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Geoscientific Model Development Discussions

Interactive comment on "The Norwegian Earth System Model, NorESM2 – Evaluation of theCMIP6 DECK and historical simulations" *by* Øyvind Seland et al.

Anonymous Referee #1

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Review of "The Norwegian Earth System Model, NorESM2 – Evaluation of the CMIP6 DECK and historical simulations" by Seland et al.

General Comments: This manuscript documents the NorESM2 model, which is being used in CMIP6. The model is based on the CESM2 model with some notable differences in for instance, the representation of aerosols and their climate interactions; convection parameterizations as well as using different physical ocean and ocean biogeochemistry models. A description of the model and its differences from CESM and CAM6 models are detailed and an overview of the development and tuning of the fully coupled configuration in a pre-industrial climate is given. An assessment of some key

climate responses is provided, including the equilibrium climate sensitivity and transient climate response as well as future climate projections. An overview of the present-day performance of the historical simulations against observations is also provided.

This is a useful description and overview paper of the NorESM2 model and its climate characteristics and will be a very useful reference for the on-going and future CMIP6 analysis work. It is suitable for publication in GMD although I find it in its current form very long with 30 figures. The authors have included a lot in this one manuscript, covering a wide range of the model assessment with the result that the analysis of the individual components feels quite "light touch while I feel it is still missing evaluations of important parts of the fully coupled Earth system. I would recommend publication in GMD after my recommended revisions and additions to the analysis are made which I outline below.

Specific Comments:

There is no evaluation of aerosols (even though this is a key difference from the CESM2 model) or ocean or terrestrial biogeochemistry provided. While the land model is essentially CLM5 and documented elsewhere the authors note the important implications of the updated nitrogen-carbon limitation on the carbon cycle in the model but no assessment of the carbon cycle is provided. The ocean biogeochemistry is a totally new component compared to CESM but again no evaluation of this important component of the ES model is given. Overall given this is an overview documentation paper of NorESM2 I feel an assessment of the global carbon cycle at the very least is warranted.

Similarly for aerosols, I note the authors cite other papers that are in preparation, however again as a top-level documentation paper and the importance of aerosol-climate interactions for the climate response of the model some overview of the performance of aerosols in the model is needed. In particular, both sea salt and DMS emissions have been used to tune the final coupled model but no detail of this tuning nor impact

on the aerosol simulation is provided.

Tuning: More detail and clarity is needed in some aspects of the tuning description. It is evident from Section 3 that a number of variables have been used to tune the low and higher resolution models (LM and MM) but the tunings differ in a number of places between the two resolutions. A table summarizing the primary and secondary tuning parameters as well as the untuned/tuned values chosen for each configuration would be very beneficial. It should include the impact of the tuned values on an appropriate metric which ideally would be constrained by observations eg: RESTROM or the SW cloud forcing in the case of the tuning of gamma parameter (paragraph beginning 266).

The tuning was carried out in a pre-industrial climate , yet the authors set their tuning targets in order "to maintain values of mean atmospheric and ocean temperatures close to observations" (L254) , given that the observations are predominantly in the present-day the authors should comment on the limitations of any such comparison. Were any present-day simulations done in parallel to validate this tuning and evaluation?

You state (Line 224) that present-day year 2000 AMIP timeslices were used for the general development of CAM6-Nor. Given that a continuous year 2000 forcing is not a realistic representation of present-day climate or of observations over recent decades, can the authors comment on the decision to use year 2000 forcing instead of a timeseries forcing and implications this may have on the model development and evaluation.

Overall the different choice of tuning parameters for the two resolutions will impact the models evolution and therefore limits the assessment of the role of resolution on the model performance and any potential benefits of the higher resolution. Can the authors comment on this ?

Line 250: What were the main changes from CESM2 –> CESM2.1? These should be documented, perhaps in Section 2 and here in the Tuning section document the impact of these developments on the tuning and development of NorESM2.

Some sort of schematic or diagram (even if included in an appendix) would be useful to aid understanding of the spin-up process, detailing offline spin-up and fully coupled spin-up. How long was the total spin-up period of the final tuned model before the official piControl for the DECK began? Later in Section 4.1 you state the abrupt 4xCO2 and 1pctCO2 runs were started at year 1 of the control -presumably you mean here the piControl but there's no indication of the full length of spin-up and how you determined that the model was fully spun-up.

Ensemble size: It is very hard to assess the robustness of the NorESM2-MM model given there is only a single historical member used in this analysis. The authors need to acknowledge such limits in the text. Indeed, the historical evolution of global mean surface temperature is outside of the LM model range but its impossible to say if this is meaningful. Furthermore, it's not clear if only single ensemble members were run for the future projections in both models, it looks like this is the case but again it needs to be clearly stated and limitations on conclusions drawn need to be discussed.

It is very interesting how the ECS is so remarkably different in NorESM2 compared to CESM2. Can the authors expand on the detail given here? Have additional sensitivity experiments been done to pick apart the role of the differences between the two models on the response eg, ocean model, aerosol-cloud representation? It seems a very relevant investigation to understand potentially more generally the multi-model differences in ECS, in particular given the tendency of CMIP6 models to move towards higher ECS it's all the more interesting that NorESM2 has gone the other way.

