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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.

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Author Comments to the Comments of Referee #1

Response to the major comment

The referee's only major comment concerns the assessment of the ocean component. We absolutely agree that a detailed evaluation of the ocean component would be a valuable addition to the presented more technical description of the model development. However, there are a couple of reasons why we decided to focus in this paper

only on a base evaluation with respect to sea ice as the communicator between atmosphere and ocean:

1. The two component models HIRHAM5 and NAOSIM have already been used in previous studies that also include their evaluation. Since the setups of the component models have been left unchanged for the present coupled model version as far as possible, the primary task is to demonstrate that the new coupling procedure with the aid of YAC is technically working properly and that interactions between the component models are actually represented in an acceptable way.

2. The decision trying to publish the upgraded version of the coupled model in GMD as "Development and technical paper" has been made in order to allow a more detailed description of technical details from which in particular model users may benefit, but also other model developers who want to couple different model components. For this very reason, a more detailed model evaluation with a geoscientific focus was not intended and is beyond the scope of a development and technical paper in GMD.

3. A full evaluation of the entire model, not only with respect to the ocean component, but also with respect to the not less complex atmosphere component, would be so substantial that one or more stand-alone papers are required or at least highly recommended for a detailed evaluation of different aspects of the Arctic climate system. Some of these aspects, for instance sea-ice drift, Atlantic water inflow, and atmospheric cyclones, are already subject of our current research and will likely result in follow-up papers in pure scientific journals.

4. The current base configuration of the coupled model is certainly not the final model configuration, as already noted in the manuscript. A comparison with other fine-tuned coupled models, or even with pure ocean–sea ice models which use CORE-II interannual forcing and do not comprise feedback processes between

atmosphere and ocean–sea ice components, makes little sense at the present stage and should better be postponed until the development process towards an improved configuration will have been completed. However, the current base configuration of the upgraded coupled model already performs better than the old model version, indicating that interactions between the model components have clearly improved in spite of imperfect model configuration.

5. A further and more technical reason why we did not take into consideration to evaluate the deep ocean is the simple fact that such data are not available from the old model version (HN1.2). A comparison of vertical temperature and salinity profiles between HN2.0 and HN1.2 is therefore impossible.

As a reasonable compromise, because two of the three referees requested to include evaluation of the ocean component, we have incorporated a new subsection (section 3.5) in which the upper ocean temperature of HN1.2 and HN2.0 is compared with the PHC3.0 climatology. The upper ocean temperature was selected due to its direct influence on the sea-ice conditions. It also provides additional insight into the different bias structure of HN1.2 and HN2.0 and might be a valuable supplement to the discussion of model improvements that are directly related to the coupling.

Because we have kept the focus mainly on a base evaluation with respect to sea ice as the communicator between atmosphere and ocean, and in order to clarify what the reader may (and may not) expect from the paper, we have added the following sentence to the abstract: "The evaluation focuses mainly on sea ice as the communicator between atmosphere and ocean." We have also added a brief outlook in the Conclusions section that more detailed evaluation of the model is postponed until the development process towards an improved configuration will have been completed and will be subject of follow-up studies.

Another comment made by the referee with respect to the ocean component was the initial ensemble mean sea-ice state in 1979. The referee notes that this state is dif-

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ferent between HN1.2 and HN2.0 and argues that this difference might be related to a more or less marked ocean drift in the two ensembles HN1.2 and HN2.0. Actually, the difference in the ensembles' mean sea-ice state in 1979 is an expression of different steady states due to differences in the model physics. Even though the initial states of the two ensembles have been selected on a slightly different basis, this basis was in either case an already steady-state simulation of the respective coupled model version with its specific model physics. This setup for building an ensemble of coupled model simulations has already been used in previous studies (e.g., Dorn et al., 2012) and ensures an minimization of internal model drift due to the use of initial conditions from the steady state of a coupled spin-up run with the identical model version as used for the ensemble simulations themselves. Therefore, ocean drift is not a major issue in this case.

Clear information that all HN2.0 ensemble members were initialized with ocean–ice conditions from the steady state of the specific model configuration has now been added to the description of the ensemble simulation setup. Later, when we mention the HN1.2 ensemble for the first time, we have now pointed explicitly to the comparability of the two ensemble setups from a scientific point of view, even if they differ technically.

