The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2017-197-AC2, 2018

The Cryosphere Discussions

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

Xianmin Hu et al.

xianmin@ualberta.ca

Received and published: 17 January 2018

article graphicx float booktabs [justification=centering]caption

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

Xianmin Hu,Jingfan Sun, Ting On Chan and Paul G. Myers

January 17, 2018

Reply to Reviewer 2:
We thank reviewer 2 for pointing out various issues with our manuscript. Here are our responses.

Answer to general comments:
"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 Δ_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."

First, in our original comparison, we did not address clearly the differences between in-situ observed and simulated ice thickness. This is pointed out by #3 reviewer. The observation (ECCC site data used in this study) represents the "immobile level first-year (seasonal) ice of the uniform thickness that forms close to shore, and is forced by thermodynamic processes". Second, we have to consider the differences between 1d and 3d simulations. The on site ice thickness can be better or more easily captured by the 1d simulation, e.g., in Dumas et al. (2006). But, in 3d coupled ocean and sea ice simulations, it is very difficult to reproduce such local behavior because of the resolution of both the model and atmospheric forcing data. However, we need 3d simulations to better understand seaice processes, particularly when they are not dominated by thermodynamics and their spatial distribution.

An estimate of the skill of the model is needed but very limited time series are available for a fair comparison. Neither the interpolation or the nearest point method is perfect in such comparisons because it is essentially not resolved by such simulations. Thus, we do not think the method used in this study itself affects our results here.

The differences between the observed and simulated ice thickness also explain the reviewer's question on ice thickness at Eureka and Alert.

The polynya related questions are great and interesting questions. We think they could be further investigated in a future study. In this study, we focus more on the big picture aspects of the simulations.

For the wavelet analysis, the simulations do show the fact (seasonal and diurnal cycle) that we might have expected from the real world. Instead of thinking "it must be the case", we prefer to show and quantify it. In addition, we do think it is a good thing to see that the model can reproduce the basic physical processes because models do not always do the right thing. Thus, we still think we do have some scientific contribution in this study.

Answer to 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."
  We think this sentence should be read considering the contex of the whole abstract. We are trying to describe what we see based our analysis (thus it is part of our results), but not to declare that we are the first one who found "thermodynamic growth of ice is inversely proportional to thickness".
- "P2 L8: 10% of the sea-ice area? Or volume? Please clarify"

Added "volume" in the text.

- "P2 L20: Landfast Ice implies u=v=0, not just 100% concentration"
  Added "without motion" to the text.

- "P2 L28: Model simulations are not substitutes for in-situ observations. Your manuscript is showing discrepancies between the obs and the simulations!"
  We did not attempt to express that model simulations can replace the in-situ observations. The point here is to say why we need numerical simulations and that we need to evaluate them and understand their strengths and weaknesses.

- "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)."
  We added the suggested references."Note that recent studies (e.g., Lemieux et al., 2012; Bouillon et al., 2013; Williams et al., 2017) showed that more iterations are needed to reach a viscous-plastic (VP) solution. Without doing that, the divergence field will be affected, i.e., being noisy (Dupont, 2017, personal communication). Thus, to what degree it will impact the final averaged ice thickness will vary in space. Such an investigation in the CAA is beyond the scope of this study."

- "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."
  We added the text "These forcing fields are linearly interpolated onto model grid". This is done using the NEMO on-the-fly interpolation, which is a standard way to do this in numerical models.

- “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.”
  The CORE-II dataset provides the inter-annual atmospheric fields although with different temporal and spatial resolutions. The climatology of the data set is documented in the reference, Large and Yeager (2009). We understand the CORE-II inter-annual dataset is based on a mixture of NCEP reanalyses and satellite observations with adjustments. This is different from the CGRF (from a GEM simulation) used in other simulations involved in this study. However, the differences between the forcing fields and their impacts are not the focus of this study.

