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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 #2

Response to the major comments

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

C1: *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.*

A1: Arctic amplification is mentioned as an example, because advanced knowledge about Arctic amplification is the overall objective of the project funding. The upgraded version of the coupled model system has been developed in order to provide contributions to this objective. Also, and this is the most important point, our proposed application of the regional model is not to simulate future climate changes as realistically as possible, but to simulate interaction processes between atmosphere, sea ice, and ocean in the Arctic, and to better understand feedback processes that contribute to Arctic amplification. Having this in mind, the constrained lateral boundary conditions are indeed a major advantage, since the model does not need to show reasonable performance in simulating global teleconnections. The overturning circulation is largely imposed by the ocean boundary conditions. Of course, variability in the overturning circulation is not captured using a climatological ocean boundary, but this limitation is irrelevant for the description of the coupled model system and its basic evaluation that clearly demonstrates 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. However, we agree that the choice of the lateral boundary forcing is an important issue. Accordingly, we have included statements about this limitation. In the revised version of the paper, we have mentioned that "the coupled regional climate model has to rely on the atmospheric and oceanic data used as lateral boundary forcing" and that "this is a particular issue for the lateral ocean boundary and represents a limitation of coupled regional climate models in adequately reproducing observed climate changes." Later, when we mention the Levitus climatology for the first time, we have added a comment on the choice of this climatology: "The Levitus climatology was chosen to remain comparable to previous NAOSIM stand-alone and HN1.2 simulations." In the Conclusions section, we have now noted that "the model might also benefit from time-varying lateral ocean boundary forcing from ocean reanalyses which are envisaged to replace the Levitus climatology in the future."

C2: *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 and Uotila et al 2018 can be followed.*

A2: We admit that the MOM-2-based model is an old model, but this version of MOM is an integral part of NAOSIM. Both component models, HIRHAM5 and NAOSIM, already existed in the present form before we started to coupled them. In the revised version of the paper, we have now included more details on the two component models that might be helpful to understanding the upgraded coupled model system without the requirement of reading secondary literature. This includes the improvement of section 2.2 on the NAOSIM description. NAOSIM has already been used in previous studies that also include their evaluation. These studies showed that the model is successful in simulating the ocean circulation in the Arctic (e.g., Drange et al., 2005; Karcher et al., 2012; Aksenov et al., 2016), Arctic Ocean freshwater content (Rabe et al., 2014), sea-ice concentration (e.g., Adams et al., 2011) and drift (e.g., Rozman et al., 2011), or the impact of cyclones on the sea ice in the Arctic Ocean (Kriegsmann and Brümmer, 2014). This justifies its usage. Furthermore, NAOSIM takes part in the international "Forum for Arctic Ocean Modeling and Observational Synthesis (FAMOS)" project, where state-of-the-art ice–ocean models are intercompared and evaluated. There, NAOSIM does not

stand out as an outlier, which again justifies its usage. 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 (please take a look at the Author Comments to the Comments of Referee #1 for more details). However, 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.

C3: *More details on the HN1.2 runs are needed for a clearer understanding of the intercomparison results*

A3: A considerable number of details on the previous version of the coupled model has been added to the paper, whenever differences between HN2.0 and HN1.2 appeared to be helpful for understanding the intercomparison results.

C4: *The manuscript would largely benefit from a detailed analysis of the possible changes in the air-ocean-sea ice interactions*

A4: 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. Detailed analysis of the possible changes in the air-ocean-sea ice interactions would have a geoscientific focus and goes beyond the scope of a development and technical paper in GMD.

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

A: The horizontal resolution of 0.25° corresponds actually to approximately 27 km in the rotated spherical coordinate system where the equator crosses the model domain. The use of a rotated spherical coordinate system together with the location of its North Pole as only necessary information for its definition was already mentioned at the beginning of this section.

C: *Pg 5, line 10: provide a clearer distinction between the setting of stand-alone mode and the coupled mode*

A: Information on the HIRHAM5 stand-alone mode is not absolutely necessary and has therefore been removed in this paragraph in order to ease the understanding of the coupled mode which is the key aspect of the description.

