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The Cryosphere Discussions

Interactive comment on "Seasonal and interannual variability of sea-ice state variables: Observations and predictions for landfast ice in northern Alaska and Svalbard" *by* Marc Oggier et al.

Anonymous Referee #1

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In the submitted manuscript Oggier et al have analyzed 180 fast-ice cores from Alaska and 60 ice cores from Svalbard gathered over roughly a decade. The cores are binned together by degree days (a unit the authors use instead of time to sort the cores into differing stages of the sea-ice life cycle), and various properties of the ice are discussed in regards to the sea-ice's life cycle and how much they vary from year to year. At both study locations simulations are run using the 1D CICE sea ice model, and the model output is compared to the ice core data and other measurements taken from the many measurement excursions over the years.

Given that the paper discusses sea ice in detail, it definitely falls within the scope of

TC. The novelty of the paper lies less in the data and simulations used, and more in the methods used to compare sea ice from differing times and of different thickness. The many cores in addition to the model simulations provide the authors with a wealth of data to draw conclusions from. However, I find that the authors struggle to distil new insights from this wealth of data. A lack of clear scientific questions made it difficult to judge if the methods used are suitable, and neither the introduction nor the structure of the paper give the reader a sufficient frame of reference to follow. I am unable to distinguish when the authors summarize what has already previously been known from when the authors are introducing their own results.

In addition to the missing storylines and poor flow of the paper, the figures of the manuscript are extremely busy and difficult to process. The colors chosen are difficult to distinguish and not colorblind friendly, and data is often obscured by overlapping lines/dots. A further issue is that the authors do not follow the TC data policy. I found no statements regarding the availability of the data used, nor a link or reference to the precise model version of CICE used to run the simulations.

For the reasons listed, I recommend that the paper be rejected. However, since the data itself is solid and because there are many interesting facts scattered throughout the submitted manuscript, I strongly encourage the authors to refine the aims and scope of the manuscript and then resubmit. My impression of the submitted manuscript is that it attempts to cover too many things at once.

The remainder of the review will raise some general issues I found particularly problematic, followed by detailed comments on the individual figures.

General issues

Missing questions

Currently, the paper introduction raises no questions. It simply states that data is needed, and that the authors provide data. If this is the case this paper should be reformulated as a technical description or data paper. There are a wealth of questions that could be raised. Here just two examples:

- The decade long collection of ice cores in Alaska is unique in the number of cores gathered and the time covered. However, it is unclear if the data contains additional variability in addition to the interseasonal and spatial variability due to the constantly changing participants who extracted the cores. In this paper we .....
- Reference profiles of salinity are commonly provided in normalized coordinates from ice-snow interface to ice-ocean interface (e.g. lots of citations). This approach functions poorly for first year ice which changes thickness rapidly. In this paper we will determine if providing reference profiles in meters from the ice-ocean interface is more suitable for studies of ice biogeochemistry.

Model-observation comparison

The authors failed to convey what is gained by including the model in this paper. The inclusion of the model is further complicated by the authors not cleanly separating what is used to force/tune the model versus what is used to evaluate it. The ocean heat flux was tuned to fit the ice depth, and then the ice thickness was used to evaluate the model performance? It has been known since the 60s that ice thickness is dominated by ocean heat flux, atmospheric heat flux, and snow depth. Accordingly evaluating

simulated ice depth says less about the model than the forcing data. Are the authors attempting to verify the consistency of the forcing data? All aspects of the model are studied in much greater detail in other papers, for example Lecomte et al 2013 in regards to snow. Are they attempting to evaluate the salinity parametrizations? If so they should refer to and frame their results in regards to recent research in that area, e.g. Max Thomas et al 2020 "Tracer Measurements in ....", or Jacob Buffo et al 2018 "Multiphase reactive transport and platelet Ice Accretion ..."