- “P7 - see 3rd paragraph in general review”
  An estimate of the skill of the model is needed but very limited time series are available for a fair comparison. Neither the interpolation or the nearest point method is perfect in such comparisons because it is essentially not resolved by such simulations. Thus, we do not think the method used in this study itself affects our results here.

- “P8 - see 1st and 4th paragraph in general review”
  First, in our original comparison, we did not address clearly the differences between in-situ observed and simulated ice thickness. This is pointed out by #3 reviewer. The observation (ECCC site data used in this study) represents the “immobile level first-year (seasonal) ice of the uniform thickness that forms close to shore, and is forced by thermodynamic processes”. The differences between the observed and simulated ice thickness also explain the reviewer’s question on ice thickness at Eureka and Alert.

- “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?”
We changed the text to “which is likely due to data assimilation in GLORYS2v3” to make it clear. Data assimilation is done only in GLORYS2v3 here, and the technical details of the data assimilation in GLORYS2v3 is documented in the reference, Masina et al., (2015). In their simulation, only the concentration field is assimilated for seaice. Basically, we are including this additional experiment to show that data assimilation can change the model behavior in the region but not necessarily make it closer to observations.

- “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.”
Different y-axis scales were used in the plots. The observations were not sampled at the same time, thus interpolation will be involved for the difference-type plot. We tried to keep to the original data as much as possible. Thus, we added “Different y-axis scales are used.” in the caption to make it clear. As well, the addition of figure 3 with the mean seasonal cycles, helps highlight the differences.

- “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.”
Agree the timeseries without averaging can help to see whether there is any interaction between the two processes but the full timeseries are hard to read on paper unless presented one row for each year. We did have one example at Resolute for 2012 only in our original draft (fig 6 in the old version, and now fig 7 in the revised version). We can add them if the editor think they are worth the space.

- “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.”
  This analysis presents information on the dominant periods of variability (thermodynamic), the lack thereof in terms of the dynamics as well as some detailed information on the details of thermodynamic changes during the break-up period. Thus, we think this material is worth retaining.

Answer to specific comments:

- “P1 L11: A relatively small”
  Changed.

- “P1 L22- P2 L4: Confusing, rephrase”
  Rephrased to “Economically, shipping through the CAA , via the Northwest Passage (NWP), is of particular interest to commercial transport between Europe and Asia because of the great distance savings compared to the current route through the Panama Canal (e.g., Howell et al., 2008; Pizzolato et al., 2016, 2014). This has been a hot topic under the context that Northern Hemisphere sea ice cover has been declining dramatically (e.g., Parkinson et al., 1999; Serreze et al., 2007; Parkinson and Cavalieri, 2008; Stroeve et al., 2008; Comiso et al., 2008; Parkinson and Comiso, 2013), especially after 2007.”

- “P2 L13: Rephrase. Also remove the quotes around statistically significant”
  The quoted words are from the original reference. We think it is the proper way to cite the original words from a reference. We rephrased the the sentence to “Reduction in the September MYI cover is also found to be -6.4%

per decade until 2008 (Howell et al., 2009). But this trend was not "yet statistically significant" due to the inflow of MYI from the Arctic Ocean, mainly via the Queen Elizabeth Islands (QEI) gates in August to September (Howell et al., 2009). With extended data in recent years (until 2016), Mudryk et al. (2017) showed that the summer MYI decline rate has almost doubled" to make it clear.

- "P2 L17: There are"
  Corrected.
- "P2 L24: conditions"
  Changed.
- "P4 L19 "can" does not make sense here. No-slip boundary conditions define that the velocity is zero at the coast line"
  Removed as requested.
- " P6 L6: This sentence is unclear and further explanation of the assimilation process is required."
  This sentence has been removed.
- "P6 L12: delete "only""
  "only" here is to address the number of observation sites is less in the New Icethickness Program compared to the original one. So we prefer to keep it.
- "P6 L13: delete "period""
  Removed.
- "P7: L9: calculation"
  Corrected.

- “P9 L3: “Cambridge Bay” rather than “the Cambridge Bay””
  Corrected.