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Interactive comment on "Thermodynamic and Dynamic Ice Thickness Changes in the Canadian Arctic Archipelago in NEMO-LIM2 Numerical Simulations" *by* Xianmin Hu et al.

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Thermodynamic and Dynamic Ice Thickness Changes in the Canadian Arctic Archipelago in NEMO-LIM2 Numerical Simulations

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January 18, 2018

Reply to Reviewer 3:

general comments: "This is an interesting study, comparing sea ice thickness simulations from a numerical model with landfast ice thickness observations at eight sites in the Canadian Arctic Archipelago, separating simulated changes in ice thickness into thermodynamic and dynamic contributions, and describing diurnal oscillations in ice thickness and thermal ice production. However, I feel that the purpose of the work is not clearly articulated. I suggest it could say something like "first, to evaluate the skill of a numerical model in simulating sea ice thickness by comparing the simulations with observations of landfast ice thickness at several sites in the CAA. Two features of the simulations will be then be discussed: 1) the relative importance . . .". I also feel that the paper does not make sufficiently clear the difference in the properties of the observation data versus the simulation data. The observation data represents immobile level first-year (seasonal) ice of uniform thickness that forms close to shore, and is forced by thermodynamic processes. The simulation data (page 8, line 12) gen- erally represents ice found beyond the near-shore ice and is a mixture of deformed (ridged/rafted) and level first-year ice, young ice and old (perennial) ice, is mobile for part of the year, and is forced by both thermodynamic and dynamic processes. The degree to which we should expect them to agree therefore depends on the concentration of old ice and deformed ice, differences in the timing of freezeup/breakup, etc.

I think that more detail is required to describe the skill of the model. The summary (but not the abstract) mentions the capability of capturing the seasonal cycle and amplitude of ice thickness. This would be clearer if the seasonal cycles were plotted as in Howell et al. (2016). In addition, such a plot would more clearly show the differences/agreement between model results and observations at Resolute and Cambridge Bay. Perhaps the dynamic processes in Figures 4 and 5 could then be used to explain, in part, these differences. Does the model have any significant skill with respect to interannual variability (or does it not, because of snow depth variations on small horizontal scales)?"

We thank reviewer #3 for the comment about the differences between different types of sea ice, particularly on the site observations ("immobile level first year ice"). This helps us a lot in understanding the discrepancies between the simulated and observed ice thickness. We added the related text both in the data section and the comparison section to state the differences clearly. Please see more in our detailed answers. In the revised version, we added a new section on

the ice volume budget within different regions in our study area. Comparisons with previous studies are also included to support the inter-annual variability seen in our results.

minor comments:

- "Page 1, lines 3-6: "the model captures well the general spatial distribution . . . (âĹij4m and thicker)". While this may be true, the model was compared with landfast ice thickness observations (first year ice only, no old ice or deformed ice), that are generally not much greater than 2m. Why not describe a general comparison with published data from IceSat, CryoSat or other sources (e.g. Laxon et al., 2013; Tilling et al.,2015), which include the thicker ice types?"
We added the related references of ice thickness observations to support our statement. "Here we focus on the ice growth process between December and April of the following year. Figure 4a and 4b show the simulated ice thickness in ANHA12 at the beginning of December and at the end of April, respectively. Geographically, at the end of April, a) very thick sea ice is located in the northern CAA ($\sim$ 4 m by the end of April) with regional maximum ($> 4.5\,m$) at the openings to the Arctic Ocean. This is consistent with the ICESat and Cryosat-2 estimations (e.g., Laxon et al., 2013; Tilling et al., 2015; Kwok and Cunningham, 2015). b) less thick sea ice covers western, and central Parry Channel (just in the west of the site Resolute) and M'Clintock Channel with a thickness of 2.5 m to 3 m. These values are similar to previous obser- vations from airborne electromagnetic surveys (Haas and Howell, 2015) and satellite (Tilling et al., 2017). "
- "Page 1, lines 6-8: What is meant by "compares well"? Do you mean the seasonal cycles and amplitudes, as stated in the summary? Is agreement with first-year

landfast ice better in the south because there are low concentrations of old ice?"
To make it clear, the text has been revised to "simulated ice thickness compares reasonably (seasonal cycle and amplitudes) with weekly Environment and Climate Change Canada (ECCC) New Icethickness Program data at first-year landfast ice sites but not at the northern sites with high-concentration of old ice".