Similarly, why are the authors looking at ice heat capacity during melting? It is already known from basic sea-ice thermodynamics that the heat capacity is very sensitive to changes in salinity and temperature close to the melting point, no model or observations are needed to confirm this. The heat capacity also has very little impact on simulating ice melt compared to the completely dominating effect of the surface albedo. I personally found the modelling aspect of the paper very unconvincing, and would encourage the authors to figure out exactly how the model helps them convey their results.

Structure

I would highly recommend that the authors rethink their current approach of having one big results section, followed by a very long discussion section. It is also not helpful that the current results section is predominantly filled with descriptions of figures. By just describing data in "results" without a purpose the reader has no guidance what is important. And then when the authors raise points in the discussion many pages later the reader has already long forgotten what they saw. I recommend going through the scientific questions one by one, and supply the specific data needed to address each specific question as it is being discussed.

Climatology

Despite how often it is referred to I do not know what the authors mean by climatology. It initially sounds like they are attempting to provide a reference set of profiles for others to use, similar to a sea surface temperature climatology map. But by the time we have reached section 4.2., "climatology" seems to mean sufficient data to plot a yearly cycle. After rereading section 4.2.1 a few times I have come to the conclusion that the only new contributions are lines 482-486, with the rest either being obvious or previously known (Eicken 2002). I find is also very confusing that the authors do not mention more recent attempts at analyzing the salinity cycle. For example the authors cite Griewank and Notz 2015, but fail to mention that Griewank and Notz 2015 not only look at the same seasonal cycle of salinity, they even used the same ice core data from Alaska! I urge the authors to single out what their analysis provides that others can not, and properly frame their results in the context of what else has been achieved in the last decade. If the authors are not trying to provide a reference climatology they should avoid raising that expectation, and if they are they should provide and link to that data in some format that others can use.

Figures

1 Nice plot, no complaints.

2 Left subplot: red and green lines are not distinguishable by red-green colorblind people, the black dashed line is barely visible against the dark blue, why does the plot start and end so abruptly cutting off the ice core points. Right subplots: Far too many dots lie over each other obscuring what is happening. If it is important that the reader can distinguish the individual plots, make the figure big enough for this to be possible.

3 Too many lines lie over each other, with hard to distinguish colors (e.g. yellow

vs light green on white background). Image quality is poor, lines blur together when zoomed in. In subplot a the line farthest to the right seems to randomly switch from grey to orange to red and back to grey. The axis limits are poorly chosen. Temperature in b and d goes to -20 or so, but the lowest value is -11, in a) and b) salinity need only go to 11 or 12, subplots e,f,g,h, have the same issue. Using better x-axis limits would increase the distance between the individual lines, making it easier to tell them apart. Saving as a vector format would allow the reader to zoom in.

4 This figure has large amounts of redundant information and dead space. The lines which are interesting to compare to each other (e.g. salinity at 25-35 TDD from Van mijen Fjord vs Utqiagvik) are too far apart to compare easily. If only one core of data is present, while technically correct it seems misleading to label it as "max". I am not sure what data is important in the plot and what the authors are trying to convey. Perhaps this is a plot better suited as supplementary information.

5 See comments on Figure 2. Subplot c is nicely done, but has nothing in common with a and b and I would recommend treating it as it's own figure. The light blue line (0.47 from ice bottom) is hard to see.

6 This is again a very busy plot, and I struggle to find what is relevant to support what the authors are trying to convey. Like Figure 4, this feels more like supplementary information. The colorbar is maxed out in many errors hiding the values. A symmetrical log scale (e.g. matplotlib.colors.SymLogNorm for Python) might help. A more minor detail, but using different colorbars for temperature and salinity would make it easier to identify which plots are comparable to each other.

7 The actual data in the plot is very small and hard to see. Given that all 4 subplots share the same x axis it seems that stacking them vertically makes more sense

than horizontally, or not? And why are differences to cores shown as bars, while differences to mass balance sites are plotted through a scatter plot?

8 What are the dotted lines? What time period/ice depth to the X and + represent? It should be explicitly stated if the dashed or drawn through line is + or -.

9 I do not understand this plot, nor understand why it is relevant. A quick link to the relevant subsection in the figure caption could help.

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