Geosci. Model Dev. Discuss.,
doi:10.5194/gmd-2016-187-AC3, 2016

Geoscientific Model Development Discussions
Open Access

Interactive comment on "On the influence of sea-ice physics in multi-decadal ocean-ice hindcasts" *by* Petteri Uotila et al.

Petteri Uotila et al.

petteri.uotila@fmi.fi

Received and published: 22 December 2016

This article analyzes the effect of the new LIM3 sea ice model compared to the old LIM2 sea ice model in ocean stand-alone simulations with the new NEMO3.6 model. The results show an improvement of the sea ice representation but little effect on the rest of the ocean. Since the NEMO-ocean model is widely used in the climate modelling community and will also be the ocean component of a number of CMIP6 models, I find this comparison useful and worth to publish. The article is generally well written and organized. However, at a number of places, some more clarifications are needed and I find a few of the explanations for the differences between the model versions not entirely convincing.

Printer-friendly version

Discussion paper

Author response: We thank the reviewer for carefully reading the manuscript and for her/his constructive suggestions that significantly improved the manuscript. Please find below our responses to the reviewer's general comments and specific points.

Main points:

1. Several times, the authors state that the main objective of this study is to evaluate ocean hydrography and circulation. The manuscript in its present form does not reflect this. While the comparison of sea ice representation between LIM2 and LIM3 is done in detail, the evaluation of the ocean circulation part is rather superficial, partly because differences between NEMO-LIM2 and NEMO-LIM3 are small in the ocean away from the ice. If the ambition of the authors really is to focus mainly on the evaluation of the ocean circulation and if this article should be the major reference for the performance of the new NEMO3.6 model, much more detailed analyses are needed. However, if the main idea is to specifically focus on the effect of LIM3 and LIM2 in NEMO3.6, I would suggest to state that this article should be on: "Sea ice representation and some aspects of the ocean hydrography and circulation". In this case not much additional analysis is needed.

Author response: Thank you very much for this suggestion. Yes, our main idea is to focus on the effect of LIM3 and LIM2 in NEMO3.6. Hence, we decided to follow your suggestion and changed the title of our article to: "Comparing some aspects of the ocean hydrography, circulation and sea ice between NEMO3.6 LIM2 and LIM3". We think this new title well describes the manuscript content and matches the GMD journal requirements, as pointed out by the Editor.

We would also like to make a point that the majority of oceanic diagnostics we carried out, such as hydrography and transports, were excluded from the manuscript because they showed very small differences between LIM2 and LIM3, or the differences had similar characteristics than what the oceanic diagnostics included in the manuscript reveal. We think this is due to the fact that the largest impacts of the sea-ice model are concentrated to the upper ocean. Therefore we think that is fair to say that the oceanic analysis has had a large focus, in addition to sea-ice, although only a small part of it ended up in the present version of the manuscript due to the reasons mentioned.

2. It would help to add also subfigures of "LIM2-Obs" in the figures. It is often very difficult to really judge from LIM3-Obs and LIM3-LIM2, how much LIM3 really improved the result, especially if the colour scales for LIM3-OBS and LIM3-LIM2 are different.

Author response: A good point. We added "LIM2-Obs" panels to the figures.

3. Differences between NEMO3.6-LIM2 and NEMO3.6-LIM3 are often rather small and taken over a relatively short period (10-years). Thus, significances of the differences should be calculated and shown in the figures.

Author response: We agree. We used the t-test to estimate the 5% significances levels for average LIM3-LIM2 differences and hatched the areas of statistically significant differences in the figures.

4. The impact of the ice model on mixing, deep water formation and ocean circulation will take place through salinity changes. However, the restoring in the model (+ the prescribed atmosphere that cannot feed back onto the ocean) might hide much of this effect. Thus, the experiments without freshwater adjustments are very important in order to analyze the impact of the ice-model on the ocean and results from these *experiments should be discussed more in detail.*

Author response: This is true and we concur. We have added panels to the figures and expanded the discussion on the experiments without freshwater adjustments. Our main finding is that the LIM3–LIM2 differences are smaller than LIM3FW–LIM2FW differences, in particular in the upper ocean. However, the difference patterns are remarkably similar.

5. It should be considered to reformulate the abstract. It is not very clear, includes some, for the abstract, unnecessary information and could instead mention some more of the major results from this study.

