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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 #1

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General comments:

This paper presents an upgraded atmosphere-ocean-sea-ice coupled model version for Arctic climate studies. This coupled configuration combines some new atmospheric parameterisations, an increased horizontal and vertical spatial resolution for all components and the use of a new coupler software. Then it assesses the atmospheric and the sea-ice components using an ensemble of 10 hindcast simulations over 1979-2016. This paper is well structured allowing an easy read; it also shows both improvements and (robust) weaknesses and proposes some clues to explore for solving them. So I think it suits the aims of GMD publication. This new coupled configuration has required,

without doubt, an important work that would deserve to be highlighted in adding an analysis of the oceanic component. I have a few comments and questions below that, hopefully, will help to improve the paper.

Major comment:

My only major comment concerns the oceanic component. Although, in the paper, it is mentioned that (pg 12, lines 1- 2) 'Sea ice is an important indicator for the overall performance of a coupled Arctic climate model, because it depends on atmospheric and oceanic processes to a similarly large extent.', the oceanic component is not assessed at all and I think this aspect is missing.

Therefore, I will suggest the authors to assess the oceanic component in both HN1.2 and HN2.0. They could look at the paper of Ilicak et al. (2016) in which the Arctic ocean has been assessed in a set of ocean-ice simulations performed with the same inter annual CORE surface forcing for a models inter-comparison exercise. Many diagnostics are proposed and could be reproduced here but I think their Figure 7 could be of interest. Indeed it represents the mean vertical temperature and salinity profiles in the Nansen and the Canadian basins and compare them to the PHC3.0 climatology. This will give an interesting Arctic ocean vertical stratification state in the two coupled version models as the potential discrepancies compare to the climatology.

The Figure 5 of the present paper shows that the initial ensemble mean sea-ice state in 1979 is different between HN1.2 and HN2; it might be worth to show how is the ocean ensemble mean vertical stratification in 1979 as well. As mentioned by the authors, the spin-up protocol is not the same between HN1.2 and HN2.0, meaning that ocean/sea-ice spin-up time length has been built differently. This can lead to a more or less marked ocean drift in the two ensemble HN1.2 and HN2.0 which might be useful to identify. I think this will add value to the paper in giving a complete view of the three physical components especially in a climate study framework.

The reference previously mentioned: Mehmet Ilicak, Helge Drange, Qiang Wang, Rüdi-

ger Gerdes, Yevgeny Aksenov, David Bailey, Mats Bentsen, Arne Biastoch, Alexandra Bozec, Claus Böning, Christophe Cassou, Eric Chassignet, Andrew C. Coward, Beth Curry, Gokhan Danabasoglu, Sergey Danilov, Elodie Fernandez, Pier Giuseppe Fogli, Yosuke Fujii, Stephen M. Griffies, Doroteaciro Iovino, Alexandra Jahn, Thomas Jung, William G. Large, Craig Lee, Camille Lique, Jianhua Lu, Simona Masina, A.J. George Nurser, Christina Roth, David Salas y Mélia, Bonita L. Samuels, Paul Spence, Hiroyuki Tsujino, Sophie Valcke, Aurore Voldoire, Xuezhu Wang, Steve G. Yeager. An assessment of the Arctic Ocean in a suite of interannual CORE-II simulations. Part III: Hydrography and fluxes, Ocean Modeling, Volume 100, 2016, Pages 141-161, ISSN 1463-5003, https://doi.org/10.1016/j.ocemod.2016.02.004.

Minor comments:

1- Introduction:

pg 1, from line 10 to 15: the term feedbacks is never detailed, give few examples on major well known feedbacks even not fully understood ?

pg 1, from line 14 to 16 : 'Since these feedbacks . . ...' indicating that model upgrades are still needed. This sentence is not clear to me and might be reformulate

2- Description of HN2.0:

2-1-3 Boundary forcing:

pg 5, lines 10-11, to make the text easier to read, maybe just give only the lower boundary conditions of HIRHAM5.0 for the coupled configuration and not the stand alone case

2-2-4 Domain and discretization:

pg 7, line 15: '...but the model tends to crash every now and then. . .' I don't understand 'every now' here, need to be changed

pg 7, line 17 reformulate the sentence: ' ...the here presented simulations were running

" . . . by something like " . . .our simulations were running .. '

2-2-5 Boundary forcing:

pg 7, line 22: it might be worth to mention what is the lateral dynamical forcing in complement to the Levitus climatology T/S restoring

pg 7, line 27-29: as for the atmospheric component and to simplify, it might be helpful to focus only on the coupled and non coupled one boundary forcing. The stand alone mode doesn't help the reader

pg 8, line 3: as previously mentioned, talking about the stand alone mode is useless, focus only on the coupled and non coupled domain is enough. the key message is: in non coupled area, the atmospheric forcing are computing using NAOSIM bulk formula based on ERAI state variables such as wind components, clod, precipitation . . ..etc while over the coupled area the atmospheric fluxes are calculated by HIRHAM5 and transmitted to NAOSIM It might be interesting to mention the bulk formula type used in NAOSIM.

