Response to Anonymous reviewer comments

Reviewer comments in italics, my replies in regular type.

This paper tries to do too much, and does not do anything particularly well.

The paper covers a lot because it aims to set up a broad framework upon which future studies can build.

The notion that multiple externalities requires multiple Pigou taxes is well known. It can be found in any textbook in environmental economics.

Indeed this is well known, and this need is what motivated this study. However, as the discussion and cited literature shows, knowing that in principle the multiple externalities should be accounted for has not translated into them being included in emission metrics to date, at least in any studies I'm aware of (if the reviewer knows of such studies I'd be happy to learn of them).

Table 1 is the core result of the paper. There is double-counting. Instead of correcting the DICE model with the latest health impact studies, the author adds things at the margin. The author does not demonstrate that addition at the margin is appropriate (it is only under very particular assumptions).

Agreed that Table 1 presents core results. The only potential case of double-counting is for climate-health impacts, as these are calculated using the WHO analysis and included in the IAM-based valuation of climate damages. The table footnote and the text explicitly discuss this issue, however, and state that the sum therefore uses 50% of the WHO-based climate-health impacts with an uncertainty range equal to half the magnitude of that effect so that the results span zero additional climate-health impact beyond that in the IAMs to the full value of the WHO-based climate-health impact which can be substantially larger than the total climate valuation from the IAMs. Thus while there may be some double-counting at the highest end of the uncertainty range, there is unlikely to be substantial double-counting over most of the range unless the IAM-based climate damages are dominated by climate-health impacts. The IAM-based valuation is not readily separated into components, so a better approach is not obvious. The only other potential double-counting would be composition-agricultural impacts for CO2, but these are not included precisely to avoid double-counting as stated already in the Table.

As noted above, the IAM-based damage function is not readily separated into components based on the particular impact. Thus it is not clear how to alter the DICE model to include a new damage function based on a revision to the climate-health impact alone. As for adding things only at the margin, the primary point of the paper is to present a metric including a broader array of impacts such as composition-health and regional climate change for aerosols and to cover a broader array of pollutants. Those additional effects are by no means marginal for methane or the aerosols. For CO2, the reason for evaluating the health impacts separately was not to make a marginal change to the IAM-based climate damages, but to use the same methodology for climate-health valuation as for composition-health valuation. Hence I took the WHO values for premature deaths due to climate change and applied the same valuation as for premature deaths due to air pollution so the multiple impacts within the SCAR would be consistent. As described previously, half that value (and it's range) was then used to address the double-counting issue, but this preserves the primary goal of using comparable methodology for health impacts across the various pathways through which those impacts take place. As discussed in the text, the climate-health calculation also revealed a potential underestimate in the conventional IAM-based valuation of climate damages.

I tried to reconstruct the estimates for black carbon, but failed. The paper talks about BC in a number of places, but never gets specific.

The program to calculate the valuation is now posted alongside the article so that any estimates can be reconstructed. Specific values for each portion of the BC valuation are given individually in Table 1 and each portion of the calculation is described in the text (e.g. that BC contributes 5.5% of the present-day population-weighted PM2.5, and thus that fraction of the total valuation is assigned to BC).

The value of a statistical life used is peculiar. Why would it be constant? Why is it so low for the US? How did you make it consistent with the climate impacts?

The constant VSL is a good point, and the VSL would in fact likely increase faster than the rate of inflation (which is removed by using constant $2007) as the Willingness-to-Pay would follow GDP per capita, which is assumed to increase by 1.6% per year. Including this increases the valuation associated with the Climate-Health impacts, especially for long-lived gases, but the total valuation per kg is not dramatically affected (e.g. at 3% discounting, CO2 goes from $44 to $56 per kg). Thus this does not alter the primary conclusions of the paper, but it does change values and it reinforces the findings of a substantial discrepancy between the IAM- and WHO-based climate-health analyses. I thank the reviewer for raising this question and will incorporate the trend in VSL in revisions.

The VSL for the US is $7.5 million, which is the US EPA's value based on the mean of 26 peer-reviewed studies (The Benefits and Costs of the Clean Air Act: 1990 - 2010. US EPA Report to Congress (Office of Air and Radiation, Office of Policy, Washington, DC, 1999)). It is not clear why the reviewer believes this is particularly low as no references are given to support that statement. The method used here applies the identical VSL to the climate-health impact calculation based upon the WHO climate-mortality results, and as discussed above, half that result is added to the IAM-based climate damages. As with the response to the prior comment on modifying the CO2 valuation only at the margins, the primary rationale for performing a separate climate-health valuation was to ensure consistency in the methodology between the composition-health and climate-health valuations. Hence these used the same VSLs, populations projections, etc. This point can be further clarified in revisions.

The discount rate is substandard. You can't use a constant money discount rate. You can't have a declining discount rate and uncertainty about the impacts.

I have tried to understand this terse comment, but I cannot. A range of discount rates, reflecting the uncertainty in the appropriate value to use, is applied to future valuations, which seems to be standard practice. While all values are reported in $2007, valuation of specific damages such as Climate increase with time due to growth in GDP and temperature changes, so it's not clear what the reviewer is referring to as constant. I note that the values I obtain for the conventional IAM-based climate damages across the various discount rates are in good agreement with those reported in the US Government Interagency Working Group on the SCC (2013) across those same discount rates, so it does not seem that there is an obvious problem.

The impact uncertainties stem from factors such as the uncertainty in climate-health impacts (based on the aforementioned differences between the WHO-based and IAM-based estimates) and the uncertain human health response to particular pollutant levels (based on epidemiological studies). I do not see how those would be removed in the case of a declining discount rate.