Author response: The abstract has been reformulated. We hope it is now more clear with necessary information and major results.

Specific comments:

Abstract: 1. p1, l6: "Results of such analysis . . ..": I do not think this justification is needed in the abstract

Author response: You are right. We have removed the sentence.

2. p1, l8: Delete "while NEMO-LIM2 deviates more"

Author response: Deleted.

3. p1, l11: “skill sufficient for ocean-ice hindcasts that target oceanographic studies”: unclear, make clearer or delete

Author response: We clarified this sentence and state now that "... produced sea ice with a realism comparable to that of LIM2."

4. P1, l17-20: Since coupling to the atmosphere is mentioned, the potential effect of ice variations/ trends on atmospheric circulation should be shortly discussed as well (e.g. Barnes 2013; Francis et al. 2009; Francis and Vavrus 2012; Garcia Serrano and Frankignoul 2014; Hopsch et al. 2012; Koenigk et al. 2016; Liptak and Strong 2014; Overland and Wang 2010; Petoukhov and Semenov 2010; Screen 2014; Yang and Christensen 2012, ...). One motivation to improve sea ice models is that this might have large consequences on atmospheric climate conditions as well.

Author response: Yes, this is definitely an aspect that deserves to be mentioned. We added such a discussion to Introduction.

5. P2, l2-3: See main point 1: The main focus of this study seems to be the effect on sea ice and not on ocean circulation, which is by far less intensive analyzed in this study.

Author response: We agree, please see our answer to your Main point 1.

6. P3, l12: I thought the minimum horizontal length is a bit smaller than 50km in the Arctic near the poles, e.g. around Greenland. Please check.

Author response: You are correct. After checking the ORCA1 grid file, we found that the smallest grid cell lengths in the Arctic Ocean are between 40-50 km. We reworded the sentence to begin with "A typical horizontal..." from "The minimum horizontal..." as we want to tell the reader what the typical ORCA1 grid resolution is in the polar regions.

7. P4, l17: Please explain what is meant with a salinity restoring rate of -100mm/day. If this is a freshwater flux (global average?), this sounds very large.

Author response: The salinity restoring rate is a global negative feedback coefficient which is provided as a `namelist` parameter. The SSS restoring term should be viewed as a flux correction on freshwater fluxes to reduce the uncertainties we have on the observed freshwater budget. We added this additional information to the text. We admit that -100 mm/day is a large value. However, it is a smaller one than the default NEMO value which is -166.67 mm/day. We decided to to use the smaller value after discussions with the NEMO users of the COST EOS Ocean Synthesis action. Based on the community discussion it is likely that many NEMO users are using this, or even a higher, salinity restoring rate with ORCA1.

8. P4, l30-P5, l2: I am confused about what tuning has been done for each of the versions? Here, it is stated that a specific and optimized tuning has been done for each of the versions. In the conclusions you state; "no specific tuning has been done". I agree that two optimized model versions should be compared. In this context, I wonder if really the same effort has been done to optimize NEMO3.6LIM2 as for optimizing NEMO3.6LIM3? My worry is that the LIM2-ice-parameters have been taken from an older NEMO-LIM2 version and that the NEMO3.6-ocean parameters have been tuned with LIM3 and not with LIM2. Please describe in more detail how these two versions have been tuned and optimized.

Author response: This is a good comment and we think that the reviewer's concern regarding the LIM2–LIM3 comparison are justified. It is a very difficult one to address, because, in practice, there has been no systematic tuning procedure. As a result, the default parameter values of both sea-ice models are probably not the most optimal ones. They are, however, the default values obtained with the code and the ones that an average NEMO-LIM user is likely to end up using. Moreover, the systematic optimisation of both sea-ice models would have been a too daunting and complex task for this paper. Instead, we selected a more pragmatic approach and used the default parameter values. We think that this approach produces valuable results to the NEMO user community.

Regarding the detailed history of the LIM parameter values, we note that LIM2 has been used with the DFS forcing for about 10 years by the DRAKKAR community, mostly at 1/4° (ORCA025) resolution. The default LIM2 parameter values are a result of this exercise. Only the horizontal diffusivity (for scalability) and the EVP rheology (for numerical stability) were adjusted to the ORCA1 resolution.

