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Geoscientific Model Development Discussions

Interactive comment on "HIRHAM–NAOSIM 2.0: The upgraded version of the coupled regional atmosphere-ocean-sea ice model for Arctic climate studies" *by* Wolfgang Dorn et al.

Anonymous Referee #2

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The paper presents a new version of the regional coupled model HIRHAM-NAOSIM. The predominate changes are physical improvements in the atmospheric component, increased resolutions, new coupling system and computational efficiency. Results from a set of experiments are outlined for the atmospheric and sea ice components. It is important to document these models as they develop, and thus there are good reasons for GMD to want to publish this paper. However, there are a number of areas in which the manuscript needs to be improved. The paper is well written but could benefit from some clarifications. I recommend the paper is published subject to major revisions.

Major comments: 1. The authors highlight the importance of properly representing

feedbacks between atmosphere, sea ice and ocean in studying climate. They mention only the Arctic amplification as example, that is largely influenced by complex teleconnections with lower latitudes. I would suggest to include a description of limitations of regional modelling in climate studies, together with the advantages (lines 17-23 pg 1). A regional model covering the Arctic region does not have to adequately reproduce the overturning circulation that largely impact the Arctic properties, but the choice of the prescribed boundary conditions is crucial. Here an old and low-resolution climatological data set is used. Add a comment on that, too.

2. The ocean component is a key part of the climate system and largely affects the sea ice model performances. First, I am surprised that a very old version of MOM is used. I suggest to justify this choice and include a more complete description of the ocean component and its setup in the paragraph 2.2. Then, the ocean results are totally missing in the paper - a proper analysis and validation has to be included. Diagnostics from Ilicak et al 2016 (https://doi.org/10.1016/j.ocemod.2016.02.004) and Uotila et al 2018 (https://doi.org/10.1007/s00382-018-4242-z) can be followed.

3. More details on the HN1.2 runs are needed for a clearer understanding of the intercomparison results

4. The manuscript would largely benefit from a detailed analysis of the possible changes in the air-ocean-sea ice interactions

Minor comments:

Section 2.1.2 Pg 4, line 4: 0.25degree corresponds to approximately 27km at the equator. Is this the nominal resolution of the atmospheric grid?

Section 2.1.3 Pg 5, line 10: provide a clearer distinction between the setting of stand-alone mode and the coupled mode Pg 5, lines 11-12: what are the implications of different conditions applied to uncovered sea points?

Section 2.2 Pg 5: I suggest to add details on relevant differences between HRM and

FRM

Section 2.2.1 Pg 5: more details on the ocean components are needed, parameterizations, mesh, bathymetry, etc.

Section 2.2.4 Pg 7, lines 1-2: the description of Arakawa B grid corresponds to the description of C grid in 2.1.2 Pg 7, lines 15-19: a more accurate description of numerical instability is needed. How are the authors sure that the model crashes are all ascribed to the choice of the mixing time step? Pg 7, lines 27-29: as for the atmospheric component, provide a clear distinction between stand-alone and coupled setting, and comment the possible implication of different forcing in coupled and uncoupled sectors of the domain

Section 2.2.6 Pg 8, lines 11-15: add a description on the new parallelization since this is one of the major improvements to the system. Which method has been used? Which component of the system is affected? Only NAOSIM is mentioned. How the impact on stand-alone simulations compares with coupled simulations?

Section 2.3 Pg 8, line 16: motivate the choice of YAC version 1.2, also compared to the previous coupler.

Section 3 Pg 10, line 10: the namelist of ocean and sea ice parameters is missing in the manuscript

Section 3.1 Pg 11, lines 6-14: this is unclear, I suggest to rewrite the entire paragraph adding precise information on the spin-up time for NAOSIM and HIRHAM, and for the coupled HN2.0 runs. The HN1.2 runs follow the same spin-up strategy? "The simulations were driven by ERAI data" refers to the coupled runs? If so, I suggest to reword, "driven" generally is for an ocean-sea ice simulation forced by atmospheric reanalysis.

