

Response to referees.

(Interagency Working Group on Social Cost of Carbon 2010).

I thank Prof. Hammitt and Anonymous for the attention given to my manuscript. Their comments indicate that I should have underscored more emphatically the study's point of departure. In early 2010, the United States government published estimates of the social cost of carbon (SCC) for use in regulatory cost-benefit analysis.¹ These estimates concern the climatic benefits of regulations that reduce carbon emissions, are based on the expected damages associated with an extra ton of emissions. Thus a cost of inefficient refrigerators should reflect the monetary damages of the excess emissions they produce and should not take into account any social benefits or losses from diminished refrigeration caused by higher refrigerator costs. This paper grew out of policy discussions about ways of incorporating a risk premium into applied SCC estimates. Accordingly I set out to compute a risk premium based on damages alone, and conforming as closely as possible to the guidelines in the US government analysis. A companion manuscript² reviewed the literature and found a diversity of approaches but very few explicit calculations of a risk premium. I find a lower bound on society's willingness to pay (WTP) based on "revealed preferences". The risk constraint elaborated by Andersen and Bows³ served as an illustration. My manuscript was not intended as an endorsement either of the US government methodology nor of Anderson and Bows's interpretation of society's revealed preference; rather, the goal was to show, given this point of departure, how a risk premium might be calculated. It is gratifying that neither reviewer found reason to contest the idea of computing risk premium as the cost which a risk-neutral insurer would charge for the risk swap in moving from the BAU to a risk-compliant emissions path. Rather, the substantive comments are mostly directed to these points of departure, and to the computation of the Shapley value.

Prof Hammitt

Prof Hammitt raises three reservations:

- (1) *"it is not necessarily true that a risk-averse agent would be willing to pay more than the reduction in expected damages to exchange one risky prospect for another"*
- (2) *"WTP to swap one risky climate damage prospect for another depends on the dependence of the climate risks...on other risks"*
- (3) *"society's preference to swap a BAU emissions path for a risk-constrained path implies that its WTP exceeds the combined value of the reduction in climate risk and the abatement costs required to achieve the swap".*

¹ US Government Working Group on Social Cost of Carbon (2010). Social Cost of Carbon for Regulatory Impact Analysis Under Executive Order 12866. Washington, DC, February.

² Kousky, C. Kopp, R.E. and Cooke R.M. (2011) "Risk Premiums and the Social Cost of Carbon: A Review", this volume.

³ Anderson, K. and A. Bows (2011). "Beyond 'dangerous' climate change: emission scenarios for a new world " Philosophical Transactions of the Royal Society A **369**: 20-33.

Reservation (1) is completely correct. The statement in the manuscript following eq.3 “If wealth is not fixed, a more complicated set-up is required, but the end result is the same” is wrong. A more careful formulation is on the following page: “[risk-averse individuals’] utility function is concave and the expected utility of a lower variance risk is greater than the expected utility of a higher variance risk with the same expectation” with the footnote “This is an approximation based on neglecting higher order terms in the Taylor expansion of $U(X)$ around the expectation of X ”. Thus, $U(x)$ is approximated as $U(x_0) + (x-x_0)dU(x_0)/dx + (1/2)(x-x_0)^2d^2U(x_0)/dx^2$, where $E(X)=x_0$. Taking expectations of both sides yields $EU(X) = U(E(X)) + AVar(X)$ for some positive constant A . The restriction to risk averse utility functions of this form should be more visible.

Reservation (2) is correct, but is directed to the guidelines in the US government analysis.

Reservation (3) is also correct and again refers to the US government analysis’s stipulation that the SCC be based on damages. As Prof. Hammitt notes, this does not contradict the computed lower bound on WTP, but entails that this is not the greatest lower bound. Although abatement costs could reasonably be included, I would prefer neglecting these on a first pass as the computation of abatement costs involves many additional assumptions and uncertainties. The US government guidelines stipulate that only uncertainty in climate sensitivity be taken into account. In computing a minimal cost risk compliant emissions scenario, I assumed that abatement costs were proportional to the square of emissions reductions relative to the BAU scenario. To compute the actual abatement costs, one would have to determine the constant of proportionality.

Anonymous

I address the comments point-wise.

“no risk premium is necessary’ This passage juxtaposes Nordhaus & Mendelsohn and Yohe & Tol. That juxtaposition is false”. For clarity, propositions are true or false, not juxtapositions. The point was that “different frameworks” lead to different results, and the cited statements show how different these are.

“Throughout the text, the 2 degrees / 19% target should be labeled as illustrative. It is not a revealed preference”. It was not my intention to endorse the Anderson and Bows interpretation of society’s revealed preference. On re-reading, however, I see that statements like “we follow Anderson and Bows...” could be interpreted that way. Mea culpa. *“Contrary to what was promised in the introduction, risk is not priced.”* Table 6 shows the price allocations for the first three periods 2015, 2025 and 2035. This table could obviously be extended out to 2205.