The LIM3.6 default parameter values mostly come from the initial model version (Vancoppenolle et al. 2009), with some corrections on ice strength P^* and albedo following Rousset et al. (2015). Both studies used a NCEP based atmospheric forcing, so it is quite comforting, and even a bit surprising, that no specific tuning of LIM3 to DFS forcing was required.

By contrast, the LIM3SC virtual sea-ice thickness parameters were specifically tuned to match two key relationships of the multi-category version: 1) the growth rate–thickness dependence, and 2) the rate of concentration decrease versus sea-ice thickness dependence.

9. P5, l4-10: I am not sure I really understand this: Are you saying that LIM2 with P from LIM3 simulates much less ice volume but the same ice area than LIM2 with its standard P*? Why is this indicating "insignificant oceanic impacts"? Please clarify.*

Author response: We have reworded the text and decided not to mention the unclear "insignificant oceanic impacts". Instead we note that the LIM2 with its standard P^* results in a more realistic sea-ice volume which is why we decided to use it instead of the LIM2 simulation using the higher LIM3 P^*.

10. Experiments: A table listing the different simulations would be helpful

Author response: This is a good suggestion. We have added such a list as Table 1.

11. P5, l15: If LIM3 with 1 ice category is much better than LIM2 but physically closer to LIM2 than LIM3, what is the reason that LIM3-1IC is better? This feature is then obviously more important for an ice model than e.g. multiple ice thickness categories.

Author response: Suggested reasons for different performances between LIM2 and LIM3SC clearly point to differences between the thermodynamics parameterisations, including the latent heat reservoir. It is really hard to deduce the differences beyond this, because the thermodynamics code of the models are quite incompatible.

Regarding the second point, the sea-ice differences between LIM3 and LIM2, and LIM3 and LIM3SC are comparable and have the same sign. This indicates that the impact of multiple ice categories versus a single ice category is clear and systematic although when comparing LIM3 and LIM2 this is partially masked by additional LIM3–

LIM2 differences due to other differences in model configurations. The corresponding differences between LIM3SC and LIM2 are on the average smaller which signifies the primary importance of ice categories rather than the sea-ice thermodynamics parameterisations.

12. P6, l 24: Again, I do not have the impression that this study only shortly focuses on the sea ice and merely on sensitivity experiments and oceanographic analysis. Section 3 is the longest of all sections.

Author response: This is true, please see our answer to your Main point 1.

13. P6, l29, Figure1 b: I do not see 50% reduction in the East-Siberian Sea. Largest reduction seems to be between North Pole and northern Kara Sea. Please check.

Author response: Well spotted, we changed the text accordingly.

14. Figure 1: It would be good to show the spatial distribution of LIM3MC as well.

Author response: We added LIM3MC (now LIM3SC) spatial distributions to Figure 1.

15. Figure2: It is not really self-explanatory to call LIM3 with 1 single ice category "LIM3MC". This sounds like LIM3 with multi-ice categories. Maybe better LIM3SC.

Author response: True. LIM3MC stands for LIM3 mono-category, but since it may be confused with multi-category we follow your suggestion and use the LIM3SC abbreviation instead.

Printer-friendly version

Discussion paper

16. P 7, l34: I do not think you can explain the LIM3 low summer ice extent by the ice-albedo-feedback. The ice-albedo feedback exists in reality. Maybe you can explain the low summer ice extent by too low ice albedo in LIM3 or a too strong effect due to unrealistic distribution of ice thicknesses (e.g too much thin ice). On the other hand, the annual cycle in LIM2 is even larger than in LIM3 (which is opposite to the NH). Does not this speak against an effect of the ice categories?

Author response: Our thinking was too simplistic here. What we concluded for the spring NH sea-ice extent evolution in terms of the enhanced ice-albedo feedback due to LIM2 sub-grid-scale ice thickness distribution seem not to directly hold for the SH spring sea-ice extent evolution. We reformulated the text to explain this better:

The time series of annual mean sea-ice extent of LIM3 is rather well reproduced and closely follows observations (Figure 2d), but the sea-ice spring retreat is systematically too strong and summer extent too low. The LIM3 winter sea ice is on the average thicker than the LIM2 sea ice, while in summer their thicknesses are close to each other (not shown here). On the other hand, the average LIM3 sea-ice concentration is systematically about 1–10% smaller than the LIM2 one, even in the central ice pack. As a result, the LIM3 sea-ice extent is smaller, particularly in summer.