Section 3.2 Pg 11, line 20: how robust is the validation against ERA Interim since it has been used for the initialization? Why not to use independent data? Pg 11, line 22: reword "quasi realistic" Pg 11, lines 27-28: clear statements on the differences and sim-

ilarities between HN2.0 and HN1.2 would help, in addition to Graham et al 2017. I do not understand the sentence "Differences in the simulation results . . . indicate changes in the model performance". There are many differences between the 2 versions and probably between model set-up and spin-up. Please clarify.

Section 3.3 Pg 12, lines 1-3: a good representation of sea ice properties does not guarantee a good representation of ocean and atmospheric fields. I think that assessing the quality of ocean/ atmosphere components would largely improve the manuscript. For example, how does the increased ocean resolution impact the ocean circulation and water properties in the Arctic and consequently the sea ice? Pg 12, Figure 3. I suggest to compute the seasonal cycle over the same period for the three products. Pg 13, lines 4-5: where the thicker sea ice in HN2.0 comes from? The amplitude of the melting season is similar from the area/extent seasonal cycle. How different are the sea ice properties (concentration, thickness, temperature, etc.) in the initialization fields? Explain the different amplitude of the volume seasonal cycle between the 2 model versions. Pg 13, line 7: please define "relatively thick and thin ice". Maybe a distinction between pack ice and marginal zone ice may help. Which mechanisms (dynamics, thermodynamics, both) improve the thickness representation in HN2.0? Is the ice drift similar in the two models? Pg 13, line 9: change Januar to January Pg 14, lines 5-6: could this differences in the growing season also be related to differences in the ocean and atmosphere between the two models? Are similar are, for example, air temperature and sea surface temperature in HN1.2 and HN2.0? Which one is the main driver of ice growth in the model?

Section 3.4 Pg 14, line 12: the agreement between PIOMAS and HN2.0 is "reasonable" only in March, the variability in September is not captured in the most recent years. For instance, the 2007 and 2012 minima are not reproduced. Given the modelled trend and the variability, I would not call "agreement" the overlap between curves. Why does sea ice extent in NH2.0 (mainly in March) present weaker inter-annual variability? Pg 14, line 16: ". . .trend in sea-ice volume . . . can thus only arise from large-scale atmospheric changes". I do not believe that is true. What are the differences in the two oceans? Is the variability of air temperature the same in the models? How are the feedbacks between the two components affected by the new coupler? Pg 14, lines 29-34: I did not understand the message within those lines. Please, rephrase.

Section 3.5.1 Pg 16, line 5: Kelvin in the text and Celsius in Fig.7. Use the same.

Section 3.5.2 Pg 17, line 5: 0 degree is the freezing temperature of freshwater (no salt in it). This is not the case for the Arctic ocean upper layer. Rephrase. Pg 17, line 7-8: about the reason of different summer temperature, how different are the heat fluxes between ocean and atmosphere in the two models? Is the air/ocean poleward heat transport the same? Then, rephrase "with an approximately by 10% underestimated sea ice concentration…" Pg 18, Figure 7: it might be more useful for the reader to have directly the plots of the differences HN2.0 – HN1.2 and HN2.0 – ERA Interim. It would help to add a contour indicating to the Arctic water freezing point. Pg 18, line 2: does the sea ice model include a melt-pond scheme? If so, which one? Was the same in HN1.2? Pg 18: maybe a comment on differences in solid and liquid precipitation between the 2 models and the comparison with ERA Interim might be helpful

Section 4 Title of section 4 "Conclusions: I do not detect so many conclusions or discussion on the model performances, more future work. It would be nice to add some conclusions drawn from the model results; alternatively rename Section 4 to "Conclusions and Future work". Pg 19, line 11-17: this study might also suggest that the physical core of the regional model components needs larger improvements. From those lines, a question arises whether the manuscript should include a better tuning and so better results. I would suggest to reformulate. Pg 20, line 3: how are the snow and ice albedo defined in HN1.2? Pg 20, Code availability: add the link to Max Planck InstituteÂăwebpage on YAC

I do not think that Table A1 and table A2 are necessary. For example, for the ocean depth, it might be enough adding something like: the layer thickness is 10m from the

surface to 215m and then increases up to about 350m at the bottom.

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