Geosci. Model Dev. Discuss.,
doi:10.5194/gmd-2016-187-RC1, 2016

Geoscientific Model Development Discussions

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

Anonymous Referee #1

Received and published: 21 September 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.

Please find below a few more general comments, followed by a number of more 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.

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.

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.

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.

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.

Specific comments:

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

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

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

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.

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.

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.

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.

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.

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.

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

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.

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.

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.

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

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.

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?

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.

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

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).

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.

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

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.

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.

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?

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.

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?

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.

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.

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.

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 . . ..

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?

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. 3 3. 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?

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, Iceland-Faroe-Scotland).

Typings, etc.

p1, l7“while NEMO-LIM2 deviates more”. Could be deleted, if LIM3 agrees better than it is clear that LIM2 deviates more.

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

P 14, l10: delete one “be”

P17, l13: A set . . . “was” performed.

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

Interactive comment on Geosci. Model Dev. Discuss., doi:10.5194/gmd-2016-187, 2016.