The processes explaining the low LIM3 summer sea-ice extent are related to (1) the steeper decline of LIM3 mean sea-ice thickness and (2) to its systematically lower sea-ice concentration. Arguably the most important process is the positive ice-albedo feedback, which is governed by the fast melting of thin ice enabling an effective penetration of solar energy into the upper ocean. Negative sea-ice–related feedbacks are the ice thickness–ice strength relationship and the ice thickness–ice

Printer-friendly version

Discussion paper

growth rate relationship which is important during the growth period. Models with sub-grid-scale ice thickness distribution have a less resistant ice pack to convergence resulting in thicker ice than a single-category model under similar conditions (Holland et al. 2006) In LIM3, this feedback exposes more open water during the melt period. In summary, the primary reason for the LIM3 low summer sea-ice extent seems to be its systematically low sea-ice concentration, and large open water fraction, which reduces the grid cell mean albedo and enhances the ice-albedo feedback. The LIM3 sub-grid-scale ice thickness distribution further enhances this feedback process, while simultaneously reducing the ice thickness–ice strength feedback.

17. P8, l21: I think your numbers are wrong. PIOMAS shows values of about 4-8x103 km3 in September and 20-25x103 km3 in early spring in the last decade. Please check your values.

Author response: Yes, the numbers were incorrect, thank you for pointing this out.

18. P8, l26: LIM2 shows a stronger negative trend as LIM3? This is thus opposite to the ice area trends?

Author response: This is the case. LIM2 has on the average thicker ice and a too small negative sea-ice extent trend. However, LIM2 has a more strongly decreasing sea-ice volume than LIM3.

19. P9, l18: Here you state that the sea ice albedo feedback is less important in the SH. I agree but this is in contradiction to the argumentation before that the stronger sea ice albedo feedback in LIM3 explains the LIM3-low summer ice extents (see point 16).

Author response: This contradiction does not exist any more, please see our response in point 16. Sea-ice albedo feedback occurs in NH and in SH, while it is more directly enhancing the spring sea-ice melt of LIM3 compared to LIM2 in the NH than in the SH.

20. Figure 3: The displacement of the Beaufort Gyre in LIM3 seems to agree with the positive ice bias and the general tendency of LIM to have the thickest ice displaced/ extended towards the Northern American coast.

Author response: This is a good observation. We have added it to the text.

21. P10, l12/13: I do not understand: ". . .at regions." Sentence uncomplete?

Author response: Yes, this sentence was incomplete. We changed it to: "LIM3 has a smaller ice extent and a lower ice concentration close to the ice edge (not shown here)."

22. P 10, L25: Yes, but on the other hand LIM3MC with constant salinity does perform quite well. Without simulations that separately analyze the impact of the different new features in LIM3, it is very speculative to argue that the prognostic sea ice salinity improved ice volume and area. The results from section 4.1 do not support the conclusion that prognostic salinity is strongly improving the ice volume/ extent. Please rethink this statement here.

Author response: We understand this and we wanted to be very careful with our wording. After rethinking we note that the impacts of the sea-ice salinity scheme appear rather small as no clear signal showing the improvements due to the prognostic sea-ice scheme emerges from our simulations. Therefore, we decided to remove the first sentence of this paragraph.

23. P12, l 22ff: 54 years are rather short from an ocean circulation point of view. A systematic decrease by 1-2 Sv in the AMOC might lead to larger effects on the ice after longer periods. Furthermore, I think the statement that "if problems appear, they are related to the coupling .." might be misinterpreted. In the uncoupled ocean-ice model, the atmosphere cannot feedback to the ocean. Thus, the effect of salinity/ freshwater changes on the ice is probably not very large. However, in a coupled system, small changes in the freshwater balance or related changes in AMOC and SST-pattern could lead to strong effects in the atmosphere, which in turn might strongly affect the ocean currents, ocean heat transports and ocean-ice coupling as well. Thus, changing the freshwater balance in the ocean could create important issues in a coupled model and could be the reason for performance issues in the coupled model. Please reformulate to avoid misunderstanding.

Author response: We agree, it is likely that the differences between the simulations continue to increase if the simulations are run further. Also, it is true that in a coupled system oceanic changes modify the atmosphere which then modifies the upper ocean characteristics. We reformulated the text and do not discuss about the coupled modelling environment any more.

