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Earth System Dynamics
Discussions

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.

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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.

– Overall Response:

We understand the concerns of Referee #1 (as well as Referee #2) about the paper's scope for ESD, as well as the apparent lack of focus. Before we propose how to deal with this, please take into account that the manuscript was actually submitted to be considered for publication in GMD. It was intended for inclusion under the special collection of articles on the Norwegian Earth System Model (NorESM). This paper's version of the NorESM (NorESM1-Happi) is an update of NorESM1-M used for CMIP5. The AGCM-version of the model has been used to contribute to multi-model investigations in the HAPPI project, with AMIP-type experiments that address differences between a 1.5 degrees and a 2.0 degrees warmer world than the pre-industrial (1850). Our intentions with the present paper, when submitting it to GMD, were:

1. to validate the fully coupled NorESM1-Happi which employs the atmosphere and land components that are used in the experiment in the HAPPI project, e.g. by comparing its performance to NorESM1-M, and

2. to apply a slab-ocean version (NorESM1-HappiSO) which includes the sea-ice model from the fully coupled NorESM1-Happi, to investigate how the polar amplification of the temperature signal may change the modelled difference between a 1.5 and a 2.0 degree world. In addition, results from the RCP2.6 and RCP4.5 runs with NorESM1-Happi complement the discussions, although these scenarios are not targeting the two temperature increments per se.

Obviously, the visibility of point (2) has suffered due to the lengthy and detailed discussions over the first 4 sections. Point (1) was emphasized in the manuscript when designing it for GMD rather than ESD. The topic editor of GMD who was assigned for the paper almost a month after the submission, chose to reject the paper for GMD and recommended to transfer it to ESD. With the publication constraints for papers to be

referred to in the scheduled Special Report from IPCC in mind, we chose to follow the recommendation without much discussion, although we suspected that the profile of the manuscript could produce confusion with the ESD editors and reviewers.

Now, with both the referees' comments in mind as well as our own concern about the paper's profile for ESD, we propose to restructure the manuscript as follows, hoping to achieve stronger focus, better clarity, and a more suitable profile for ESD.

– We propose to reduce the amount of material that documents the standard validation of NorESM1-Happi as a global climate model, mainly by compressing much of the information given in the present text into tables, and collect them in a separate set of "supplementary material". A considerably shortened text will still be kept in one sub-section of the main manuscript, where the model design is described. Some validation results that are relevant for the polar amplification topic will be kept in the main text (feedback analysis, extratropical storm-tracks and blocking, sea-ice and aspects concerning the freshwater cycle).

– A clearer presentation of the reasons for running the slab ocean experiments to complement the AMIP-type experiments will be given, and in particular the replacement of a prescribed sea-ice cover in the AMIP-runs with a fully dynamic-thermodynamic sea-ice in the slab ocean model runs.

– We are underway with a selection of simulations with the fully coupled NorESM1-Happi model, which target the 1.5 degree and the 2.0 degrees warmer world (compared to 1850 pre-industrial). This will better complement the AMIP and SO-experiments, with specific focus on polar amplification than the runs based on RCP2.6 and 4.5 that are now included.

All in all, this should considerably strengthen the paper's focus on polar amplification, while still including the necessary results in support of the model validation as a secondary item. With this, we believe the paper will be suitable for ESD, and in particular to be included in the special issue about The Earth system at a global warming of 1.5°C

and 2.0°C (https://www.earth-syst-dynam.net/special_issue909.html).

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 it the right platform to report on this.

– Reply

As mentioned in the overall response, the paper was written for and submitted to GMD, but the assigned topical editor thought it was better suited for ESD. We now realize that the title of the manuscript may have given the GMD topical editor the impression that the paper is not a model evaluation paper (our fault). Since ESD has a special issue on "The Earth system at a global warming of 1.5°C and 2.0°C", we decided to follow the suggestion of the GMD topical editor (even though we did expect that the ESD-referees probably would comment on this).

We are therefore prepared to restructure the paper, and propose to include major parts of the pure model validation in a "supplementary material", reduce its volume, and only keep a shorter discussion in a subsection of the main text. Instead, the discussion of the differences in climate between a 1.5 and a 2.0 degrees warmer world will be more prominent, with the role of polar amplification as the paper's focus.

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:

– Reply: Please see the detailed point-by-point replies below.

