less arbitrary, at the problem's simplest level. Moreover, there are several peer-reviewed precedents for the sort of functional form I am advocating, including Peck and Teisberg (1992), Weitzman (2010), Ackerman *et al.* (2010) and Dietz (2011). The third point, on uncertainty about adaptation, feeds into the broader uncertainty about the functional form and parameterisation of the damage functions. I certainly do not deny the importance of adaptation, especially at low levels of warming. Indeed, probabilistic approaches to parameterising the damage function, such as Dietz (2011), admit the possibility that damages are linear or even concave with global mean temperature, a story consistent with enormous adaptive capacity. Nevertheless, admitting the opposite possibility (not certainty) that damages could rise very steeply with warming drives up the certainty-equivalent social cost of carbon, which is my core point.

The second claim is that by using an exogenous constant discount rate the WG underestimated the effect of low-probability, catastrophic consumption losses.

- *This is a valid argument and one of several reasons for using Ramsey discounting in the simulations the way the author describes.*

This was the WG's most elementary mistake (if one can call it that), so I am glad the referee shares my view.

The third claim is that a simple averaging of the three IAMs necessarily implies two things: First, that the WG adheres to the principle of insufficient reason and second, that the WG is ambiguity neutral.

- *The author describes a different interpretation of ambiguity aversion than I am familiar with. The author claims that ambiguity aversion would imply that WG should have given more weight to the model that produces the highest SCC. I would say ambiguity aversion implies the model with more quantitative probabilistic treatment of damages receives the most weight.*

The standard definition of ambiguity aversion in economics is a preference for actions with known probabilities over those with unknown probabilities, all else being equal (Itzak Gilboa, 2009). There are competing models for representing ambiguity aversion, but a 'smooth model of ambiguity aversion' has been developed by Klibanoff *et al*. (2005), which is tractable in empirical applications and is thus arguably the leading model in the field. They show that ambiguity aversion implies a dislike of mean-preserving spreads in the expected utilities of multiple models, which in turn implies placing relatively more weight on models in which expected utility is low (which is a simple analogy to risk aversion in the range of utilities), which in this context are the models with a high SCC. Moreover, the smooth model is fairly general and is capable of nesting other classic models in this field such as Gilboa and Schmeidler's (1989) 'maxmin expected utility', which incidentally would place all weight on the model with the highest SCC. Some of these further citations could be added to the paper.

The fourth claim is that a quantitative long term target with carbon price set at MAC rather than SCC has the two fold advantage of being more certain that a given target will be met and less uncertainty in the correct price.

- *The author acknowledges that setting an optimal target requires knowledge of the SCC but does not seem to give that fact much standing in the rest of his argument.*
- *If the author could convince the reader that uncertainty in the target (because of an uncertain SCC) is less harmful than uncertainty in the calculation of the price (because even if the calculation were precise you would arrive at a suboptimal policy if the target was off) then this section would carry more weight. As it stands, uncertainty in the SCC precludes any economically efficient carbon policy – price or quantity based.*

On the first point, I do acknowledge the role of the SCC in setting the quantitative emissions target in the introduction, but admittedly not in the later section, where more extensive discussion is offered. This discussion could thus be expanded upon, although the argument has already been made in Dietz and Fankhauser (2010) and consequently the editors may take the view that the citation is sufficient.

As far as I can understand, the second point is essentially about the comparative efficiency of price and quantity instruments under uncertainty. This is discussed on page 9 of the paper. Opinion in the profession differs on whether the relative slopes of the marginal abatement cost and marginal damage cost functions favour a price or quantity instrument. In the short run, I fully agree with those like Pizer (1999) who argue that the analysis favours a tax, but in the long run it is consistent with my arguments about the convexity of damages with respect to temperature that a quantity target is preferred (see also Stern, 2007).

Line-by-line comments

Page 4, last paragraph – I don't think the claim that 18 degree warming only results in 50% GDP loss in DICE is relevant. Equilibrium climate sensitivity would have to be well above any conceivable value to reach an increase in average global temperature of 18C over the relevant time horizon. I suggest choosing a lower temperature increase that could be reached by the end of the century with a high but still conceivable climate sensitivity.

The thought experiment with the Nordhaus quadratic aggregate damage function of how much warmer the world needs to become, relative to the pre-industrial level, in order for 50% of global GDP to be lost is quite well known, and that is why I have chosen to use it to illustrate my point (in retrospect I should provide a citation to earlier mentions). I cannot disagree, however, that 18C warming may be impossible (or perhaps extremely unlikely) to be reached within any timeframe relevant for decision-making. One could easily select a lower global mean temperature at which to read off damages, and I could do so, but my preference would be to retain the current formulation due to its relative prominence.