2-2-5 Technical modifications:

pg 8, line 15: the elapse time required to simulate 1 calendar year as the number of processors dedicated to both HIRHAM5 and NAOSIM is an interesting information that would be nice to mention here. The parallelization relies on MPI, OpenMP, hybrid MPI-OpenMP ?

2-3 Coupling procedure:

p8: it might be worth to precise the authors motivations for using YAC in NH2.0. What was the coupler used for HN1.2 ?

3 - Evaluation of the base configuration:

pg 10 line 13 & pg 11 lines 1-3: '. . .the local greenhouse effect is underestimated in the curent HN2.0 simulations...': does it means that air temperatures are also underestimated within the domain ? Details from Graham et al. (2017) (their Fig. 4.e) will be interesting to mention there. Furthermore, is this specificity (underestimation of local greenhouse effect) is also present in the HN1.2 version used here for the comparison ? This could be valuable to mention it.

3.1 Ensemble simulation setup:

pg 11, line 13: it would be helpful for the reader to get a clear information on the spin-up time length for both ocean-sea-ice components used as initial state for each 10 hindcast, i.e. spin-up range [33 - 42] years for HN2.0 if I am right and for HN1.2?

3.2 Data for model evaluation:

pg 11, line 26: the HN1.2 ensemble covers the period from 1979 to 2014, this period should be the one retained for the comparison with observations and with HN2.0 instead having 3 different periods, i.e. 1979-2016 for HN2.0 and 1979-2015 for satellite data. It will allow simplification.

pg 11, line 27-28: Differences in the simulation results between the ensembles of HN1.2 and HN2.0 thus indicate changes in the model performance. This sentence is a Âń shortcut Âż and is not convincing to me. I will agree if the ocean/ice ensemble setup for the spin-up was the same between the two ensemble which is not the case here. So the differences may not be only due to the changes in the model performance or I do not understand what the authors want to express.

pg 12: Figure 3: the mean seasonal cycle of sea-ice variables should be computed over the same common period between satellite data and the 2 ensemble hindcats as previously mentioned. Furthermore, the shaded colors are not visible enough, make them darker. For data voids around the North Pole, why not removing data from models instead filled gap with distance-weighted averages, it would make sense to do it rather than extrapolating for the comparison ?

3.3 - Sea-ice climatology:

pg 13, line 9: '. . . from january to august . . ..', word correction

pg 14, lines 5-6: the authors suggest the potential effect of lateral freezing reference value to explain the overestimation of both the sea-ice extent and area in HN2.0. How behave the seasonal cycle of air temperatures over the Arctic in HN2.0 compare to ERAI and HN1.2 ? I guess they could be lower leading to sea-ice formation combined to low value for the lateral freezing ?

3.4 - Sea-ice trends and variability:

pg 14, line 14-15: about the underestimated downward trend in sea-ice, it might be worth to detail here the implications of : * using a climatological boundary forcing for the oceanic component: do the authors think about ocean drift effect ? * constant greenhouse gases in HN2.0 and HN1.2: is it related to the air temperatures trend ? Add a comment (Figure 5) on the fact that the relatively weak ensemble mean sea-ice volume for both HN1.2 and HN2.0 in 1979 against PIOMAS (keeping in mind that PIOMAS is not an observed data set) might also explain this sea-ice underestimated trend if true ? And that this trend might be also lowered by weaker air temperatures in HN1.2 and HN2.0 (compared to ERAI) so limiting sea-ice melt process ?

3.5 - Near-surface air temperatures:

3.5.1 - Winter:

pg 17, line 8: '..be associated with an approximately by 10 % underestimated sea-ice concentration..'. Sentence to reformulate.

pg 18, Figure 7: I think that plotting 2-m air temperature differences against the ERA-interim data will clearly help the reader to catch spatial structures and also locations described in the text. Leave the ERA-interim air temperature field as it is and show differences for both HN1.2 and HN2.0

pg 20, line 12: mention the web site for the coupler YAC as it is done for the model source code ?

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