REVIEW

QUESTION 1. Is the contribution made by this paper potentially significant?

In theory this paper should make a significant advance in the literature, because I have not seen a breakdown of the social cost of carbon by sector and/or by region previously. It is therefore has the potential to be a very interesting analysis for the reader. The authors correctly point out the significance of such calculations.

First of all some suggestions for clarification to make the work more readily understandable to a wider readership: it would be useful to explain in words (page 9) how the regional/sectoral breakdown is done (especially given the statement on p 3 about regional interactions). For example, to state in words that the damage Dr, s refer to the damage done to sector s in region r by additional emissions of a ton of C, and that the damage done in a region is the sum of the damages done to the sectors within that region. And also simply that the regional SCC Is considered to reflect the damage done in that region by an additional ton of C emitted (in that region or somewhere else). On page 8, clarify what is meant by a 'business as usual' path and a path with an 'incremental increase in emissions.' Does the latter pathway have greater or smaller emissions than the former? Does the business as usual path correspond to an extract from the widely used SRESA1B scenario?

The authors should also list what affects the SCC in each region: for example, the climate change damages experienced in the region, the population, and the GDP therein. Sensitivity analysis is carried out for population and GDP through the use of different SRES scenarios.

Question 2: Is the Analysis Correct?

Unfortunately, I have identified a number of significant problems with the calculations.

1. A regional breakdown of the social costs of carbon is dependent on a good regional breakdown of the simulated climate change impacts, and a good representation of climate change impacts in the model generally. Although the authors give as a caveat to the work that 'SCC impacts are only as good as the impacts research literature', unfortunately, the damage functions used in the study do not take advantage of recent impacts literature. Most of the damage functions used, including those for agriculture, one of the key sectors, are described in a 2002 paper which itself refers to publications at earlier dates. Hence, the last decade and a half of research in impacts modelling is not reflected in these damage functions. For example, the agricultural damage functions are based on Darwin et al 1995, Reilly et al 1994 and Rosenzweig & Parry (1994).

   Although Darwin et al did not include CO2 fertilisation, according to Tol (2002) the results are scaled to include it, so that a single level of strong CO2 fertilisation is used in FUND, based on these three publications via a regression of the results of the three studies with CO2 fertilisation included. Since these were published, there has been a debate as to whether CO2 fertilisation will actually occur in practise, whether

simulations under controlled conditions are really representative of the field, and whether benefits of fertilisation will be offset by concurrent damages from increases in tropospheric ozone and increases in the ranges of pest species as climate changes.

Further, the authors therefore need to state clearly how, for example, the agricultural damage functions were calibrated to simulate impacts in each specific region. Whilst it is possible to extract tables of region specific parameters such as optimal temperature for agriculture for the different regions from Tol (2002) and references therein, because these values have a critical influence on the results of this paper, the reader should not be left to try to trace how region specific calibration was carried out using older papers. The paper lacks a detailed section on how regionally specific parameters were obtained for each region in each sector. Ideally the parameters should be available in supplementary on line information, giving references. It would not be sufficient to present the region specific parameters without explaining how they were derived or providing citations.

2. On page 4, the authors state that the physical and monetized impacts of climate change tend to be misrepresented in the first few decades of the model runs. Since the values of climate change impacts are discounted, it is precisely these values which most strongly affect the social cost of carbon. Therefore, it would seem that except in the sensitivity study with the very low discount rate, the climate change damages simulated in this paper are in fact misrepresented.

   I therefore recommend that the authors redesign the model to correctly present climate change impacts in the next few decades before continuing to present calculations of the social cost of carbon.

3. Van Vuuren et al (2011) and Warren et al (2011) both highlighted issues with the representation of climate change in IAMs including FUND. For example the latter publication notes that the climatic effects of incremental reductions in carbon are underestimated relative to recent IPCC projections. Such climatic effects are key to the projection of social costs of carbon. Whilst an updated version of the model is used in this study (FUND3.5), so these issues may have been addressed, it is important to state in this publication whether they have been, especially since the description of the climate model seems very similar to that of the previous version. The authors refer to an ESRI report (Tol 2009) which explains that the model has been updated to include climate feedbacks that were not previously included, by modifying the carbon cycle, details of which are given in Table 1 of that report, but the issue is sufficiently important that information on the update should be summarised here. This change is certainly a great improvement, as previously the model di d not include carbon cycle feedbacks. However it would also be useful to clarify whether the updating of the parameters completely addresses the issues raised by the two publications. For example, the statement that there is a calibration of sea level rise and temperature to the IS92a scenario suggests that there might still be further updating required to fully address these issues.