We suspect that missing clarity is also a consequence of the hectic situation caused by the deadline for submission on Nov. 1st 2017 (for papers to be referred to in the

IPCC's special report on 1.5 degrees). We will pay considerable attention to the clarity of language when updating the manuscript, and we intend to seek help by a native English speaker before finalization.

p.3 l. 34 ('AA has been calculated with a strong response?),

– Reply: The sentence should read: Even for the remote and regionally localized forcing caused by reduced European sulphate aerosols since the 1980s, the strongest amplification of warming is found in the Arctic (Acosta Navarro et al., 2016).

p.11 l. 11 ('in reality?'),

– Reply: Replace by "already", and in addition modify the entire paragraph to read: NorESM1-Happi is a version of NorESM1-M with relatively minor updates. The most radical difference is a doubling of the horizontal resolution in the atmosphere and land models. As NorESM1-M is already thoroughly documented through CMIP5, this paper presents only selected features of the updated NorESM1-Happi.

p. 11, l. 24 ('inaccurately calculated': was the calculation inaccurate or wrong?),

–Reply: "incorrectly" is the correct phrase. p. 13, l. 6 ('calculated time-developments'), Reply: We propose to amend the sentence (if it will be used in the new manuscript) to become: Fig. 5 shows the simulated time evolution from 1850 to 2100 of some of the quantities in Table 2.

p. 14, l. 23 and Fig. 9 legend ('extension' –> 'edge'),

– Reply: will be changed to "edge". Comment: "ice extent" will replace "ice extension". Unfortunately, in a few places the ice extent has been confused with ice area. Ice area takes into account the fractional ice-cover in any grid cell, while the ice extent measures the size of the entire domain where sea-ice is present. This will be corrected.

The second sentence in the Fig. 9 legend should read:

The solid black line shows the sea-ice edge estimated as the 15% iso-line of monthly

sea-ice concentration in the OSI-SAF reprocessed data set (OSI-SAF, 2017).

p. 17 l. 29 ('describe' –> reproduce?').

– Reply: agreed. "reproduced" is better.

Section 5.2 is really hard to follow:

– Reply: We are sorry for the sloppy and confusing language many places. This section will be re-written and expanded in a new manuscript.

p. 22, l. 10 ('enhanced with the SO model compared to the AMIP': not sure what this means),

– Reply: see next point.

p. 22, l. 11 ('latter': not sure what this refers to).

– Reply to both points: Sorry! Yes, this is confusing. The sentences should read: The PAF is considerably larger in the Arctic than in the Antarctic. Furthermore, the SO model produces stronger AA (by 18%) than the AMIP model.

p.22 l. 15 (probably the tendency...: grammatically incorrect).

– Reply: see nest point. p. 22, l 17-18 (unclear)

– Reply to both points: The following amended text is proposed for the entire paragraph:

The SO model has a tendency to produce a colder winter climate than the AMIP model, consistent with the systematic cold bias in the fully coupled system. The differences in summer climate between the SO and AMIP simulations are much smaller. The SO model generally simulates less reduction in sea ice area than is prescribed in the AMIP simulations, with the exception of the NH summer response for 2.0oC-PD. These differences are also reflected in the temperatures and precipitation over land.

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.

– Reply

The many numbers are there to document basic properties in order to validate the model as a suitable tool for the study of processes in the climate system. We do not explicitly comment on every number in the text, although the overall model properties, e.g. concerning the simulation of a stable pre-industrial climate, climate sensitivity, and climate variability and change, are discussed. Such numbers are essential in a climate model validation. We understand, however, that a full model validation was not to be expected in an ESD-article, or from the title of the paper. We believe this will be considerably improved by moving many of the tables to a "supplementary material".

We think it can be a good idea to include NorESM1-M results in Figs. 10 and 12 as well, to the extent that they bring substantial additional information (there are already many figures). The details mentioned for Figs. 13, 14 and 17b will be taken into account. The figures will in any case be changed when re-organizing the manuscript.

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 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.

– Reply We realize that the description of the differences between the SO-experiments and the AMIP-experiments is insufficient. We will expand the text accordingly as indicated below. The referee is, quite understandably, concerned about the fact that the SO-model simulates sea-ice properties (and therefore also to some extent SSTs) which deviate considerably from the AMIP experiments, even though the calibration of the slab ocean is constructed to mimic the prescribed ocean state used for the AMIP experiments.

Does this imply that the SO-model is profoundly wrong, and is not valid for the intended study of polar amplification?

