

Interactive comment on “Community Climate Simulations to assess avoided impacts in 1.5 °C and 2 °C futures” by Benjamin M. Sanderson et al.

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Thanks to the reviewer for the thoughtful comments. We respond to each individual point below:

1) I start with a broad comment related to the interpretation of the results. The paper ends with the statement stressing the differences in impacts between 1.5C to the 2C levels: “Irrespective of feasibility, these simulations indicate that a relaxation of ambition from the 1.5C to the 2C level would result in significantly greater impacts at the global scale, in the tropics and at high latitudes.” The abstract also highlights the differences, rather than the similarities: “Exceedance of historical record temperature occurs with 60 percent greater frequency in the 2C climate than in a 1.5C climate aggregated glob-

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ally, and with twice the frequency in equatorial and arid regions. Extreme precipitation intensity is statistically significantly higher in a 2.0C climate than a 1.5C climate in several regions. The model exhibits large differences in the Arctic which is ice-free with a frequency of 1 in 3 years in the 2.0C scenario, and only 1 in 40 years in the 1.5C scenario.”

I take issue with the direction of argument, which is somewhat implicit in this paper. The paper makes me wonder what are the motivations. It is perhaps too broad to raise this here, but given the upcoming IPCC Special Report on Global Warming of 1.5C, are we as a community in charge of concluding urgently that there are discernable differences in impacts between 1.5 and 2C warming levels? The reason why I am raising this is that my overall impression of the results is drawn more toward the similarities. Visual inspection of the series of results certainly shows that there are significant (but not drastic, except for the sea ice (Fig. 1)) differences for various metrics (e.g. extreme precipitation (Fig. 10)) at the global mean level. But when it comes to regional and grid levels, differences are generally obscured by spatial and temporal variability as indicated by overlapping uncertainty ranges (just like any other global climate projections). In other words, similarities are more dominant than differences in my eyes. If there were multiple models performing the simulations, regional differences could be even less tantalizing. As a suggestion, I would think it is worth pointing out the similarities, not just the differences, at the abstract level. If the authors wish to bring forward only the differences, I would suggest that the basis of judgement be clarified to substantiate the claim.

Thanks for this point, which is well taken. We have weakened the language of the abstract as follows: “precipitation intensity is statistically significantly higher ... in some specific regions (but not all). “ and “Significance of impact differences with respect to multi-model variability is not assessed.” We have also added caveats to our concluding comments as follows:

“Irrespective of feasibility, these simulations indicate that a relaxation of ambition from

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the 1.5C to the 2C level would result in significantly greater impacts in some regions, at least compared with internal variability in CESM. Further study should consider these results in a multi-model context, using HAPPI and pattern scaling work together with these coupled single model experiments to produce a comprehensive assessment of avoided impacts in high mitigation scenarios.”

2) In my view, comparisons between 1.5degNE and 1.5degOS results are worthy of more discussion especially in the final section of the paper because it informs what the overshoot means in the context of 1.5C stabilization. It is unclear how the Paris Agreement would deal with an overshoot from the Agreement text. But, given the closing door for the 1.5C target as pointed out in this paper (page 15, line 5), possibilities of overshooting the target before achieving it are ever more relevant. As far as I am aware, implications of overshoot in the context of 1.5C target are not specifically analyzed in previous studies (e.g. (Rogelj et al. 2015)). I think a more dedicated discussion on the comparison between 1.5degNE and 1.5degOS results would thus be useful.

Good point, thanks. We’ve added the following to the Conclusions: “Our study considered two mechanisms to achieve 1.5C, one which stabilizes by the mid 21st century, while the other overshoots reaching 1.7C in 2050 and stabilizes at 1.5C by 2100. Although the focus of the impact studies considered here have compared the equilibrium states at 1.5 and 2C, our scenarios allow a consideration of additional impacts which the overshoot would imply. In 2050, an additional 10 percent of global land area would be expected to exceed historical summer temperature records in the 1.5C overshoot, compared with the 1.5C stabilization case - although differences are not significant at the gridcell level. Our results do not suggest significant differences in sea level rise between the 1.5C overshoot case and the stabilization case and ice-free Arctic summers are simulated to be rare in both scenarios. Our analysis not suggest any evidence of long term climatic difference post-2100 of the overshoot relative to the stabilization case.”

