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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.

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Author Comments to the Comments of Referee #3

Response to the general comments

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

C1: *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 previsouly 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 than 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.*

A1: We agree that the description of the changes and modifications was not always clear. In particular, a clear differentiation between the general HIRHAM5 description and specific modifications directly related to HN2.0 was missing in the description of the atmosphere model. In the revised version of the paper, we have specified the technical modifications in HIRHAM5 compared to HIRLAM7 and ECHAM5 in a separate section. The subsequent section provides a brief overview about the physical parameterizations and general differences to HN1.x followed by the specific modifications in HN2.0. In addition, the lead paragraph of section 2 includes now basic differences between HN2.0 and HN1.x in order to avoid any misunderstanding from the outset. This includes information on the model resolutions in HN2.0 and HN1.x which were previously missing for HN1.x. Further differences between HN2.0 and HN1.x are explicitly stated when describing the model components.

C2: *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.*

A2: The regional atmospheric climate model HIRHAM5 was developed a few years ago and has already been used in a number of previous studies that partly include very detailed description of characteristics of the modeled atmosphere. We have added citations to some of these previous studies to the preface of section 2.1, primarily to avoid the impression that the development of HIRHAM5 is related to the development of HN2.0. This is not the case; HIRHAM5 already existed before and was chosen as the new atmosphere component of the coupled system, precisely because it has already successfully been applied in a number of previous studies. The aim of the present paper is to document the new coupled system, and not the individual components, and 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. Sea ice is the communicator between atmosphere and ocean. Its reasonable representation in HN2.0 clearly indicates that the coupling works well, even if there is still need for further improvements in the model configuration. A full evaluation of the entire model would be so substantial that one or more stand-alone papers are required or at least highly recommended. Apart from this, a detailed evaluation of different aspects of the Arctic climate system would have a geoscientific focus and goes beyond the scope of a development and technical paper in GMD. 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.

Response to the specific comments

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

C1: *Pg1, line 10: "allow to simulate". Please rephrase, perhaps "allow one to simulate" or similar.*

A1: We have replaced "allow to simulate" by "provide the possibility to simulate".

C2: *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).*

A2: The motivation for upgrading the atmospheric component is simply to make use of the most recent HIRHAM version which includes more sophisticated parameterizations. This HIRHAM version, named HIRHAM5, was built up with HIRLAM7 and ECHAM5 a few years ago and has already been used in a number of previous studies (and a subset of them has now explicitly been cited). The development of HIRHAM5 is not related to the development of HN2.0, it is just the atmospheric component of the new coupled system. We have realized that there is need for a clear differentiation between the general HIRHAM5 description and specific modifications directly related to HN2.0, and we have revised section 2.1 accordingly.

C3: *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.*

A3: The two model components are not part of the development of the coupled system. Both HIRHAM5 and NAOSIM were developed a few years ago and have already been used in previous studies that partly also include more detailed model descriptions. The aim of the present paper is to document the new coupled system and not the individual components, although we agree that it could be helpful to go into more detail when describing the components. In the revised version of the paper, we have added more information on the components, but we have kept the focus on specific changes for the coupled model system.

C4: *Pg. 3, line 6: spelling error, should be "aerosol"*

A4: Should actually be "aerosols". Corrected!

C5: *Pg. 3, line 8-9: "The most important 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.*

A5: All modifications refer to the ECHAM5 parameterizations as component of HIRHAM5. HN1.2 comprises the atmosphere model HIRHAM4, a different model, except for the name HIRHAM. This circumstance has now explicitly been mentioned. Further, we have restructured the division into subsections to differentiate between the general descriptions of HIRHAM5 (with previous modifications with respect to the original HIRLAM-7.0 and ECHAM5 codes) and the current modifications for HN2.0. In the revised version of the paper, there is now a subsection for the HIRHAM5 components with general technical modifications compared to the original model codes and a subsection for modified parameterizations as part of the development of HN2.0. Ambiguous formulations have been reworded. The statement "for the most part" has been removed.

