Comments on The Marginal Damage Costs of Different Greenhouse Gases: An Application of FUND

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The paper is well written and tackles an important topic. While CO_2 is the GHG of primary concern, decision makers will undoubtedly consider promulgating regulations that will have an affect on the emissions of other GHGs such as CH_4 and N_2O . This paper has the potential to provide a valuable contribution by informing the discussion regarding the relative social value of such potential policies. However, the model utilized in this analyses seems to contain a number of components which are based on outdated science, which I address in my comments below. With an update of these components this paper would provide a valuable contribution and I would strongly recommend its publication. Without such an update the results are far less informative as the potential effect of omitting recent scientific developments is unknown.

1 General Comments

• The atmospheric methane concentration, M_t , is modeled as a simple mean reverting process of the form

$$\Delta M_t = \beta \left(M_{pre} - M_{t-1} \right) + \alpha E_t,$$

where E_t are current year's emissions and β defines the rate of decay. In the FUND model β is a constant drawn from a triangular distribution. However, the net atmospheric lifetime of methane represented through β will vary with abundance of tropospheric OH, which in turn varies with gases such as CH₄, NO_X, CO, etc along with the temperature (Ehhalt et al., 2001). Therefore the decay rate of methane will not only be dependent upon its own level but also dependent upon assumptions regarding the emissions of other relevant gases in the reference scenario. These characteristics have been previously built into simple climate models utilizing one box geometric decay for atmospheric methane (e.g., Meinshausen et al. (2008)). This makes me wonder 1.) what the authors' justification is for leaving out these well documented characteristics that others have been incorporating into simple climate models for over a decade, and 2.) what impact this omission has on the paper's results.

- Similar concerns can be raised about the the modeling of N₂O concentrations in the atmosphere. Specifically the IPCC reports a small but systematic effect of N₂O concentrations on its own lifetime (Ehhalt et al., 2001). Also of potential importance is the fact that it takes roughly three years for the N₂O to become well mixed in the middle stratosphere before its decay begins. While potentially small, such effects are easily incorporated into simple climate models (e.g., Meinshausen et al. (2008)) and their effect could easily be examined/incorporated into this paper.
- The paper focuses heavily on the impact of the CO₂ fertilization effect within the agriculture sector and its impact on the results of the study. According to the referenced documentation on the FUND model version 3.5, the parameters characterizing this component are calibrated by considering the difference between a series of studies using CGE models with and without the effect of CO₂ fertilization. The studies used have publication dates of 1992 to 1996, making the underlying crop science included within those models is on the order of 15-20 years old. If this part of the model is going to underlie a large part of the discussion in the paper I would strongly suggest that the authors' consider how their results would change if they incorporated the results from the large

body of research that has been conducted during this time. Are these results consistent with new research based on FACE experiments (e.g., Long et al. (2006) or Tubiello et al. (2006))? This is an area where either an update of the model or justification for why an update is not required is critical.

• The comparison of the current paper's results to those of previous studies, such as in Figures 4 and 7 appear inappropriate. The current paper considers the social cost of marginal CO₂, CH₄, and N₂O emissions in the time frame of 2010-2019. However, of the previous studies listed in the text (page 15) only those by Fankhauser (1994) and Hammitt et al. (1996) provide estimates for emissions within the years studied in this paper. The other studies listed consider emission perturbations up to 15 years earlier. Given that the growth rate of marginal damage estimates (which are endogenous to each model) will differ for various gases, the global damage potential will change depending on the year of analysis as was shown by Marten and Newbold (2011). The reasons for the differing growth rates are even eluded to by the authors themselves on page 18.

2 Specific Comments

- Page 4: You imply that decision makers have used CO₂ equivalents valued by the SCC to value non-CO₂ emission reductions and provide two EPA citations. I am not aware of any EPA action in which non-CO₂ GHGs were valued explicitly using GWPs and I am unable to check your source since the full references for these EPA documents are not included in the bibliography. If these documents don't include an explicit use of GWPs and the SCC to value non-CO₂GHG emissions I would suggest rewording.
- Page 5: The results will be dependent upon the choice of socio-economic-emissions scenario. Therefore I wonder why the authors would use a scenario from the EMF 14 exercise which is about 15 years old. Newer scenarios may be better able to capture important unexpected events during that period including the recent economic downturn, cost effective extraction of natural gas from massive shale gas reserves, technology developments, etc.
- Page 5: It is stated that the EMF 14 based scenario is extrapolated from years 2100 to 3000. The FUND documentation which the paper refers to describes how the scenario was extended past 2300, but does not provide information about the extrapolation between 2100 and 2300.
- Page 6: It is stated that the sea level rise component is calibrated to the IS92a scenario. Given the substantial amount of research that has taken place since then, including the use of satellite observations from the mid-1990s on, I wonder how in line this calibration is with the more recent scientific literature. It appears that the more recent literature may be diverging from the rates previously reported.
- Page 9: The definition of δ in equation (1) should read "for $2010 \le t < 2020$."
- The use of SCC_i to denote the social cost of GHG *i* is confusing, given that SCC is an acronym for the "Social Cost of Carbon". The use of a term like SC-CH₄ or SCCH₄ would make the paper much easier to follow.
- Page 10: The variable GDP has been previously used in the paper (e.g., page 7) as an acronym for global domestic product. To avoid confusion the authors should type out the term global domestic product when necessary instead of using the acronym.
- Page 12: I am curious as to why the authors use a fixed value for the equilibrium climate sensitivity in the base case. The model defines many of its parameters by probability distributions and is runs probabilistically. Given that using a distribution over equilibrium climate sensitivity has become fairly common when computing the expected SCC, in order to capture the high level of uncertainty regarding this parameter, why would the authors chose a fixed value?
- Page 15: It is stated that the studies to which the current papers results are compared do not include CO₂ fertilization in any form. However, at least one, Reilly and Richards (1993), was calibrated with CO₂ fertilization. See Reilly and Richards (1993) Table II note. I would suggest checking the others as well.

References

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