C: *Pg 5, lines 11-12: what are the implications of different conditions applied to uncovered sea points?*

A: Because there are only a few sea grid points in HIRHAM5's boundary zone that are not covered by NAOSIM, and because these few sea grid points are far away from the NAOSIM domain (see Figure 1), there are no implications like discontinuities or the like. The different surface conditions are therefore of no importance for the coupling with NAOSIM.

C: *Pg 5: I suggest to add details on relevant differences between HRM and FRM*

A: The few relevant differences between HRM and FRM as used in the coupled model have been specified now.

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

A: We have added information on the tracer advection scheme and the parameterizations of friction and diffusion to section 2.2.1 and on the bottom topography (bathymetry) to section 2.2.4. The grid (mesh) was already described in every detail.

C: *Pg 7, lines 1-2: the description of Arakawa B grid corresponds to the description of C grid in 2.1.2*

A: The description of the Arakawa B-grid is correct, but we have now indicated explicitly that both horizontal velocity components are staggered in the same way, which is in contrast to the Arakawa C-grid. The description of the Arakawa C-grid in the previous section 2.1.2 (now section 2.1.3) was not incorrect, but incomplete, because we missed to mention the discrete staggering of u in x-direction and of v in and y-direction. We have added corresponding information to section 2.1.3.

C: *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?*

A: The reasons for numerical instability are hard to determine. At least the model crashes can definitely not be ascribed to the choice of the mixing time step, as the referee supposes; they must have any other causes. We suppose that sharp gradients in the atmospheric fluxes, which often occur in the vicinity of coastal mountains, might trigger small-scale instabilities in the nearshore currents that cannot be resolved numerically. However, this is guesswork and should not be stated as explanation without any proof. Therefore, we did not state potential reasons for the occurrence of the numerical instability, but only an option how to overcome the model crash when it occurs. The choice of the mixing time step is such an option, since mixing time steps may damp time splitting characteristic of schemes centered in time. The Euler backward time step, which replaces the centered leapfrog time step at regular intervals, is beyond that diffusive and also damps spatial scales. Therefore, a smaller number of mixing time steps, which is in turn equivalent to a higher frequency of Euler backward time steps, was chosen in case of a model crash and has proved to be effective to overcome the crash. We have now indicated explicitly in the corresponding paragraph that the choice of a smaller number (of mixing time steps) is a suitable "option" to avoid a model crash, but we have abstained from adding any speculative assumptions on potential reasons for the occurrence of the model crash.

C: *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*

A: Just as for the atmosphere component, information on the stand-alone mode has been removed also in this paragraph, since the coupled mode is the key aspect of the description. In contrast to the atmosphere component, the coupled and uncoupled sectors of the model domain adjoin each other, and it cannot be ruled out that discontinuities occur due to the different treatment of surface conditions. This is indeed the case in HN1.2 and earlier versions, where the atmospheric surface fluxes inside and outside the coupling domain systematically differ. In HN2.0, the atmospheric fluxes inside and outside the coupling domain basically agree in their temporal and spatial variation. Consequently, the transition from inside to outside the coupling domain is

rather smooth and the discontinuity does not appear any longer. It is suggested that this additional improvement in HN2.0 can not only be attributed to improved physical parameterizations in the atmosphere component HIRHAM5, but also to the new coupling procedure which applies conservative remapping and consistent time averaging of coupling fields with correct timing. 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 recapitulated this additional improvement in HN2.0 again in the Conclusions section.

C: *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?*

A: The paragraph has been extended by more specific information on the new parallelization of NAOSIM. Specifically, information on the parallelization method as well as on 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. Since HIRHAM5 already comprised an efficient parallelization, there was no need for improvement in HIRHAM5 concerning this matter. Therefore, only NAOSIM is mentioned.

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

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 10: the namelist of ocean and sea ice parameters is missing in the manuscript*

A: Relevant namelist parameters (and other physical constants) of all model components are listed in Table B1. This is mentioned a few lines later at the end of the paragraph (previously page 11, lines 4–5).