C6: *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 (?).*

A6: If we included only the equation for the restriction of the melt pond fraction, we would explicitly emphasize a non-observationally based model adjustment. A reader of the paper might think that the equation with a value of 25 % represents a general improvement of the albedo parameterization, just because the equation is specified. This is not the case. We already noted in the manuscript that the value of 25 % can be considered as a tuning value and that a more realistic parameterization of the fractions

of snow and melt ponds should be derived from observations. First efforts towards an observationally based parameterization of the melt pond fraction are already underway. And, of course, the albedo parameterization is highly relevant for sea ice as is generally known.

C7: *Pg. 3, Line 22: "The second modification...". From what? ECHAM or HN1.2?*

A7: All modifications refer to HIRHAM5 as noted before.

C8: *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.*

A8: Every numerical scheme has its pros and cons. Atmospheric climate models usually base on NWP models and need to rely on their skill in simulating the weather using the specifically implemented numerical scheme. The reasonable simulation of the frequency of occurrence of dominant weather pattern is also essential for atmospheric climate models, certainly more important than everything else. HIRLAM was and is successfully applied by a number of European weather services, and also HIRHAM5 was and is applied by a couple of research institutions. So far, conservation issues using the semi-Lagrangian scheme with large time steps in HIRLAM or HIRHAM5 have never been documented in the literature. The time step of 600 s was chosen, because the model still runs stable with this time step, and produces results comparable to simulations using the Eulerian advection scheme, even if the CFL number is larger than one. Assuming that HIRLAM and HIRHAM5 does not have conservation issues, why should HN2.0 have them? We would understand if such a comment was addressed to authors of a HIRLAM or HIRHAM5 development paper. For the description of the coupled model system, the discussion of a non-existent issue in one of its components does not provide an added value.

C9: *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.*

A9: We have added basic differences between HN2.0 and HN1.2 to the lead paragraph of section 2. This also includes information on the different model resolution. The phrase "largely the same" has been dropped in the course of reformulating the lead paragraph. Subsequent to the reference to Fieg et al. (2010), we have added a brief overview about the few differences between HN2.0 and HN1.2.

C10: *Section 2.2.3: How is this different from HN1.x?*

A10: The ocean-sea ice coupling in HN1.x and HN2.0 is identical. The optional scheme by Castellani et al. (2014) is not available in HN1.x, but is also not used in HN2.0.

C11: *Section 2.2.4: How is this different from HN1.x?*

A11: The model domain is the same, but HN1.x uses lower horizontal, vertical, and temporal resolution than HN2.0. We have now explicitly specified the resolutions of HN2.0 and HN1.x in the lead paragraph of section 2. Model crashes did not occur in HN1.x., but they did occur in stand-alone simulations with the FRM version of NAOSIM. Evidently, they are not related to the coupling with HIRHAM5.

C12: *Pg. 7, line 29: Is there any blending used when going from HIRHAM5 forcing to ERAI?*

A12: Blending is not used, neither in HN2.0 nor in HN1.2. Especially HN1.2 shows discontinuities in the atmospheric surface fluxes inside and outside the coupling domain. In HN2.0, the transition from inside to outside the coupling domain is rather smooth

and the discontinuity does not appear any longer. We have added the discussion of the discontinuity at the boundary of the coupling domain to the new section 3.5, subsequent to the discussion of the upper ocean temperatures, and have emphasized this additional improvement in HN2.0 again in the conclusions.

C13: *Pg. 8, line 1: "standard bulk formulas". Please describe. Are these the same bulk formulas used by HIRLAM when coupled?*

A13: We have now specified that 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). Further, any bulk formulas of HIRLAM are completely irrelevant for HN2.0, since the originally embedded physical parameterization package of HIRLAM was replaced by that of ECHAM5 as already mentioned on page 2 of the manuscript. ECHAM5 includes bulk formulas only for turbulent surface fluxes. These formulas are more sophisticated, since they explicitly take account of atmospheric stability in the near-surface layer.