3) Fig 1 shows that significantly negative CO2 emissions (about -2 GtC/yr in average)

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for more than 50 years (1.5degNE case) do not lead to a decline in the global-mean temperature. It is a removal of roughly 100 GtC from the atmosphere. I think this appears at odd with the rule of thumb that the stabilization level is determined by the cumulative CO2 emissions (Allen et al. 2009). Is there any explanation or perhaps some references that help clarify this temperature response?

We're working on a paper to talk about this exact issue. But in short, we don't think that the Cumulative emissions rule of thumb is useful in a highly negative scenario. To show this convincingly requires a large ensemble of models with a range of carbon cycle responses - which we're in the process of performing.

4) While the carbon in the land surface (as C sub l) is shown in Fig A1, it does not seem to be the case from the text that the land carbon cycle itself is explicitly modeled. Only the climate-land carbon cycle feedback is provided without being linked to the land carbon mass (Equation (A2)).

Furthermore, in many simple climate models, CO2 fertilization effect is modeled as a logarithmic function of the fractional increase of atmospheric CO2 concentration from preindustrial level (e.g. see equation (2.1.50) in page 28 of (Tanaka et al. 2007)). On the other hand, Equation (A2) indicates that CO2 fertilization effect is not a function of atmospheric CO2 concentration. These points need to be clarified because applicable ranges of this model may be limited to low scenarios because of the treatment of carbon cycle-related feedbacks.

It is correct that the land carbon cycle pool does not scale the carbon cycle feedback in this version of the model, but it does include a simple linear representation of CO2 fertilization, following Friedingstein 2003 (but as a prognostic model, rather than diagnostic). The ocean carbon model is simple, temperature dependent diffusive (i.e. not using the Friedingstein formulation. We add this caveat. "Note that the land carbon cycle feedback is not a function of the land carbon pool in this version, but is modeled using the parameters of Friedlingstein, 2003, where land carbon uptake is governed

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linearly by atmospheric CO₂ content and temperature."

5) The paper says in page 3 "Our main design choice was to minimize the number of its degrees of freedom to allow for fast calibration to reproduce the global mean trajectory of any given GCM." But when I look at the number of parameters, especially those for CH₄ and N₂O, I must say it is not really a model of minimal complexity. As some of the co-authors are aware, I developed a simple climate model (Tanaka et al. 2007; Tanaka et al. 2009), which I consider simple but not minimal at all. Even my model has less tunable parameters for CH₄ and N₂O (Table 3.2 of (Tanaka et al. 2007)). But this is just a naming issue, not a scientific one.

The point is well taken. The non-CO₂ portion of the model is based on a prior study, and we agree is not maximally simple. We have changed the sentence to simply read: "We have provided a simple multi-gas climate model to perform this emulation,"

Nevertheless, I do not understand some of the parameters in Table B1. For instance, the present-day growth rates for CH₄ and N₂O (ppb/a) and the present-day concentration of N₂O should be model outputs, rather than model parameters because it is stated in page 17 lines 10-11: "The inputs to MiCES are global total emissions of greenhouse gas emissions (CO₂, CH₄, N₂O, CFCs, HCFCs, CO)." This requires a clarification.

The CH₄ and N₂O models are from Prather (2012), which allows for bias correction in present day growth rates. We've added the following to clarify this: "The source code for MiCES is included in the supplementary material of this paper. Non-CO₂ forcings $F(t)$ are calculated using the atmospheric chemistry model defined and published in Prather (2012), which calculates the lifetimes and radiative forcings of non-CO₂ atmospheric components (CH₄, N₂O, HCFCs, CFCs), the model includes some bias correction for present day concentrations and growth rates. "

Technical comments:

Appendix A The notation for the conversion factor between ocean carbon content in Pg

and ocean carbon concentration is not consistent. It is rho in some places but rho sub o in other places. Fixed

Page 15: Line 25 The sentence is unfinished. Fixed

Page 15: Equation (A1) One of the brackets is not closed. Fixed

Page 16: Line 18 Perhaps “due to” instead of “due”? Fixed

Please also note the supplement to this comment:

<https://www.earth-syst-dynam-discuss.net/esd-2017-42/esd-2017-42-AC3-supplement.pdf>

Interactive comment on Earth Syst. Dynam. Discuss., <https://doi.org/10.5194/esd-2017-42>, 2017.

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