

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 #3

Received and published: 22 January 2019

This paper presents an upgraded version of a regional climate model covering a pan-Arctic region. The main difference is the use of a new atmospheric modelling component. The modifications to the model are described and a basic evaluation of sea ice and surface air temperature are presented. The main aim is to provide a reference for this new configuration to be used in further regional climate applications. As such, publication in Geoscientific Model Development seems appropriate. However, I find that the model description could be more clear and that the evaluation, focusing only on basic properties of the sea ice and near-surface temperature, is too limited. I recommend this paper for publication with major revisions. General and specific comments are provided below.

C1

General Comments:

1. Since the main purpose of this paper is to document changes to the coupled model, it is important that this description be clear and thorough. However, the authors fail to clearly present the differences between the new version HN2.0 and the previous version (HN1.x). The main change is the use of a new atmospheric model, which itself is built on two previously described components (HIRLAM7 and ECHAM5.4). The model description is often quite difficult to follow as the authors intermingle modifications with respect to HN1.x with modifications to HIRLAM7 and ECHAM5.4. I recommend this section be rewritten to make these differences clear. In particular, if the aim here is to document the differences between HN2.0 and HN1.2 then these should be outlined in detail, and not rely on previous publications of HIRLAM7 and ECHAM5.4. Without a clear description of direct differences between HN1.2 and HN2.0 it is difficult to interpret the results of the model evaluation presented in Section 3.

2. While I agree that sea ice is an important indicator of overall model performance, a reference paper such as this is more useful when a broader presentation of model performance is outlined. Given the large changes in the atmospheric component I would have expected to see a more detailed description of characteristics of the modelled atmosphere.

Specific comments:

1. Pg1, line 10: “allow to simulate”. Please rephrase, perhaps “allow one to simulate” or similar. 2. Section 1, para 3: It would be helpful to explain the motivations for upgrading the atmospheric component and any particular deficiencies that it is aiming to overcome. Also, the choice for the particular components chose for HN2.0 could be justified (ie HIRLAM7 and ECHAM5). 3. Pg2, line 18: Regardless if they have been described in reference manuals, if the aim of this paper is to document the new model version than a description of model components should be provided here. 4. Pg. 3, line 6: spelling error, should be “aerosol” 5. Pg. 3, line 8-9: “The most important

C2

modification” from what? From HN1.2 or ECHAM5? Mixing these up makes the text difficult to follow. Also statements like “for the most part” are vague and should be avoided. Rather, explain what has been changed and what hasn’t. 6. Pg. 3, line 17-18: “. . .attenuated such that at least 25%...”. This sentence is quite difficult to follow. If this is the most important modification then it would be worth including the equation and describing this properly. Also it seems it may be relevant for the sea ice results presented in Section 3 (?). 7. Pg. 3, Line 22: “The second modification. . .”. From what? ECHAM or HN1.2? 8. Pg. 4, line 13: semi-Lagrangian advection schemes are known to have conservation issues when used at high CFL number. For a weather model this is usually not a problem, but for a regional climate model this could affect the results. A discussion of this issue and the extent to which HN2.0 is conservative should be included, perhaps with some demonstration of applicable CFL numbers. 9. Pg. 5, line 17: “fine resolution” and “high-resolution” are not very useful. Please include a more precise indication of model resolution. Also, on pg2, line 16 it is noted that the ocean component is “largely the same”. If the model configuration has completely changed this statement is not accurate. Moreover, simply stating that the difference in model configuration is described in Fieg et al (2010) is not sufficient. At least a brief description should be provided here as well. 10. Section 2.2.3: How is this different from HN1.x? 11. Section 2.2.4: How is this different from HN1.x? 12. Pg. 7, line 29: Is there any blending used when going from HIRHAM5 forcing to ERAI? 13. Pg. 8, line 1: “standard bulk formulas”. Please describe. Are these the same bulk formulas used by HIRLAM when coupled? 14. Pg. 8, line 8: Is this the only difference in how fluxes are calculated? For example, are surface roughnesses and boundary layer stability all treated the same? 15. Pg. 11, line 10: The use of ice-ocean fields from Januaries 1991 to 2000 seems a rather odd choice. Some explanation should be provided. Also, since thickness over this period were thinner than for the earlier period, please describe any impact on mean sea ice results (i.e. due you see any spin up effects? Is there any change in ensemble spread from year1 to year 20+? 16. Pg. 11, line 27: If the main comparison presented in this paper is against this HN1.2 ensemble, than an

C3

explanation of how the setup differs should be given. 17. Fig. 3: (middle). It would be helpful to include PIOMAS here as well to be able to differentiate spatial differences (area) from thickness contributions to total volume. 18. Pg. 14, line 7: “had been resulted” change to “. . .resulted. . .” or similar. 19. Pg. 14, line 19: “observation-like” is a bit of an unusual term. Perhaps change this to “reference” 20. Pg. 14, line 32-33: The inability to simulate extrema is not necessary just a matter of model internal variability though as many key processes are missing (e.g. wave-ice interactions which played an important role in the 2012 minimum that is used as an example). It would be good to note this limitation in simulating extremes and comment on the degree to which this may be important for simulations with this regional climate model. Since HN2.0 has a higher resolution ocean-ice model, does this affect extremes? 21. Pg. 17, line 5-10. It would be helpful to show some additional diagnostics here associated with albedo and surface heat fluxes to understand better the source of these differences. 22. Pg. 18, line 3: If there is increased melting from the ocean, may this also be related to changes in ocean transports. A higher resolution ocean configuration may allow more Atlantic water to enter the Arctic via Fram strait and the Barents Sea. Some comment/validation of this would be helpful to understand how the behavior of HN2.0 differs from HN1.2. 23. Pg. 19, line2: “. . .be solved until now” change to “. . .as of now”.

Interactive comment on Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2018-278>, 2018.

C4