C14: *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?*

A14: There are numerous differences in the calculation of fluxes between sophisticated atmosphere models like HIRHAM5 and simple bulk formulas. However, from the viewpoint of the coupled model system, it is enough to know where the fluxes come from and not how they were calculated. There is no need to detail the differences in the calculation at this point.

C15: *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 is ensemble spread from year1 to year 20+?*

A15: The choice of initial ocean and sea-ice fields from different years of a spin-up run is motivated by the fact that the real ice-ocean state in January 1979 is practically unknown. The different initial conditions from the spin-up run represent the diversity of ocean–ice conditions within the steady state of the specific model configuration. Two essential points are that the spin-up run already reached a quasi-stationary seasonal-cyclic state of equilibrium for the mid-1980s and that the spin-up run was carried out with the identical model version as used for the ensemble simulations themselves. The latter is necessary to avoid an initial drift in the ensemble simulations due to inconsistent model physics. Consequently, spin-up effects in the ensemble simulations are negligible. Systematic temporal changes in the ensemble spread are not visible, too. The specific method for choosing initial conditions is well-conceived and was already applied in previous studies (e.g., Dorn et al., 2012). Clear information that all ensemble members were initialized with ocean and sea-ice fields that represent the diversity of ocean–ice conditions within the steady state of the specific model configuration has now been added to the description of the ensemble simulation setup.

C16: *Pg. 11, line 27: If the main comparison presented in this paper is against this HN1.2 ensemble, than an explanation of how the setup differs should be given.*

A16: We have replaced "differs slightly" by "differs technically", because the differences in the two ensemble setups are only of technical nature. Further, we have specified now that the initial ocean and sea-ice fields were taken from different years of two earlier simulations with HN1.2, and have emphasized that the two ensemble setups are comparable from a scientific point of view.

C17: *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.*

A17: Ice area/extent from PIOMAS differs slightly from the satellite data, especially during the summer months. The sea-ice differences between HN2.0 and PIOMAS are indeed lower than those between HN2.0 and the satellite data. Nevertheless, we think

that the satellite data are more reliable than the model assimilation system PIOMAS, and we decided then to not include ice area/extent from PIOMAS in Figure 3 (as well as in Figure 5), just to avoid confusion in the interpretation of the figure by the presence of two observational data sets. This argument still holds true.

C18: *Pg. 14, line 7: "had been resulted" change to ". . . resulted. . . " or similar.*

A18: Has been corrected to "had resulted".

C19: *Pg. 14, line 19: "observation-like" is a bit of an unusual term. Perhaps change this to "reference"*

A19: We have replaced "observational-like" by "reference".

C20: *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?*

A20: We agree completely that the inability to simulate extrema is not necessarily just a matter of model internal variability. The special note to sea-ice extrema by exemplifying the sea-ice minimum in 2012 has been removed from this paragraph. Instead, we have written: "Deviations from observed sea-ice conditions in specific years can therefore also be a consequence of internal model variability."

C21: *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.*

A21: In-depth analysis of the differences is topic of upcoming studies and goes beyond the scope of a development and technical paper in GMD.

C22: *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.*

A22: The ocean mixed layer beneath the Arctic sea ice is more or less decoupled from the Atlantic water due to the stable halocline in the Arctic Ocean that minimizes the vertical exchange. Except for the marginal and shelf seas of the Arctic Ocean, potential changes in ocean transports play only a minor role for bottom melting of sea ice. The heat content of the Arctic ocean mixed layer is primarily controlled by the heat exchange with the atmosphere. This applies to both HN2.0 and HN1.2. The impact of the different resolution in NAOSIM on the ocean circulation was investigated by Fieg et al. (2010) with a focus on the oceanic transports through Fram Strait.

C23: *Pg. 19, line2: "... be solved until now" change to "... as of now".*

A23: The term "as of now" has a different meaning than "until now". The latter is what we would like to emphasize at this point.

References

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