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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.

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We sincerely thank both reviewers for the constructive comments and time devoted to discussing our manuscript. We agree with the general comments from both reviewers that the manuscript is overly long, and requires reorganization. We plan to modify our manuscript in response to the comments as detailed below. We will clarify and enhance text describing the aims and scientific goals of this paper, which are:

1. In this paper, we develop a new framework to analyze sea ice core profile data sets. We introduce (i) a dual coordinate system referencing both the snow/ice and ice/ocean interface to capture relevant processes in both upper and lower ice

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layers, and (ii) cumulative degree days (DD) as temporal reference to determine the mean, range and standard deviation of ice core profile data (i.e. a climatology of profile data).

2. We build a sea ice climatology based on existing collections of ice cores, and provide a readily available reference climatology, which will be available online in accordance with The Cryosphere requirements. Such climatology serves two goals. First, it may function as a model benchmarking tool to be used by the modeling community. To date, ice core data from Utqiagvik have been used in many scientific studies (e.g., Griewank and Notz, 2015; Petrich et al., 2013; Vancoppenolle, 2007), but have lacked a common framework for analysis and validation, making intercomparisons and quantitative assessments of model performance difficult. The framework developed here can also support sampling strategies and modeling of biogeochemical processes in and under sea ice (Steiner et al., 2016). Second, such climatology can be used to evaluate representativeness and guide growth of artificial sea ice in a range of laboratory experiments, such as in the oil-in-ice experiments of Pegau et al. (2016) or Petrich et al. (2018).We investigate the climatology in terms of mean down-core profiles and variability, examine the sources of variability (spatial, seasonal, methodological), and compare our findings with results from other published studies.

3. We focus on the benefits of the developed framework to evaluate the performance of the CICE Los Alamos sea ice model in replicating key aspects of ice core climatology, and the implications for studies of sea ice processes. The choice of the model was motivated by its wide adoption in climate system models, as well as ocean and weather forecasting models.

In order to improve the readability of the paper, we propose to restructure the manuscript based on these goals. A revised outline of our manuscript is attached as an appendix.

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 location 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 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.

RE: We hope the proposed restructuring of the manuscript outlined above addresses those general comments.

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.

RE: In the following section, reviewer's comments are shown in italic.

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.*

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RE: We agree we did not clearly identify the goals of the paper in the introduction. In the revised introduction we will highlight the four goals, previously identified, namely:

1. Introducing a new framework to analyze ice core profile data based on (i) a dual coordinate system referencing both the snow/ice and ice/ocean interface, and (ii) a DD as temporal reference.

2. Building a climatology based on the decade-long ice core collection from Utqiagvik which has been used in many scientific studies (e.g., Griewank and Notz, 2015; Petrich et al., 2013; Vancoppenolle, 2007), but has lacked a common framework for such analysis and validation. In this paper, we determined that the cumulative DD at the date of coring, rather than the date of coring are most appropriate for intercomparison and temporal classification. Also, ice core profile data are analyzed both with respect to the ice-snow and ice-ocean interface, an approach that is found to be more appropriate to aggregate and intercompare ice core profile data.

3. Investigating the climatology in terms of mean down-core profiles and variability. While the seasonal evolution of salinity profiles is well described (Malmgren, 1927; Petrich and Eicken, 2017), and the spatial variability for salinity is commonly expected to vary within 0.5 to 1 ‰; most analyses are based on single season data sets (Weeks and Lee, 1962; Eicken et al. 2002; Gough et al. 2012). In this paper we focus on two-decade-long data set, which allows us to quantify the interannual variability, and examine the sources of variability (spatial, seasonal, methodological).

4. Evaluating the CICE Los Alamos model, integrated in many coupled climate system models. Turner et al.

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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?

RE: This is a very helpful comment. We will build on the reviewer's comment to revise the paper. We thank the reviewer for pointing out the circular reasoning between the use of ice thickness to tune the ocean heat flux, and subsequent use of the ice thickness to evaluate the model performance. As stated later, we wanted to verify the model consistency. Although the ocean heat flux is adjusted to the observed ice thickness, but it is the same seasonal cycle for every year, so the modeled ice thickness still includes interannual variations independent of the ocean heat flux. Thus validation in Fig 7 is not affected by the use of ocean heat flux.

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 ..." developing

RE: The CICE Los Alamos model has been widely adopted by climate system modelers, and claims to replicate desalination with the added mushy thermodynamic component (Turner et al., 2013). Knowing how the model replicates interannual and seasonal variability in terms of salinity and temperature variables is key for research using those

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variables as proxies for further modeling (e.g., contaminant transport, biogeochemistry). To our knowledge, this paper is the first to evaluate the model performance with decade-long observation recordsFollowing the reviewer's pointer, we will include a comparison of the model performance to previous model assessments.