4. The uncertainty analysis is not sufficient to convince the reader that the relative differences in the SCC between regions is robust.

   The authors are to be commended on their thorough exploration of the sensitivity of the results to some of the key drivers of uncertainty in calculations of the social cost of carbon, specifically, the discount rate, the inclusion or not of equity weighting and the climate sensitivity.

   However, the sensitivity of the results to different representations of the climate change damage functions in the model is not explored. At the end of the paper the authors do state that the uncertainty analysis is only partial, and that estimates are sensitive to the functional form of the damage functions. Hence the omission of this sensitivity analysis might be acceptable if it were not for the fact that a central aim of this paper is to explore the differences between sectors and regions.

   The issue is that there is no uncertainty analysis of the regional differences between damage functions, so it is difficult for the reader to know whether these inter-region differences in damage functions, which presumably might be quite uncertain, would affect the inter-region SCC results presented, given the difficulty of calibrating damage functions to different regions. This is against a background of lack of clarity as to how the regional differences in damage functions are generated in the first place.

   Damage functions will also be very different depending on the amount of adaptation that is assumed to occur. Adaptation is not cost-free, so the ability to adapt and therefore reduce damages, suggests that damages avoided by adaptation still contribute to a social cost of carbon. For example, in the agricultural sector, FUND is based upon a calculation of what the projections of the three aforementioned studies would have been with and without adaptation. The case with adaptation is the one included in FUND.

   For these reasons, a key sensitivity analysis to perform would be to repeat the model experiments without adaptation assumed and without CO2 fertilisation assumed, so that a fuller range of potential agricultural damage is simulated. The damage functions also need to be updated to reflect the range of projections of agricultural impacts available in IPCC (2007).

5. It is good that the model now includes the impacts of storms, and that the authors are aware that large scale changes in the earth system are not included. As the authors state, whilst current knowledge does not permit us to detect an additional risk for these events from one incremental ton of carbon, one could also calculate this by considering the effect of an additional billion tons of carbon and then dividing by a billion. The lack of ability to detect a change in probability from an incremental is not a valid reason for omitting these effects from estimates of the social cost of carbon. Hence, it would be

better if the authors simply stated that these effects are left out and that this might mean that social costs of carbon are significantly higher than in the estimates presented.

Other comments

When the results are presented, the major role of air conditioning in contributing to the social cost of carbon needs some discussion. To my knowledge this has not been highlighted as a major issue in reviews of climate change impacts, which have generally highlighted the damages from climate change on agriculture, hydrology and coastal systems, as well as damages from extreme weather events. Whilst the issue is certainly mentioned in previous literature, the seemingly extraordinarily large contribution made by this impact requires some explanation and comparison with previous work. If calculations of the social cost of carbon are really driven so strongly by this, this leads one to question whether the monetization scheme used to value the impacts is appropriate, since policy makers considering justification for acting on climate change in physical terms would perhaps be unlikely to list the need to install more air conditioning high on their list of problems. Also, this large additional use of energy would be likely to result in further emissions of greenhouse gases and hence in turn further climate change impacts.

The authors should also point out that there is a growing understanding of complex relationships between climate change impacts in different sectors and different regions. Lack of inclusion of these knock-on effects leads to underestimation of social costs of carbon, which might be very large for large climate change, and in areas which are hotspots of climate change impacts in a number of sectors. The treatment of impacts in ecosystems is partial, and the statement that the value of the impact is more important than the impact itself should be removed. Since this paper only simulates the monetized value of impacts, clearly only that is considered here. What the ecosystem analysis does not include is the role of ecosystem services, which interacts with all the other impacts, and this is very difficult to simulate. These services comprise for example, the cleaning of air and water by forests and marshes, the stabilisation of watersheds by vegetation cover, the provision of pollinators for crops, and the provision of resources for medicine development by biodiversity. This is an example of interaction between climate change impacts on species and other impacts on humans in other sectors.