Line 402: What is NorESM1-Happi? Given the prevalence of its usage in the analysis of the climate response and present day performance of the NorESM2, an appropriate reference and a brief description of how it's different from NorESM1 is warranted Overall, I don't get the motivation for including NorESM1-Happi in the analysis and find it often confuses the analysis. . Also, Presumably the NorESM1 models are not driven with the updated CMIP6 forcing for instance?

The final “Summary and Discussion” reads really just solely as a Summary. Can the authors draw some overarching Conclusions from their analysis, which might include for instance the overall improved performance of the NorESM2 models compared to its predecessors; the motivation of 2 resolutions for NorESM2, role of resolution on model performance and potential benefits or applications of the different resolutions to different aspects of wider CMIP6 analysis. The would be beneficial to a reader and future potential user of NorESM2 data.

Length of Paper: This paper is very long. It appears to me that many of the figures are surplus to the requirements of the main thread of the analysis and don't get discussed much in the main text I would recommend moving some of them to a supplementary material. The ones that strike me are Figures 15, 21, 22, and a reduction in the number of ENSO plots (currently Figures 26-30).

Technical Comments:

The Title should reflect that use is made use of ScenarioMIP simulations also.

Line 46: participates –> participate

Line 80: It would be nice to inform the reader why the decision was made not to include land-ice model.

Line115: Please include an appropriate reference for the prescribed optical properties used for the stratospheric aerosol.

line 121: Can the authors quantify the impact of tuning both the sea salt (and later the DMS) on the total emissions of these natural sources and perhaps AOD and cloud droplet numbers? Were the aerosol tunings applied in the same way in both LM and MM?

Sect 2.4: does the dust aerosol impact the iron fertilisation of the ocean?

Line 224: I don't understand what is meant by a “data-atmosphere” for the offline forc-

ing of the ocean components – presumably the atmospheric forcing came from the CAM6-Nor simulations?

Line 246: “towards its climatology” –> towards its own climatology

Line 252: steady –> steady-state?

Line 266: Define the acronym CLUBB

Line 281: “detail here” appears to be inserted here in error. Some quantification of the change in seasalt and DMS emissions should be given.

Line 300: The lower biological production of DMS is said to agree better with observations but leads to an underestimation in DMS emissions. What observations are used here? How do the authors explain this apparent discrepancy?

Line 305: greenhouse gas climate scenarios → greenhouse gas future climate scenarios.

Lines 308âĂŤ310: Please make sure you include the appropriate references here for the CMIP6 DECK and ScenarioMIP experimental protocols.

Line 330: were there any particular criterion used for the choice of initialisation years ?

Line 331: it seems a great shame that there is only one ensemble member of NorESM2-MM included in this analysis and severely limits any conclusions drawn about the performance of this model compared to the LM equivalent. If additional members can be added this would significantly add to the value of the analysis. If not, the limitations of having only 1 ensemble member should be highlighted in the text.

Line 334: future climate development –> future climate change?

Line 338: RCPs – define acronym

Line 354: please define what you mean by a “sufficiently long spin-up”

Line 371: abrupt4xCO2 –> abrupt-4xCO2

Line 420: Does the model have a representation of nitrate aerosol? “it is likely that the aerosol forcing is similar in both model versions” – can you actually make this statement given the different cloud tuning in the two configurations with potential implications for the marine stratocumulus for instance?

Line 473 “sea level is lower” –> sea level anomaly is lower.

Line 488: Fig 9 here I think should refer to Fig 20

Line 489 Fig 10 –> Fig 11

Line 550: North Atlantic –> North American continent (also biases prevalent in south America).

Line 554: “are mitigated”–> are reduced

Figure 14: Why is the LM model so much warmer (at the surface) than MM? Is this a consequence of the different tunings? Its hard to tell from Figure 2 if the net TOA in the piControl is overall warmer in the LM model.

Line 625 “modelled cloud cover” – presumable the 70% here is referring to a global mean value. It would be helpful to refer the reader back to Table 2 here.

Line 626: The reference to Fig 15 seems to be oddly placed in between Fig 19 and Fig 20. I suggest moving the location of the figure.

Line 633: “reanalysis. along”? This whole sentence needs to be corrected as Figure 20 does not use GPCP data.

Figure 20: Either the figure caption of the figure labelling is incorrect here in terms of what model is plotted in what panel, please correct.

Line 695: friquency –> frequency

Line 700: “NorESM-LM (Figure 25(b)) –> this is inconsistent with the Figure caption of Figure 25 which states panel b = NorESM-MM. Please double check all figure captions

to make sure they are correct and consistent with the main text.

Figures 26 – 29 : It feels like there are a disproportionately a lot of figures for Section 5.9. Are all these needed in the main text, can some be moved to supplementary section?

Line 751: “medium-resolution version of the model” – you should clearly state here that the resolution differences here relate to the atmosphere.

Figure 8: March and September lines should me clearly marked on the plots.

Figure 25: In the figure caption the final sentence is incomplete.

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