Page 4, last paragraph – "While the parameters of the damage function in PAGE are modeled as random, such that damages reach up to around 10% of global GDP when global warming reaches 5°C…" Is that statement based on your own runs of PAGE, the damage function figure in the SCC Technical Document, or some other source? In any case you should make that clear and tell the reader if this is the 95 percentile, the maximum, etc. In other words, what do you mean by, "damages reach up to around 10% of global GDP?"

Happy to clarify: the source is Warren *et al.* (2006), which includes a technical description of PAGE2002 and some default modelling runs, and it is the maximum.

Page 4, last paragraph – The phrase, "it has equally been argued" is ambiguous and begs for documentation. What constitutes equally? Equal to what, the number of times DICE has been used and cited?

This is a turn of phrase and could be replaced with something like "on the other hand" if thought necessary.

Last sentence on page 4 – "Surely it is at least possible that climate damages will exceed 10% of global GDP upon 5o warming." Yes, it is "at least possible" but that is hardly a compelling argument on which to base carbon emissions policy for the United States.

There are clearly many issues bound up in determining what constitutes a compelling argument for US mitigation policy. However, one straightforward observation is that a rational expected utility maximiser should absolutely, if s/he ascribes some small positive (subjective) probability to damages over 10% of global GDP upon 5C warming, include it in his/her analysis. In fact, this observation is not limited to expected utility or to maximisation. Saying otherwise either implies the contingency is given zero probability, which the referee does not appear to do, or it is a deviation from standard decision approaches.

Page 5, second paragraph – "Combining steeply increasing damages with a positively skewed distribution on the climate sensitivity parameter, I …find that the SCC could be hundreds of dollars higher than previously estimated (Dietz forthcoming)." I appreciate that there is a forthcoming paper that describes the analysis but this also seems arbitrary to me. Increasing the slope of the damage function and the skewness of the climate sensitivity parameter can produce any SCC you like. What are these changes based on?

The paper is now published (see the reference list below). The pdf of the climate sensitivity is based on a paper in Nature by Stainforth *et al*. (2005). The pdf of the damage function exponent is subjective (see my comments above about arbitrariness), but passed a tripartite peer-review process in *Climatic Change*.

References

Ackerman, F., Stanton, E. A., & Bueno, R. (2010). Fat tails, exponents, and extreme uncertainty: simulating catastrophe in DICE. *Ecological Economics, 69*(8), 1657-1665.

Dietz, S. (2011). High impact, low probability? An empirical analysis of risk in the economics of climate change. *Climatic Change, 103*(3), 519-541.

Dietz, S., & Fankhauser, S. (2010). Environmental prices, uncertainty, and learning. *Oxford Review of Economic Policy, 26*(2), 270-284.

Gilboa, I. (2009). *Theory of Decision under Uncertainty*. Cambridge, UK: Cambridge University Press.

Gilboa, I., & Schmeidler, D. (1989). Maxmin Expected Utility with Non-Unique Prior. *Journal of Mathematical Economics, 18*(2), 141-153.

Klibanoff, P., Marinacci, M., & Mukerji, S. (2005). A smooth model of decision making under ambiguity. *Econometrica, 73*(6), 1849-1892.

Peck, S. C., & Teisberg, T. J. (1992). CETA: a model for carbon emissions trajectory assessment. *Energy Journal, 13*(1), 55-77.

Pizer, W. A. (1999). The optimal choice of climate change policy in the presence of uncertainty. *Resource and Energy Economics, 21*, 255-287.

Stainforth, D. A., Aina, T., Christensen, C., Collins, M., Faull, N., Frame, D. J., et al. (2005). Uncertainty in predictions of the climate response to rising levels of greenhouse gases. *Nature, 433*(7024), 403-406.

Stern, N. (2007). *The Economics of Climate Change: The Stern Review*. Cambridge, UK: Cambridge University Press.

Tol, R. S. J. (2011). *The uncertainty about the total economic impact of climate change*. Dublin: Economic and Social Research Institute (ESRI).

Warren, R., Hope, C., Mastrandrea, M., Tol, R., Adger, N., & Lorenzoni, I. (2006). *Spotlighting impacts functions in integrated assessment: research report prepared for the Stern Review on the Economics of Climate Change*: Tyndall Centre for Climate Change Research.

Weitzman, M. L. (2010). What is the "damages function" for global warming - and what difference might it make? *Climate Change Economics, 1*(1), 57-69.