24. From Fig. 5 it seems that more freshwater is transported in the East Greenland Current to the south and then further into the Labrador Sea. This might be related to the fact that there is more ice in the Greenland Sea in LIM2, which leads to a more constraint freshwater transport in the EGC in LIM2 than in LIM3. However, you relate the lower salinity in LIM2 in the Labrador Sea to more local melting. To decrease salinity this would also need a net transport of sea ice into the Labrador Sea because *stronger local ice growth and ice melt would not decrease the SSS. Did you analyze net ice-growth rates in LIM3 and LIM2?*

Author response: This is a good point again. We agree that it is reasonable to assume that the fresher LIM2 surface in the Labrador Sea is primarily related to the higher net transport of sea ice from the EGC, and not to the local freezing/melting of ice. We have reworded the text accordingly.

25. Figure 5 g: I am a bit surprised over the cold bias in NEMO3.6-LIM3 in the Fram Strait-Svalbard area. Is this related to too high ice velocities and too much ice in this area? For September, Figure1 b does not seem to indicate too much ice but maybe in the rest of the year. Please add a sentence on this.

Author response: A sentence was added. Both NEMO3.6-LIM3 and NEMO3.6-LIM2 show this cold bias (see new Figure 5i and l). We think that this is related to the DRAKKAR forcing, namely to its ERA-Interim based near-surface air temperature. Notz et al. (2013) have shown that the ERA-Interim forcing is too cold here and produces too much ice.

Notz, D., F. A. Haumann, H. Haak, J. H. Jungclaus, and J. Marotzke (2013), Arctic sea-ice evolution as modeled by Max Planck Institute for Meteorology's Earth system model, J. Adv. Model. Earth Syst., 5, doi:10.1002/jame.20016.

26. Figures 5/6 and salinity discussion: Given the fact that SSS is quite strongly restored in NEMO-LIM2 and NEMO-LIM3, can we really conclude from the small differences in SSS that the sea ice model has a small impact on the salinity distribution? Are the SSS-differences between LIM2 and LIM3 as small in the experiments without *freshwater adjustments?*

Author response: To check this we plotted SSS for the experiments without freshwater adjustments and show them below and are comparable to new Figure 5d and 6d (simulations with freshwater adjustments; if plots are not visible here, please see the uploaded pdf supplement).

/tmp/1078962910/figure-1.png /tmp/1078962910/figure-2.png

It is evident that the experiments without freshwater adjustments have larger SSS differences, and the regions of significant statistical difference are larger and may have changed. For example a region north of Greenland in the Arctic Ocean is not significantly saltier in LIM3FW than in LIM2FW although that was the case when comparing LIM3 and LIM2. We have added these notes to the salinity discussion and changed the conclusion that the sea-ice model has a small impact on the surface salinity, as this is not the case in the absence of freshwater adjustments.

27. P15, l1,2: Why is a larger Atlantic warm water inflow associated with a smaller AMOC? There is some discussion in the community how strong the AMOC is linked to the ocean heat transport into the Arctic but most studies suggest that an increased AMOC leads to increased transports of Atlantic water masses into the Arctic.

Author response: Our statement on the link between the Atlantic warm water inflow

Printer-friendly version

Discussion paper

and AMOC is a speculative one. After reading your comment it sounds counterintutive. We decided to delete this speculative statement.

28. Fig. 9: Are you sure that the 15% ice edge is at the right place and really the observed ice edge? It goes very far to the south and the east in the Greenland Sea and also in the Labrador Sea. Please check.

Author response: Well spotted, thanks. The 15% ice edge is from LIM3 and not the observed one as incorrectly stated in the caption. As we want to illustrate that the mixed layer depth is shallow under sea ice, we still show the LIM3 ice edge but state it correctly in the figure caption.

29. LIM3-Ref also indicates deep convection in the Greenland Sea far inside the ice area. Further NEMO does not show any deep convection in the Labrador Sea but the climatology does not either. Results from ARGO-floats, which cover the time period 2000-2015 (Holte et al. 2010; http://mixedlayer.ucsd.edu/) show deep convection in the Labrador Sea and might be more reliable than the climatology used in this study.

Author response: Thank you for pointing this out and mentioning the ARGO-float based MLD estimates. We added them to Figure 9. They seem to generally agree well with the deBoyer MLDs, although differences exists. As you say, the ARGO-float based estimates show deeper MLDs in the Labrador Sea than deBoyer ones, which is now mentioned in the text.