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.

RE: We agree, and hence we will take the reviewer guidance and remove this section, including Figure 8 and 9.

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.

RE: Following the reviewer's comment, we will restructure the manuscript. The current results and discussion sections will be reorganized into three much tighter sections.

Each one will address one of the highlighted goals. The section titles of the restructured paper follow below, with more details provided in the appendix:

1. Introduction
2. Methods
3. A spatial and temporal reference framework for analysis of ice core profile data sets
4. Interannual and seasonal variability
5. Comparison with CICE model output

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.

RE: The first goal of paper is to develop a new framework to aggregate ice core data, and propose a common framework for both model comparisons, and evaluate the representativeness of artificially grown sea ice by comparing it with data from a specific region, here Alaska and Svalbard. However, in contrast with, e.g., the availability and coverage of sea surface temperature data to produce a surface temperature climatology map, ice core data coverage is typically more limited and requires aggregation as discussed in the paper.

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).

RE: While the evolution of sea-ice salinity profiles is well described in the literature (Petrich and Eicken, 2017, Gough et al. 2012, Eicken et al. 2002, Weeks and Lee,

1962), the published studies are based on single season and single location data sets. In this paper, we analyze larger data sets, spreading across a longer period time. In addition, we developed a new analysis framework (dual interface reference horizons, DD classification of profile) that allows us to examine spatial and seasonal evolution, but also interannual variability, the latter lacking in previous studies. Finally, we included in our analysis two contrasting locations to examine variability in a broader context.

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.

RE: We appreciate the pointer to the use of the same ice core data from Utqiagvik by Griewank and Notz (2015). The motivation of our current study to develop a common and consistent framework for ice core data is in line with Griewank and Notz (2015) findings, and we will use the paper to illustrate the importance of such framework. Through curation of a broader range of data collected at Utqiagvik, the data set analyzed here is almost double the size of that used by Griewank and Notz (2015). In addition, as we were able to relate each ice core to the date of ice formation of landfast ice, we were able to directly compare the observational data to model output, with the model forced with reanalysis data for that same location. Griewank and Notz (2015) compared the observations to simulations forced at nine locations over the Arctic, which they recognized as being less than ideal. Besides, the introduction of a new method to analyze sea ice profiles, that does not require normalization of profile depth, which disproportionately stretches the upper and lower high salinity layer of thinner ice (January), relatively to thicker ice (March), allows for greater accuracy in the mean and standard deviation of salinity and temperature profiles.

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.

RE: We will make the climatology data accessible online as part of the supplemental materials, so that readers have direct access. The ice core data sets are already posted at the Arctic Data Center (Utqiagvik, https://doi.org/10.18739/A2XP6V39R) or Zenodo (Van Mijen Fjord, https://doi.org/10.5281/zenodo.3737133).

Figures:

RE: In general, we will improve the figure using a colorblind friendly color map (e.g. cividis or viridis)

1 Nice plot, no complaints. RE: Thanks.

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.

RE: On the left subplot, we will choose more appropriate colors for colorblind people, and modify the range of the x-axis in order not to cut the ice core points. For the right subplots, we will remove the color reference to the year, as we do not make use of this information in the paper.

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.

RE: We will correct the labels, and choose better axis labels, especially for the temperature.

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.

RE: We will simplify the label of the y-axis in order to highlight a, b, c, and try to remove redundant information and dead space. If data from only one core is presented, we will remove the min/max values, and display only a black line. This figure not only supports the developed framework to generate a climatology, but also highlights the limitation of data scarcity in the process. The full climatology is available as supplemental material, and the data for temperature and salinity profiles will be made available online.

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 its own figure. The light blue line (0.47 from ice bottom) is hard to see.

RE: We will split this figure into two figures, with the subplot c) treated as its own figure, which will allow us to increase the error bars.

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.

RE: We will keep the figure in the manuscript, as we feel it is important to present the difference between the modeled and observed seasonal evolution. However, we will consider the suggestion of using a symmetrical log scale, and different colormaps for temperature and salinity.

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?

RE: We will increase the size of the plot, and stack them vertically as they all share the same x-axis. This plot will be presented as supplemental material

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 -.

RE: Following the reorganization and shortening of the manuscript, we will remove this figure

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.

RE: Following the reorganization and shortening of the manuscript, we will remove this figure

Please also note the supplement to this comment:
https://tc.copernicus.org/preprints/tc-2020-52/tc-2020-52-AC1-supplement.pdf

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