(i) Is the contribution of the paper potentially significant?

The results shown in the figures are potentially interesting and useful. However, I am afraid that I cannot support the significance of this study in the form of current manuscript (due to many problems discussed below).

First, in my view the paper is not well placed in the context of previous studies and does not pay a proper level of attention to the persisting discussion surrounding the GWP and alternative metrics. The current manuscript may be misleading to many readers because the paper gives an impression that the global damage potential (GDP) is the sole alternative to GWP. The paper does mention the price ratio – however, this is not sufficient, given the ongoing discussion on global temperature change potential (GTP), which is ignored in this paper. I would suggest that the paper adds a paragraph or several sentences to touch briefly on other alternatives (see review papers such as [*Fuglestvedt et al.*, 2003; *Shine*, 2009; *Tanaka et al.*, 2010]).

This study essentially explores the sensitivity of GDP to several different assumptions in the model. There has been similar (and more comprehensive) attempts for GWP and GTP (e.g. [*Reisinger et al.*, 2010]). Results are not directly comparable, and a probabilistic approach is not possible for GDP, but the scope is similar. These related papers exploring the uncertainty and sensitivity of the metrics can be folded in the discussion of this paper.

In addition, I am questioning the relevancy of the GDP in the policy discussion because the current global climate mitigation policy is addressed under the cost-effectiveness approach rather than the cost-benefit approach. For example, UNFCCC states "policies and measures ... should be cost-effective... To achieve this, such policies and measures should ... be comprehensive, cover all relevant sources, sinks and reservoirs of greenhouse gases" (Rio, 1992) and "consider the establishment... of one or more market-based mechanisms to enhance the cost-effectiveness of, and to promote, mitigation actions" (Cancun, 2010). Both the GDP and the GWP fall into the cost-effectiveness framework [*Tol et al.*, 2008]. This contradiction should not prevent one from conducting research related to the GDP and GWP, but it would be useful if the authors elaborate why the concept under the cost-benefit framework is still worthwhile under the current policy circumstances.

Furthermore, I noticed that three similarly titled papers with the same authors (ordered differently) are uploaded to this journal website ("Regional and Sectoral Estimates of the Social Cost of Carbon: An Application of FUND" and "The Time Evolution of the Social Cost of Carbon: An Application of Fund"). It would be helpful if the authors clarify what the actual contributions of this particular paper are by directly citing and discussing these seemingly related papers.

(ii) Is the analysis correct?

It is not possible to conclude that the analysis is performed correctly because the methodology is not sufficiently described in the current manuscript.

For example, it is not very obvious from equation (4) how the CO₂ fertilization is changed in

the model. The paper states that the carbon cycle model is based on the five-box model [*Maier-Reimer and Hasselmann*, 1987], which is tuned to a more complex process-based carbon cycle model. Some degree of CO_2 fertilization is assumed as the result of this tuning, which is the standard setup in this paper, I believe. Then the paper discusses a case without CO_2 fertilization without explaining what has exactly been done to the model. In equation (4), how the CO_2 fertilization effect is separated from the rest of the model response? In other words, how are the functions *f* and *g* obtained? Is the CO_2 fertilization adjusted through the so-called beta factor? If so, what value is the beta when the authors remove the CO_2 fertilization effect. The same comment applies to the analysis involving the die back of tropical forests – it is not clearly stated how the die back of tropical forest is introduced to the model.

Due to the significant lack of clarity in the methodology of the current manuscript, I cannot review the results section and beyond with confidence. However, I should note that, if the analysis is better described and the comments here are properly addressed, the paper is potentially an interesting contribution.

More specific comments

Page 1, Abstract

- The paper analyzes the sensitivity of GDP to several different assumptions in the model (CO₂ fertilization, climate sensitivity, etc). I think that being more explicit about the scope of tis paper would be helpful for the readers.
- It is not clear what does it mean by "leaving out carbon dioxide fertilization". Does this indicate that the terrestrial biological production is assumed to be not sensitive to the atmospheric CO₂ concentration? If that is the case, I would think it an extreme assumption in view of other carbon cycle models, even though the CO₂ fertilization is a major uncertainty in the global carbon cycle. Why does the author test such an extreme case rather than some weak, standard, and strong CO₂ fertilization effect?

Page 3, Paragraph 2

• The paper states "... our understanding of the impacts of climate change has changed dramatically. We therefore revisit the empirical estimates of the global damage potential of..." It is not clear to me how the recent knowledge on the climate impact has been changed dramatically, necessitating a revision of the previous estimates of global damage potential. Please cite a few review papers to support this statement.

Page 4, Paragraph 1

- Rose (2010) is cited in the text but missing in the reference list.
- Although the CO₂ fertilization is central to the paper, it is not touched upon in the introduction. The paper needs to clarify what the CO₂ fertilization is (I am not sure how the general readers of

Page 6, Paragraph 2

• The computation method of CH_4 and N_2O needs references.

the authors bring a focus on the CO₂ fertilization?

Page 6, Paragraph 3

- How is the e-folding time of 66 years for the temperature response derived?
- The sea level calculation needs references. Where is the e-folding time of 500 years derived?

Page 7, Paragraph 2

• The paper states "The value of a statistical life is set to be 200 times the annual per capita." The results reported in this paper are sensitive to the assumption made for the human life. This is a controversial assumption – I suggest that some relevant papers be cited to better guide the readers.

Page 9, Paragraph 2

- Please fix "SCC cost of carbon". Has SCC been introduced before? If not, please define.
- The period of slightly higher emissions is not consistent between the text and the equation (1). Please fix this problem.

References

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metrics: CO2 and CH4, *Geophys. Res. Lett.*, *37*(14), L14707.

Shine, K. (2009), The global warming potential—the need for an interdisciplinary retrial, *Climatic Change*, *96*(4), 467-472.

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