Response to the minor comments

In the following, a point-by-point response to the referee's minor comments is given in the sequence of comment (C) and answer (A).

C: *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 ?*

A: We have added two examples of feedbacks, the well-known ice–albedo feedback and the not fully understood feedback between sea ice and cyclones.

C: *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

A: We have removed the whole sentence, because it is not absolutely necessary for the paper.

C: *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*

A: Information on HIRHAM5 stand-alone has been removed in this paragraph as suggested.

C: *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*

A: The phrase "every now and then" was here used as a synonym for "from time to time" or "occasionally". We have replaced this phrase by the latter.

C: *pg 7, line 17 reformulate the sentence: ' ...the here presented simulations were running " . . . by something like " . . .our simulations were running .. '*

A: The term "the here presented simulations" has been replaced by "the HN2.0 simulations".

C: *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*

A: The formulation of the open-boundary condition by Stevens (1991) used in NAOSIM does not require forcing data for solving the equations of motion due to the neglect of the nonlinear advection terms at the boundary and extrapolating from the boundary to outside the boundary for the horizontal diffusion terms. Thus, there is nothing to mention with respect to the lateral dynamical forcing.

C: *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

A: Information on NAOSIM stand-alone has been removed in this paragraph as suggested.

C: *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.*

A: Information on the stand-alone mode has also been removed in this paragraph. The bulk formulas used in NAOSIM are based on the formulations for turbulent fluxes and shortwave radiation by Parkinson and Washington (1979) and for longwave radiation by Rosati and Miyakoda (1988). We have added this information to the paper.

C: *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 ?*

A: The paragraph has been extended by more specific information on the reparallelization. The number of processors allocated to HIRHAM5 and NAOSIM for coupled simulations and the corresponding elapsed time required to simulate one calendar year have been added.

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

A: A brief description of the coupling procedure in HN1.2 and earlier versions has been added to section 2.3, directly leading to the motivation for using YAC in HN2.0.

C: *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.*

A: Air temperatures are indeed a little too cold in HN2.0. This finding is already mentioned in section 3.5 when discussing the near-surface air temperatures. Figure 4e by Graham et al. (2017) show results from the old model version (HN1.2). A comparable analysis with HN2.0 has not yet been made and is definitely beyond the scope of the current paper. The greenhouse gases in HN1.2 were set to values representative for the year 1990 (the default values of the ECHAM4 parameterizations used in HN1.2). When explicitly pointing to an erroneous setting of greenhouse gases in the current HN2.0 simulations, it should be implicitly clear that this error does not appear in the HN1.2 simulations.

C: *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?*

A: From previous studies it is known that the coupled regional model needs a spin-up time of about 6–10 years to reach a quasi-stationary seasonal-cyclic state of equilibrium (Dorn et al., 2007). If the initial ice conditions are not far away from this state, the spin-up time will be even shorter. This result was found in simulations with HN1.1 and has been experimentally verified with HN2.0. Since the initial conditions of all ensemble simulations with both HN2.0 and HN1.2 were taken after more than 10 years of spin-up, they all represent the steady state of the respective model version. Consequently, the specific length of spin-up that exceeds the 10-year limit is completely irrelevant. Clear information that all ensemble members were initialized with ocean–ice conditions from the steady state of the specific model configuration has now been

added to the description of the ensemble simulation setup.

C: *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.*

A: We have recompiled Figure 3 and Figure 4 using data only for the period 1979–2014 in all cases. There are only minor, almost undetectable variances compared to the old figures so that adaptations of the text (beyond the figure captions) have not been needed.

C: *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 shortcut 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.*

A: As aforementioned, the ensemble setup of HN1.2 and HN2.0 is comparable from a scientific point of view, even if it differs technically. Therefore, the differences in the simulation results between the ensembles of HN1.2 and HN2.0 can surely be rated as indication of changes in the model performance. We have added the reference of the scientific comparability of the two ensembles before pointing to the changes in the model performance. We have also specified "model performance" as "model performance due to differences in the physical process descriptions" in order to express that we here do not refer to the technical or computational performance, which is clearly better in HN2.0, but is not expressed by the differences in the simulation results.

C: *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 hindcasts 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 ?