“The second half of Section 3 is very problematic. Why did the author use the Excel version of DICE, which is wholly unsuited for this kind of exercise, when there is a GAMS version of the same model that is specifically designed to do this? Why make up an algorithm when there is centuries of experience with optimization?” Is the claim that the GAMS version would lead to a different result? This is a stochastically constrained optimization: find a cost minimal emissions path for which the probability that temperature exceeds 2°C is less than 0.19. This involves searching all emissions paths which satisfy the probability constraint and picking the one which is cost minimal. This would involve a Monte Carlo simulation of each possible emissions path.

This is not supported in GAMS or any other package, as far as I am aware. Instead, a proxy optimization is described in the text, based on the observation that maximum temperature is effectively determined by total emissions. This converts the optimization problem into a simple line search for which EXCEL with CRYSTAL BALL is admirably suited. A more complex stochastic optimization involving multiple uncertain variables with dependence could be performed with equal ease, an exercise I hope to pursue. It must be noted that CRYSTAL BALL assumes that dependence is represented by the normal copula, which assumption is far from innocent. Another proxy optimization could be based on the observation that maximal temperature is monotonic in climate sensitivity, *if* climate sensitivity is the only uncertain parameter. Since this would not generalize to more realistic uncertainty analyses, I did not use it.

“Section 4 is strange too. Having defined a risk-compliant path, the author then worries about how to allocate the cost. This is a relevant question between actors, but not over time as done here.” Indeed, I treat emitters in different time periods as different actors.

“The author worries about feasibility in emissions pathways, but ignores feasibility in income.” Again, this results from following the US government analysis.

“Then, Shapley is introduced but never computed as far as I can see. The title promises Shapley. The introduction promises risk pricing. These are different things, but neither is done in the paper”. This I find puzzling. The Shapley value is given in eq (8), and is computed accordingly. The Shapley value is a cost allocation algorithm. Table 6 shows these allocations.

“Section 5 is confusing. The title has “Shapley”, the text was “willingness to pay” and the tables have “insurance price lower bound” and “lower bound insurance price”. Presumably, the last two are the same, but I fail to see how they relate to the first two concepts. Given that the uncertainty about the climate sensitivity was used to derive the emissions trajectory, I do not see how it can be used again to derive a standard deviation around the “insurance price”. If the “insurance price” is P in equation (1), then it would be incorrect to do so.” The reviewer is confused here, so it follows that the text must be confusing. This may relate to the remark that GAMS is designed to do this. According to eq (8) the computation involves taking the average over all permutations, of which there are $20! = 2.4E18$. For each permutation the insurance price depends on the damages, which in turn depend on the temperature and thus on the uncertain climate sensitivity. The confusion perhaps comes from the way in which the computation of the Shapley value is combined with the optimization routine. I had hoped to intercept this confusion by clearly spelling out the optimization routine. It goes like this. Abatement costs for a reduced-emissions path are assumed proportional to the square difference between emissions on the BAU and on the reduced emissions path, discounted to the present. For each value M of total emissions and each discount rate, we find a cost minimal emissions path with M total emissions. This is a simple constrained optimization and does not depend on climate sensitivity. Then, by sampling climate sensitivity, we find the value M^* for which the probability of exceeding 2°C on the cost minimal path is 0.19. A larger probability would violate the probability constraint, a lower probability would abate too much and leave the probability constraint non-binding. This cost minimal path for M^* is used in computing the Shapley value. The temperature induced damages of this cost minimal path depend on climate sensitivity. A flat-footed computation would cycle through all permutations, for each permutation computing the mean damages by Monte Carlo simulation of climate sensitivity. Then we would average these mean values over

all $2.4E18$ permutations. This is obviously infeasible. Therefore, we sample a permutation from the uniform distribution over all permutations, sample a value of climate sensitivity and compute the term under the sum on the right hand side of eq (8). Note that the distribution over climate sensitivity and the uniform distribution over permutations are independent. For a large number of samples, the resulting mean approximates the right hand side of eq (8), which is the Shapley value. The variance includes contributions from climate sensitivity and from the uniform distribution over permutations. The computation is correct. It is well known among specialists, but not generally appreciated, that the accuracy of a Monte Carlo estimation of moments (provided they are finite!) depends only on sample size and not on the sample space. I hope this adequately explains things.

“Then, marginal damages are introduced, which are irrelevant in a risk-compliance framework.” Yes, it is irrelevant for the insurance price, but it is relevant for the SCC inasmuch as this includes the price of insurance and the marginal damages.