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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 #2

Received and published: 13 November 2017

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

General Review:

This manuscript compares simulated sea-ice thickness profiles with those from observation from a handful of locations in the Canadian Arctic Archipelago. I have a hard time pinpointing the overall purpose of the manuscript. The observations seems to indicate largely thermodynamic growth and melt i.e. relatively smooth seasonal cycles with production of 1.5 - 2m of ice during the winter which then melts out during the summer. The model simulations seem to capture this well at some locations, while at

other locations there are discrepancies between the obs. and the simulations. This point is discussed in passing in the manuscript; however a much more thorough explanation of this issue is of interest. This is particularly problematic considering that Dumas et al (2006) have shown that a 1-D thermodynamic model can largely recreate observed sea-ice thickness in the CAA at the same locations.

Furthermore, I find the methods used to compare the in-situ (point) observations to the model output to be unsatisfactory. The sea-ice thickness from the simulations as described by the author is a grid-cell mean - i.e. already smoothed compared to the observations. Yet, the authors go on to further smooth the model output with a 9 grid cell stencil. This issue likely does not substantially change the results of the comparison since the ice growth/melt is largely thermodynamic (relatively smooth and large decorrelation length scale). However, this choice to further smooth the fields is confusing and does not make logical sense.

The authors separate the dynamic and thermodynamic contributions to the change in sea-ice thickness during the Arctic winter. This is the most interesting part of the manuscript and should be expanded on. The results indicate a net thinning of the ice in Baffin bay due to dynamics and an associated thermodynamic growth. I suspect this is due to the formation of polynyas and the resulting first year ice production in Nares Strait region. It would be interesting if the authors could show timeseries of the separated thermodynamic and dynamic growth at some of these points to see if/when the polynya forms every year. It would also be useful to add a panel to figure 3 which shows the difference between panel a and panel b in order to see the full \Delta h field.

In my opinion, one of the interesting questions that arises from this manuscript is: why does the model produce much thicker ice at Eureka and Alert compared to the observations? Why is the magnitude of the interannual variability at Alert in some simulations/years so much greater than all other locations. The manuscript would be more relevant and useful to the community if this was investigated and a solution proposed to fix this issue.

The manuscript goes on to present a complicated wavelet analysis to study the seasonal and diurnal cycle in sea-ice thickness. This analysis is entirely unnecessary as it is expected (and obvious) that there is a seasonal cycle in sea ice thickness. Furthermore, the seasonal cycle is already clearly shown in the obs and simulations in figure 2. The analysis of the diurnal cycle dummer is also unnecessary as the conclusion is exactly what must be the case- i.e. daytime melt overwhelming slight nighttime freezing.

I would also encourage the authors to generally use a simpler and more concise sentence structure. There are many long and confusing sentences which makes it difficult to follow the authors' arguments.

Particular issues:

P1 L12-13: It is well known that thermodynamic growth of ice is inversely proportional to thickness. This is not a contribution of your work.

P2 L8: 10% of the sea-ice area? Or volume? Please clarify

P2 L20: Landfast Ice implies u=v=0, not just 100% concentration

P2 L28: Model simulations are not substitutes for in-situ observations. Your manuscript is showing discrepancies between the obs and the simulations!

P4: L17-20: There are many more recent and informative studies regarding the time scale and decay of the artificial elastic waves. From my experience, your choices of number of subcycles is far too low particularly at 1/12 degree resolution. See for instance: Lemieux et al (2012), Boullion et al (2013), Kimmritz et al (2016), Williams et al (2017).

P4 L27: How are the 33km wind fields used to force the simulations with different spatial resolutions? There seems to be many issues which could arise here.

P5 L5: It is unclear how the CORE II simulations incorporate the inter-annual variablity

of the atmospheric forcing. What is the climatological mean? This deserves more explanation.

P7 - see 3rd paragraph in general review

P8 - see 1st and 4th paragraph in general review

P8 L25 It's not clear how the data assimilation is taking place. What fields are being assimilated, and in which simulations? How does this affect the results? What if no assimilation is done?

P9 Fig 2: All of the observed timeseries look similar in this figure. Perhaps another figure showing the differences due to location would be useful.

P12-15: Full timeseries of these fields would be much more interesting to see at these locations rather than seasonal cycles. This would allow us to see if there is a correlation between particular dynamic events and the thermodynamics feedbacks that we expect. Perhaps keep the seasonal cycles as well for completeness.

P16-20: I do not see what this analysis adds to the story. We already see that there is a seasonal cycles and it must be that daytime melt outweighs nighttime freezing during the melt season.

Some grammar / technical issues:

P1 L11: A relatively small

P1 L22- P2 L4: Confusing, rephrase

P2 L13: Rephrase. Also remove the quotes around statistically significant

P2 L17: There are

P2 L24: conditions

P4 L19: "can" does not make sense here. No-slip boundary conditions define that the velocity is zero at the coast line

P6 L6: This sentence is unclear and further explanation of the assimilation process is required.

P6 L12: delete “only”

P6 L13: delete “period”

P7: L9: calculation

P9 L3: “Cambridge Bay” rather than “the Cambridge Bay”

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