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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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The paper provides a first assessment of impact-relevant climate change at the 1.5 and 2°C warming levels based on an earth system model CESM. The authors describe and discuss the results of model simulations specifically designed to analyze these two temperature goals in the stabilization context of the Paris Agreement. They present various facets of climate change under 1.5 and 2°C stabilizations, including mean temperature, extreme temperature, mean precipitation, extreme precipitation, sea level rise, and sea ice. Among several significant differences between the 1.5 and 2°C cases that are identified, the most drastic is the probability of September ice-free Arctic. Furthermore, they develop and apply a simple climate model to calculate inversely emissions scenarios that lead to desired temperature stabilization goals. Obviously, lots of efforts have been put into this paper. The paper is very clearly written and the results are also

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Discussion paper

clearly presented. I think it would be one of the key papers informing the debates on the 1.5 and 2°C targets. I have several minor comments as laid out below. If these are sufficiently addressed, I would formally support publication of this paper in Earth System Dynamics.

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 1.5°C to the 2°C levels: "Irrespective of feasibility, these simulations indicate that a relaxation of ambition from the 1.5°C to the 2°C 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 2°C climate than in a 1.5°C climate aggregated globally, and with twice the frequency in equatorial and arid regions. Extreme precipitation intensity is statistically significantly higher in a 2.0°C climate than a 1.5°C 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 2.0°C scenario, and only 1 in 40 years in the 1.5°C 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 1.5°C, are we as a community in charge of concluding urgently that there are discernable differences in impacts between 1.5 and 2°C 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.

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 1.5°C stabilization. It is unclear how the Paris Agreement would deal with an overshoot from the Agreement text. But, given the closing door for the 1.5°C 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 1.5°C 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.

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

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.

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. 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.

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.

Page 15: Line 25 The sentence is unfinished.

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

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

References

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