C: *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.*

A: The paragraph already includes precise information on the initialization and the corresponding spin-up time of the HN2.0 ensemble simulations. The basic spin-up strategy is to initialize the ocean and sea-ice fields with different conditions from the steady state of the coupled spin-up run. The essential point is that the coupled spin-up run has to be carried out with the identical model version as used for the ensemble simulations themselves. This is necessary to avoid an initial model drift in the ensemble simulations due to differences in the model physics. 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 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 HN1.2 ensemble uses the same spin-up strategy as the HN2.0 ensemble. The specific length of spin-up that exceeds the 10-year limit is completely irrelevant for this purpose. 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. Finally, the word "driven"

is commonly used in the regional model community for the lateral boundary forcing as well as for the lower or upper boundary forcing when applicable.

C: *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?*

A: The specific initialization of the atmosphere is only relevant in terms of numerical weather prediction; it absolutely plays no role in long-term climate simulations. Validation of the model simulations against ERAI data is therefore unproblematic. With respect to the lateral boundary forcing with ERAI data, it is even an advantage to validate the model simulations against ERAI data, because the effects of the internal model physics can better be isolated from large-scale atmospheric changes entering the model via the atmospheric model boundaries.

C: *Pg 11, line 22: reword "quasi realistic"*

A: The term "quasi realistic" has been replaced by "quite realistic".

C: *Pg 11, lines 27-28: clear statements on the differences and similarities 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.*

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 due to differences in the physical characteristics, which includes not only physical parameterizations, but also the physical interaction between the model components by means of the revised coupling. We have added the reference of the scientific comparability of the two ensemble setups 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, 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?*

A: Of course, a good representation of sea-ice properties does not guarantee a good representation of ocean and atmospheric fields. There might be model biases that are related for instance to shortcomings in the parameterization of clouds or boundary layer processes. On the other hand, a bad representation of sea ice properties inevitably induces biases in ocean and atmospheric fields due to biases in the surface energy budget. Sea ice thus plays a key role for the surface processes in the Arctic. A reasonable representation of sea ice in a coupled Arctic model system can be considered as a necessary, but not sufficient condition for a good representation of ocean and atmospheric fields. Atmospheric temperature fields are already discussed in the paper. We have extended the paper by 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 linkage to 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. The two component models HIRHAM5 and NAOSIM have already been used in stand-alone mode in previous studies that also include their evaluation. For example, HIRHAM5’s cloud parameterization was evaluated by Klaus et al. (2012, 2016), and the impact of NAOSIM’s increased resolution on the ocean circulation was investigated by Fieg et al. (2010). Since the setups of the com-

ponent 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. The latter is reflected in a good representation of sea ice.

C: *Pg 12, Figure 3. I suggest to compute the seasonal cycle over the same period for the three products.*

A: All data sets of the new Figure 3 refer now to the period 1979–2014. 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 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.*

A: The amplitudes in the seasonal cycle of ice area and extent differ considerably (see Figure 3). The initial fields are completely irrelevant as evidenced by the low and completely insignificant across-ensemble scatter. Different amplitudes in the seasonal cycle of the ice volume are a consequence of different physical parameterizations in the two model versions as verified by a couple of sensitivity experiments (see Dorn et al., 2007, 2009).

C: *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?*

A: We have replaced the term “relatively thick and thin ice” by the specification “rather thick and rather thin ice within the Arctic pack ice region". Generally, thermodynamics plays the decisive role in terms of total ice volume/extent/area, while dynamics are jointly responsible for the geographical distribution of ice. The improved geographical distribution of ice can therefore also be attributed to an improved ice drift. The evaluation of sea-ice drift in HN2.0 will be subject of a follow-up study, which is currently in preparation. We have added a brief outlook in the Conclusions section that more detailed evaluation of the model will be subject of follow-up studies.

C: *Pg 13, line 9: change Januar to January*

A: "January" has been corrected.