- "Page 1, line 13: Add "at two sites" after "ice fields""
  Added as suggested.

- "Page 2, line 34: "this downward trend is mostly associated with changes in snow depth". The meaning of this is not clear. Do you mean that in most cases, the down- ward trend in ice thickness is associated with a positive trend in snow depth (since ice thickness is negatively correlated with snow depth)? Only one of the cases had a significant trend in snow depth, and it was negative, not positive."

  We revised to "They found statistically significant thinning at the sites except at Resolute, and the detrended inter-annual variability is highly (negative) correlated with snow depth due to insulating effect of the snow (Brown and Cote, 1992)."

- "Page 3, line 3-4: Change "a sea ice model" to "several sea ice models"?"
  No. Here the sea ice model is refered to LIM2 sea ice model. The same sea ice model is used for all the simulations included in this study.

- "Page 6, line 12: Were three of the 11 stations omitted from the analysis because they were on lakes?"
  Yes. We inserted "The remaining three sites are on lakes (not included in our simulations)." in the revised version.

- "Page 6, line 16 and elsewhere: The paper would be much easier to read if the full names (not acronyms) were used for the station locations."
  Changed as suggested. We use full names in both table 2 and texts now in the revised version. Acronyms are now used only in figure 1 to keep it concise.

- "Page 8, line 10: The 3 sites with poor agreement between simulations and observations are in areas with significant concentrations of old ice, while the sites with reasonable agreement are in areas without (see Canadian Ice Service (2011)). Is this the basic reason for the poor agreement at the 3 sites?"
  Yes. We added as suggested. "The sites where the model produced much thicker ice are likely where significant concentration of old ice exists (CIS, 2011)."

- "Page 8, line 11: I suggest adding a plot of the seasonal cycles of the models and observations (as in Howell et al 2016, Figure 8). This would make it easier to visualize the asymmetric seasonal cycles and summarize the differences in amplitude etc. between the various models."
  Added as suggested as fig 3.

- "Page 8, line 21: "too thick sea ice". What would be a realistic sea ice thickness, based on the literature, given that there are significant concentrations of old ice in the area?"
  We have revised the texts to "At Eureka, Alert and Alert LT1 sites (Fig. 2 and 3, e, f, and g), there are clear differences between the simulated ice thickness and the observations (âĹij 2 m at Alert/Alert LT1 and âĹij 1 m at Eureka). Note neither ANHA4 or ANHA12 has the capability to resolve the difference between Alert and Alert LT1, thus, the same simulated values are shown on the figure for both sites. The differences between simulations and observations could be an initial value problem, particularly at

Eureka (Fig. 2g). However, given high concentrations of old ice are at these sites, observations represent the immobile level first-year ice only. Thus, the model and the observations may not be representing the same type of ice.

- "Page 10, line 6-7: The meaning isn't clear. "Thus, it is likely due to another physical process such as advection from surrounding areas" (?)"
  Changed as suggested.
- "Page 17: I suggest reversing the order of Figure 8 and 9, so that they are in the same order as in the text."
  Changed as suggested.

Answer to minor comments:

- "Page 1, Line 21: "overturning""
  Corrected.
- "Page 2, line 17: "there are still""
  Corrected.
- "Page 2, line 30: "evaluated the""
  Corrected.
- "Page 4; Table 1: "subcycling" (?)"
  Corrected.
- "Page 6, lines 16 and Table 2: Change "Carol" to "Coral"."
  Corrected.

- "Page 8, line 19: Add "(Fig. 2c and d)"."
  Added as suggested.
- "Page 8, line 20: Change "MEU" to "WEU"."
  Corrected to full name "Eureka".
- "Page 8, line 24: "green line" (add space)"
  Corrected.
- "Page 8, line 34: Add ".""
  Corrected.
- "Page 10, line 21: "just south of the site YRB" (?)"
  Changed to "just to the west of the site Resolute".
- "Page 12, line 4: "spatial""
  Corrected.
- "Page 16, line 9: "supports the notion that" (?)"
  Changed as suggested.
- "Page 19, line 4: "constraints""
  Corrected.