A: As already noted, all data sets of the new Figure 3 refer now to the period 1979–2014. The shaded areas have been made darker (transparency changed from 50 % to 80 %). Data voids around the North Pole in the satellite data are still filled with distance-weighted averages. The area around the North Pole is the central region of the coupled model system and is year-round ice-covered and therefore important. The extrapolation of the satellite data using distance-weighted averages to fill data voids is an adequate method that leads to reasonable results and enables an unbiased comparison with the model results for the identical domain.

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

A: The word 'January' has been corrected.

C: *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 ?*

A: The important role of the parameterization of lateral freezing in uncoupled and coupled models was already investigated in several previous studies (Fichefet and Morales Maqueda, 1997; Bitz and Lipscomb, 1999; Dorn et al., 2007; Wang et al., 2010; Mauritsen et al., 2012; Notz et al., 2013; Shi and Lohmann, 2017). The reference thickness for lateral freezing basically controls how fast open-water areas are freezing up when the surface energy balance is negative. A reduced value leads to faster freeze-up and reduces the heat transfer from the ocean to the atmosphere due to the insulating ice cover. One of the consequences are colder near-surface air temperatures, another consequence is decelerated ice thickness growth due to reduced heat loss to the atmo-

sphere. Thinner ice, in turn, is associated with increased conductive heat flux through the ice which damps the near-surface cooling of the atmosphere, but cannot compensate for the differences in the ocean-atmosphere heat flux due to reduced open-water areas. Consequently, the colder near-surface air temperatures, which are bound to occur during the freezing season in the HN2.0 simulations, play only a minor part in the sea-ice formation process. This partial aspect of the complex interaction between ice growth and near-surface air temperatures represents a negative feedback mechanism. Concerning negative feedbacks, it is pretty difficult to adjust a model in a reasonable way. At least fine-tuning of the reference thickness for lateral freezing is one possibility to minimize biases in sea-ice thickness and concentration as well as in near-surface air temperatures at the same time. In order to explain why the reference thickness for lateral freezing might be responsible for the sea-ice bias and represents one possibility to minimize the bias, we have added a brief discussion with reference to the study by Dorn et al. (2007) at the end of the paragraph in question.

C: *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 ?*

A: The use of a climatological boundary forcing for the oceanic component is indeed one important obstacle for a realistic reproduction of the observed downward trend. Long-term changes in the oceanic heat transport, which might have contributed to the trend, cannot take effect in the current model configuration. An artificial ocean drift effect, which could counteract the downward trend, is not really detectable in the

model simulations. As already noted before, all model simulations started from steady states. Test simulations with more realistic greenhouse gases showed that the observed downward trend in sea ice and the related upward trend in air temperatures is still underestimated, even though a little less. Therefore, we suppose that the oceanic boundary forcing plays a similarly important role as the atmospheric boundary forcing, while the local greenhouse effect is a second-order effect. This supposition still needs to be proved by elaborated sensitivity experiments that are already scheduled. At least one question raised by the referee can definitely be answered: The relatively weak ensemble mean sea-ice volume for both HN1.2 and HN2.0 in 1979 against PIOMAS does not contribute in any way to the underestimated trend in sea ice. Simulations with higher initial ice volume show comparable underestimations. Also, the (local) air temperatures do not play a role for the underestimated sea-ice trend, because they do not represent forcing variables, but prognostic variables of the coupled model system. Local temperature changes and sea-ice changes are therefore closely linked to each other. The question should rather be how well the physical processes, which constitute this linkage, are simulated by the model? Concerning this matter, HN2.0 does a clearly better job than HN1.2 as can be seen from the lower biases in both sea ice and surface temperature.

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

A: The sentence has been reworded. The clause in question reads now "…be associated with the underestimation of sea-ice concentration by about 10 % …"

C: *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*

A: We agree that it is easier to identify the differences between the simulations and

ERA-Interim when showing difference plots. At the time when we thought about the selection of figures for the current paper, we already had difference plots in mind, but finally decided to include the plots of the temperature climatologies for reasons of consistency with Figure 4. This is of course not a compelling reason. Therefore, we followed the referee's suggestion and have replaced the subfigures of the HN1.2 and HN2.0 climatologies by their respective difference to ERA-Interim.

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

A: Information about access to the coupling software YAC has been added to the section 'Code availability'.

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