C: *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?*

A: The newly incorporated Figure 7, which shows the upper ocean temperatures, clearly indicates colder ocean temperatures in the Kara and Chukchi seas in HN2.0. The early ice growth in these particular regions is certainly associated with the cold temperature bias in the upper ocean. This cold ocean temperature bias might be one of the reasons for accelerated ice growth and the overestimate of ice area and ice extent until the end of the year. We have added corresponding statements to the paragraph. HN2.0 also shows colder near-surface air temperatures in the afore-said regions, but they usually represent rather a response to the surface conditions than a driver. At the end of the paragraph, we have added some findings from a previous study (Dorn et al., 2007) about the linkage between near-surface air temperatures and ice growth as a function of the reference thickness for lateral freezing in order to explain why fine-tuning of this parameter represents one possibility to minimize the sea-ice bias as well as the winter temperature bias.

C: *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?

A: We have replaced "agreement" by terms like "close to" or "similarities". The interannual variability in March sea-ice extent in NH2.0 is as weak as in the satellite data. HN1.2 shows here definitely too high interannual variability due to the Labrador Sea bias which spreads far to the south from time to time. In September, the interannual variability is in the order of magnitude as in the satellite data when looking at individual ensemble members. The slightly weaker variability of the ensemble mean results from the averaging process.

C: *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?*

A: The only time-varying external forcing of the coupled model system is the atmospheric boundary forcing. All other climate forcings are given by fixed constants (like greenhouse gases, aerosols, ozone, solar constant, orbital parameters, land surface cover, soil characteristics) or constant seasonal cycles (like lateral ocean boundary, deep soil temperature, vegetation). In view of the fact that only the atmospheric boundary forcing changes with time and feedback cycles are internal to the climate system, there is no other explanation for the downward trend as the one given in the paper, regardless whether one believes it or not.

C: *Pg 14, lines 29-34: I did not understand the message within those lines. Please, rephrase.*

A: We have removed the last two sentences from this paragraph and have written instead: "Deviations from observed sea-ice conditions in specific years can therefore also be a consequence of internal model variability." The main message is hereby better emphasized.

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

A: We have consistently replaced Kelvin by degrees Celsius.

C: *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.*

A: There is no salt in the ice at the top surface. 0 °C is absolutely correct.

C: *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. . . "*

A: There are differences which are here beside the point. The clause in question has been reworded and reads now ". . . with the underestimation of sea-ice concentration by about 10 % . . . "

C: *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.*

A: We followed the suggestion of Referee #1 and have replaced the subfigures of the HN1.2 and HN2.0 climatologies by their respective difference to ERA-Interim. The freezing temperature of sea water is not constant, but depends on the salinity. A single contour cannot be added. Also, it would not help understanding the differences.

C: *Pg 18, line 2: does the sea ice model include a melt-pond scheme? If so, which one? Was the same in HN1.2?*

A: The sea-ice model does not include a melt-pond scheme; but the atmosphere model applies the sea-ice albedo scheme of Køltzow (2007) which includes the ef-

fect of melt ponds as mentioned in section 2.1.2 (previously section 2.1.1). The same scheme was used in HN1.2. A detailed description of the scheme can be found in the reference paper of HN1.2 (Dorn et al., 2009).

C: *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*

A: An evaluation of precipitation in the model goes far beyond the scope of the current paper. Also, reanalysis products vary drastically in precipitation estimates over the Arctic Ocean (see Boisvert et al., 2018).

C: *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".*

A: We have added a few conclusions with respect to the performance of the revised coupling procedure and have changed the title of section 4 to "Conclusions and outlook".

C: *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.*

A: There is no model that does not need improvements anymore. Arriving at model improvements is a long way that one can go only step by step. We think that publication of intermediate steps of model developments are of value as well. The indication of model weaknesses or even model configuration errors might be a poor selling point, but is an honest way that complies with the rules of good scientific practice.

C: *Pg 20, line 3: how are the snow and ice albedo defined in HN1.2?*

A: The definition of the snow and ice albedo in HN1.2 was detailed in the reference paper of HN1.2 (Dorn et al., 2009). This has already been mentioned in section 2.1.2 (previously section 2.1.1). The few modifications in HN2.0 have been described in the same section.

C: *Pg 20, Code availability: add the link to Max Planck Institute webpage on YAC*

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

C: *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.*

A: Tables A1 and A2 are not absolutely necessary, but helpful for model users, since this paper is intended as reference paper of HN2.0.

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