The reply is no. The explanation follows.

The SO model includes interactive sea-ice and snow processes which are missing in the AMIP-runs. In principle, the mean climatology for the SSTs used in the AMIP experiments should be reproduced by the SO-model experiments, provided the sea-ice properties (cover, thickness, concentration) are the same in the two setups. However, when calibrating the SO-model, we do not directly control the sea-ice properties. We define deep-ocean heat-fluxes (Q) in the SO in order to approximate the SSTs used in the AMIP experiments, but there is no similar relaxation of the sea-ice in the SO model towards the sea-ice which is prescribed in the AMIP runs. The Q-fluxes will indirectly constrain the SO-calculated sea-ice to some extent, but there is no reason why this sea-ice should be identical to the AMIP sea-ice.

We feel that this is a strength of the SO runs compared to the AMIP runs. The sea ice concentration prescribed in the present day AMIP experiment are based on satellite observations, but other properties of the ice and snow cover are more arbitrarily set as described in detail in Mitchell et al. (2017; doi:10.5194/gmd-10-571-2017). For example, the sea-ice thickness is 2 m over the entire Arctic sea ice extent and 1m in Antarctica, and the snow-cover is allowed to become unrealistically thick in many places. We will extend the description of this in an updated manuscript, thus clarifying

why the SO-model with its more realistic feedback processes associated with sea-ice and snow cover, is useful.

The heat isolation and radiation properties of sea-ice thickness and snow cover are particularly important for e.g. the Arctic temperatures over extended winter seasons. In the SO-model, these properties are allowed to vary in accordance with physical processes represented in the sea-ice model, while the sea-ice thickness is constant in the AMIP runs. Hence, the SO-simulation should have a more realistic – or at least more physically based – representation of feedbacks that influence the Arctic amplification in ways not included in the AMIP runs.

Concerning SSTs, the SO-model should produce results close to the prescribed SST-fields used in AMIP, except in regions directly influenced by the sea-ice model results. By construction, the relaxation we use to define the Q-fluxes secures that the SO-model results hit the same global temperature targets as the AMIP runs. Deviations originate from the fact that the atmosphere in the SO-model "sees" a different sea-ice and snow cover than in the AMIP runs.

We will provide a comparison of the simulated sea-ice thickness changes in the 1.5C and 2.0C experiments that are underway with the fully coupled NorESM1-Happi model with those of the SO experiments, although the fully coupled experiments are not bias-corrected for present-day such as the SO-experiments.

– In conclusion, and this will be better presented in an updated manuscript, the results from the SO-model should produce an Arctic amplification of the temperature response which differs from the AMIP-results. The differences are dominated by the changes in the sea-ice and snow cover in the SO-model with some regional influence on the SSTs. Otherwise, the contributions to AA from SST-feedbacks directly, should be included in the SST-fields prescribed for the AMIP experiments.

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)

– Reply: We were concerned with the term "target". The target is in principle an upper bound of the global temperature change, not a target that one wishes to hit. However, if "ceiling" is a misleading word, we can change to "target", while also explaining that an under-shoot should not be considered a miss.

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.

– Reply: In fact, model output was already sampled in grid volumes containing the observation points, and for dates in each of the ten years of the PD-period that coincide with the observation dates. Since the model is run in climate simulation mode without data-assimilation (or nudging), we cannot compare the exact observation times. We will update the figure legend to make this clear. The figure will be moved to the supplementary material section.

3) p. 7, line 9: Do you really mean Fig. 11?

– Reply: No, it should be Fig. 9.

4) p. 8, l. 6-8: 'changes in the major elements ...': unclear, please elaborate.

– Reply: more specifically: GHG, aerosols and land-use

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

– Reply: Sorry about this. This sentence remained from an early version of the

manuscript. It should read:

Before the piControl, the 1-degree version of the atmosphere and land model described in Seland and Debernard (2014) was spun up over 300 years, starting from . . ..

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

– Reply: The trends in both quantities are mentioned on p12, l 3-11, and compared to the trends in the NorESM1-M model. They are not discussed in depth, and we will add a sentence in the manuscript stating that while the apparent drift in those indices indicates that the deep ocean is not in perfect balance, we do not think is a serious issue our study. The magnitude of the trends are comparable to those in NorESM1-M (Bentsen et al, 2013).

