

Interactive comment on “The “NorESM1-Happi” used for evaluating differences between a global warming of 1.5 °C and 2 °C, and the role of Arctic Amplification” by Trond Iversen et al.

Anonymous Referee #1

Received and published: 18 January 2018

This manuscript evaluates the quality of climate simulations with the NorESM1-Happi model, which is a slightly modified version of NorESM1-M (which was used in CMIP5). In addition, the authors discuss simulations with NorESM1-Happi that attempt to quantify the differential climate impacts between 1.5 and 2.0 degree global mean warming. Finally, the authors attempt to make inferences on the importance of coupled atmosphere-ocean-sea ice feedbacks in 1.5 and 2.0 degree global mean warming worlds, by comparison of AGCM-only and slab-ocean (SO) versions of NorESM1-Happi. Unfortunately I have major concerns and cannot recommend publication of this manuscript at this time, due to lack of clarity, lack of focus and the subject matter being potentially out of scope for ESD.

C1

General comments:

1) While the focus of the manuscript is claimed to be the differential climate impacts of 1.5 and 2.0 degree global mean warming, it feels more like a model evaluation paper. Most text and figures are dedicated to the evaluation of NorESM1-Happi compared to the CMIP5 model NorESM1-M. While this is a useful and necessary exercise, I am not sure if ESD is the right platform to report on this.

2) Often times, there is lack of clarity in the text. Perhaps the first-author is not a native English speaker, but this should be addressed as it makes it challenging to follow the discussion. Examples: p.3 l. 34 ('AA has been calculated with a strong response?'), p.11 l. 11 ('in reality?'), p. 11, l. 24 ('inaccurately calculated': was the calculation inaccurate or wrong?), p. 13, l. 6 ('calculated time-developments'), p. 14, l. 23 and Fig. 9 legend ('extension' → 'edge'), p. 17 l. 29 ('describe' → reproduce?). Section 5.2 is really hard to follow: p. 22, l. 10 ('enhanced with the SO model compared to the AMIP': not sure what this means), p. 22, l. 11 ('latter': not sure what this refers to). p. 22 l. 15 (probably the tendency...: grammatically incorrect). p. 22, l 17-18 (unclear)

3) The presentation should be improved. There are way too many numbers listed in the tables (only a small fraction is discussed in text). This is overwhelming and makes it hard for the reader to know which are the most relevant ones. I also suggest to have a more consistent lay-out: in evaluation of NorESM-happi, sometimes NorESM-M results are included, and sometimes not (e.g. Figs. 10 and 12). I suggest to always include NorESM-M results for consistency. Also, the labelling in Fig. 13 is confusing, the x-axis in Fig. 14 is not legible (too small), and the different rcp's in Fig. 17b should have distinct colours.

4) From the text I cannot derive what the major scientific advance is from the comparison between the AMIP and SO results. Aren't the SO sea ice and SST fields meant to mimic the AMIP sea ice and SST fields? Does the fact that there are differences between the SO and AMIP sea ice and SST fields mean that the 'target' SST and sea

C2

ice fields are not achieved in the SO runs? I'm concerned that this mismatch prevent a clean comparison of the climate impacts in the SO model versus the AMIP runs.

Other comments:

- 1) p. 3, l. 5: I'm not familiar with the term 'temperature ceiling', and find it a bit misleading. Better to use 'temperature target', as the temperature references in the Paris Agreement are generally believed to apply to long-term averages, and not to the maximum as the word 'ceiling' might suggest (see: doi: 10.1002/2017GL075612)
- 2) Fig. 1: For a cleaner comparison between models and observations I suggest to sample the model only at the locations where and times when observations were made.
- 3) p. 7, line 9: Do you really mean Fig. 11?
- 4) p. 8, l. 6-8: 'changes in the major elements ...': unclear, please elaborate.
- 5) p. 10, l. 21: what is the ACCESS version of the model? Please keep naming conventions/references to model versions consistent to avoid confusion
- 6) p. 12: maybe I missed it but did you discuss the trends in the AMOC and Drake Passage transport? These seem to be the quantities with the largest drift
- 7) p. 20, l. 21-22: Very confusing to first list the numbers in the brackets and not mentioning what they mean until 5-10 sentences thereafter.
- 8) p. 21, l. 1-10: I don't think this discussion is accurate. Sanderson et al. (2017) used an emulator to construct emissions pathways that would lead to a 1.5 and 2.0 degree warmer world. Those emissions pathways were then prescribed to a coupled atmosphere-ocean model. This is not a 'simplified method' as the authors suggest. Also, lines 2-4 are a bit misleading as well. It is now well established that the equilibrium climate response is determined by the accumulated carbon emissions. This implies that, in reality and in climate models that include a carbon cycle, switching off emissions

C3

would lead to a rapid stabilization of global mean surface temperature (the delay in warming associated with ocean thermal inertia would be balanced by a decrease in greenhouse gas concentrations, see e.g. doi:10.1038/ngeo1047). My point is that it may not be as hard as the authors suggest to employ fully coupled atmosphere-ocean models to quantify the climate impacts of 1.5 and 2.0 degree global warming.

- 9) p. 21, l. 13: which 'forcing data', please specify
- 10) p. 21, l. 31: please give evidence that after 45 years a new equilibrium is indeed reached
- 11) p. 22, l. 6: What is meant with 'single projections'?
- 12) p. 22, l. 7: Table 7 does not show that the temperature targets (1.5 and 2.0 degrees above pre-industrial) are hit, but only show the temperatures relative to PD. What are the temperature relative to preindustrial?
- 13) As noted above, I found section 5.2 really hard to follow. What is particularly confusing is p. 22, l. 25-34. I think the sea ice area in the AMIP runs are compared to observations. Questions I have here are: 1) why is this comparison not presented earlier, 2) why are there any differences if the prescribed data is based on observations? 3) why is sea ice extent in Table 4 compared with sea ice area in Table 8?

Interactive comment on Earth Syst. Dynam. Discuss., <https://doi.org/10.5194/esd-2017-115>, 2017.

C4