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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.

Anonymous Referee #3

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

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

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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) generally 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)?

Minor comments:

Page 1, lines 3-6: "the model captures well the general spatial distribution . . . ($\sim$4 m 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 2 m. Why not describe a general comparison with published data from IceSat, CryoSat or other sources (e.g. Laxon et al., 2013; Tilling et al.,

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2015), which include the thicker ice types?

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?

Page 1, line 13: Add “at two sites” after “ice fields”

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 downward 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.

Page 3, line 3-4: Change “a sea ice model” to “several sea ice models”?

Page 6, line 12: Were three of the 11 stations omitted from the analysis because they were on lakes?

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.

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?

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.

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?

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" (?)

Page 17: I suggest reversing the order of Figure 8 and 9, so that they are in the same order as in the text.

Technical comments:

Page 1, Line 21: "overturning"

Page 2, line 17: "there are still"

Page 2, line 30: "evaluated the"

Page 4; Table 1: "subcycling" (?)

Page 6, lines 16 and Table 2: Change "Carol" to "Coral".

Page 8, line 19: Add "(Fig. 2c and d)".

Page 8, line 20: Change "MEU" to "WEU".

Page 8, line 24: "green line" (add space)

Page 8, line 34: Add "."

Page 10, line 21: "just south of the site YRB" (?)

Page 12, line 4: "spatial"

Page 16, line 9: "supports the notion that" (?)

Page 19, line 4: "constraints"

References

Canadian Ice Service, 2011. Sea Ice Climatic Atlas: Northern Canadian Waters (http://publications.gc.ca/site/eng/441147/publication.html)

Howell, S. E. L., Laliberté, F., Kwok, R., Derksen, C., and King, J.: Landfast ice thickness in the Canadian Arctic Archipelago from observations and models, The Cryosphere, 10, 1463-1475, https://doi.org/10.5194/tc-10-1463-2016, 2016.

Laxon S., W., K. A. Giles, A. L. Ridout, D. J. Wingham, R. Willatt, R. Cullen, R. Kwok, A. Schweiger, J. Zhang, C. Haas, S. Hendricks, R. Krishfield, N. Kurtz, S. Farrell and M. Davidson (2013), CryoSat-2 estimates of Arctic sea ice thickness and volume, Geophys. Res. Lett., 40, 732–737, doi:10.1002/grl.50193.

Tilling, R. L., Ridout, A., Shepherd, A., and Wingham, D. J.: Increased Arctic sea ice volume after anomalously low melting in 2013, Nat. Geosci., 8, 643–646, 2015.

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