Otherwise, the strong AMOC leads to heating of the deep oceans and leaves less energy for surface heating and evaporation. We will add a figure in the planned supplementary material, which documents how heat penetrates into the deep ocean during simulated RCP scenario projections with NorESM1-Happi. On p13, l 14-30, we hypothesize a connection between the strong AMOC and the simulated speed of the atmospheric freshwater cycle, as well as the under-estimated cloud cover that can be related to the horizontal resolution of the atmosphere.

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.

–Reply: Agreed. We will mention this in the beginning of the paragraph.

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.

– Reply: Of course, this is wrongly stated in the paper and will be corrected.

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 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.

– Reply: We appreciate the referee's point. We are now underway with some experiments with the fully coupled NorESM1-Happi model, targeting a 1.5 and a 2.0 degrees warmer world than the model simulated pre-industrial control run for 1850. These experiments are not emission-driven for GHGs, hence we do not base our calculations on the principle from Gillett et al. (2011, doi:10.1038/ngeo1047). Instead, we have designed GHG-based forcing with temperature targets based on amendments to RCP2.6 and 4.5, and our own estimates of the model's climate sensitivity.

We will not have time to produce an ensemble of model projections comparable to that of Sanderson et al (2017), but hopefully around 150 simulation years per temperature target. Statistics will replace those included for the RCP2.6 and RCP4.5 in the present manuscript, and thus better complement those of the SO- and AMIP-experiments.

9) p. 21, l. 13: which 'forcing data', please specify

– Reply: Forcing data here are prescribed atmospheric levels of greenhouse gases (GHGs). Details are given in Mitchell et al (2017).

10) p. 21, l. 31: please give evidence that after 45 years a new equilibrium is indeed reached

– Reply: Below is a figure (Fig.1) which shows the evolution of global mean temperature

in the SO simulations for present day 2006-2015 (PD, black), the 1.5 (1p5, blue), and the 2.0 (2p0, red) degrees warmer world than pre-industrial. The numbering of years are arbitrary.

11) p. 22, l. 6: What is meant with 'single projections'?

– Reply: Single projections means that there is no ensemble of projections (e.g. with ensemble members starting from different initial states), but only one single estimate. We propose to remove "single".

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?

– Reply: This is a consequence of the HAPPI-design (Mitchell et al, 2017), for which calculations are based on present-day (PD; 2006 – 2015) which is estimated independently at 0.8oC above the pre-industrial. Since neither the SO- nor the AMIP-experiments are run explicitly for the pre-industrial situation, we will not present the pre-industrial numbers for these two sets of experiments. For the fully coupled experiments, however, we will add numbers for the pre-industrial situation. 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?

– Reply: There are obvious reasons for a reader to be confused here. In Table 4, sea-ice extent is presented, and not sea-ice area. In winter, the difference between area and extent (area being smaller than extent) may not be considerable, but still the comparison is not accurate. Nevertheless, we believe that the comparison indicates

that there are some errors in the SO-model and probably also in the fully coupled NorESM1-Happi.

The text in the paragraph will be reformulated and its emphasis will be reduced. This should have been done during the quality check before submission, and for this we apologize. âĂČ

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

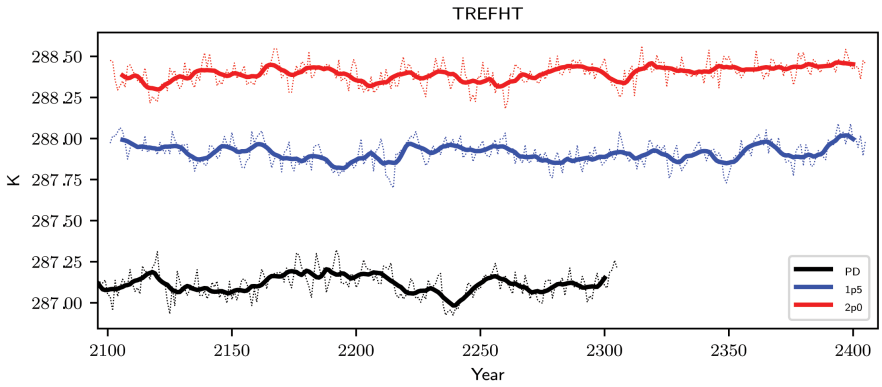

Fig. 1. The evolution of global mean temperature in the SO simulations for present day 2006-2015 (PD, black), the 1.5 (1p5, blue), and the 2.0 (2p0, red) degrees warmer world than pre-industrial. The numberin