30. P16 AMOC: The observational based estimates should be mentioned: e.g. RAPID: 16.9 Sv (at 26.5N), Ganachaud (2003) and Lumpkin and Speer (2007): 18.5 Sv (at 24N) and 16.5 Sv at 48N (Ganachaud). There are many things well simulated *in NEMO, but unfortunately not the AMOC . . ..*

Author response: We agree. We added these observational estimates to the text.

31. P17, l1-3: Again, the SSS-restoring might hide differences between LIM3 and LIM2: Is the AMOC-difference between LIM3 and LIM2 the same in the experiments without freshwater adjustments?

Author response: To see this we added the AMOC time series of experiments without freshwater adjustments in Figure 10. For both LIM2 and LIM3, experiments without freshwater adjustments, LIM2FW and LIM3FW, have statistically significantly lower AMOCs at the 5% level than the ones with the freshwater adjustments. As the LIM3 AMOC is on the average smaller than the LIM2 AMOC, also LIM3FW AMOC is on the average smaller (0.7 Sv for 2003–2012) than the LIM2FW AMOC. The difference here is that LIM3–LIM2 AMOC difference in 2003–2012 is not statistically significant, while the LIM3FW–LIM2FW AMOC difference is (at the 5% level). It is reasonable to assume that the freshwater adjustments bring LIM3 and LIM2 AMOC closer. We mention this now in the text.

32. P 17, l20: I would delete "briefly". As stated before: this comparison is - if not the main - but an important part of this study.

Author response: Deleted.

33. P17, l22 and l29-30: The conclusions on the sea ice albedo puzzle me: First you argue that the better representation of the sea ice albedo feedback is the main *improvement and then you argue that the model is stable to changes in summer albedo. The first is also related to the different thickness classes but is it really a sign for realism if the model is insensitive to the change of summer albedo? How much is the summer albedo changing with the new sea ice albedo scheme? Maybe, the difference is small?*

Author response: The other reviewer was also puzzled. Our wording is misleading and provides a view that overestimates the effect of the new sea-ice albedo scheme. The LIM3 sea-ice thickness categories enhances the ice-albedo feedback than the single-category LIM2 which has been shown by Holland et al. (2006), for example, and discussed earlier. The new albedo scheme provides better transitions between the different ice types, slightly modifies the surface albedo compared to the old scheme and affects the model behaviour to a limited extent only. Therefore, the impact of the new sea-ice albedo scheme is secondary compared to the sea-ice thickness categories. We now mention this in Conclusions.

34. P 18, l3: I think it is a bit overstated to say that you evaluated the oceanic transports across major transects of the world ocean. You only looked at the AMOC and the Drake Passage. You do not show any results from ocean heat transports in the different oceans or transports into the Arctic or overflows (Denmark Strait, IcelandFaroe-Scotland).

Author response: In the manuscript, we only show the AMOC at 50–53°N and the Drake Passage volume transports, but we have calculated and compared other transports (volume, heat and salinity) as well. We decided not to include plots of these other transports in the manuscript due to their similarities between LIM2 and LIM3 or because they did not provide any important additional information, and for not to increase the number of figures too high and not to extend the manuscript too long. Specifically, we calculated the oceanic transports for AMOC across 20–23°N, 30–33°N, 40–43°N, 45–48°N and 50–53°N. Moreover, we calculated time series across the Australia–Antarctica transect, the Bering Strait, the Denmark Strait, the Drake Passage, the Florida Strait, the Gibraltar Strait, and the Greenland–Norway transect at 60°N. This has now been mentioned in the text.

Typings, etc.

P1, l7"while NEMO-LIM2 deviates more". Could be deleted, if LIM3 agrees better than it is clear that LIM2 deviates more.

Author response: Deleted.

P13, l6/ l7: "melted freshwater" sounds weird. Better: "freshwater from melted sea ice"

Author response: Changed to "freshwater melted from excessive sea ice."

P 14, l10: delete one "be"

Author response: Deleted.

P17, l13: A set . . . "was" performed.

Author response: Corrected.

P 17, l13: ". . . in the global ORCA1 grid": Add a "configuration" or "using the global ORCA1 grid.

Author response: Added.

Printer-friendly version

Discussion paper