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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.

Marc Oggier et al.

moggier@alaska.edu

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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

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 UtqiaÄąvik 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 following section, reviewer's comments are shown in italic.

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.

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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.

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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 comments:

1. *I believe that the manuscript is overly long, and needs to be better organized. For instance, much of the methods and results are repeated in the discussion. Most of the analysis could be reported in a much shorter and clearer manuscript.*
2. *The result section is difficult to follow, with too many details that feel a bit disorganized. I believe that the important points should be identified and better highlighted.*

3. *The discussion should focus on their contributions and less on the confirmation of previous literature. While these are sometimes worth mentioning, they are too heavily discussed, which bury their actual findings.*

Response (RE.): We hope the proposed restructuring of the manuscript outlined above addresses the first 3 general comments.

4. Many figures are too complicated or ill-described in the captions. It makes it hard to find the relevant information and to cross-validate what is described in the text. This is especially true for figures from the model section.

RE.: We will rework the figures based on specific guidance, shorten the axis label, and use colorblind friendly coloring palettes. We will also improve the captions. Some figures (8 and 9) will be eliminated to tighten the manuscript based on guidance provided.

Specific comments:

Abstract: I believe that some important contributions (e.g. a new method for sampling the ice core layers) are missing in the abstract. L14 and rest of manuscript: I would not refer to ice salinity and temperature as ice properties.

RE: We will revise the abstract to include the new method for sampling ice core. Throughout the text we will use explicitly the terms salinity and temperature rather than ice properties.

L16 and below: The version of CICE should be specified.

RE: Thanks for catching that. We will add the version no. (6).

L54-55 : Too many Âń ice properties Âż. It makes this statement vague and confusing.

RE: Throughout the text we will use explicitly the terms salinity and temperature rather than ice properties.

L58-59 : This sentence is hard to follow.

RE. In the revised manuscript, we will reformulate the sentence.

L113: "The ice growth season was overall shorter and warmer than at UtqiaÄąvik" -> *this belongs in the results section.*

RE. Thanks for the comment, we will move it to the results.

L135-140 : Was any cross-validation made between the thermistor string measurements and ice core temperature measurements? If so, it would be interesting to quantify the accuracy of the internal ice temperature measurements from ice cores. I am wondering how the extraction and handling of the cores may be influencing the temperature readings?

RE: We did some cross-validation, but we prefer not to include to keep the manuscript shorter. We propose to add a couple of sentences about this in a "sources of error" subsection within the climatology section in our reorganized manuscript.

L145-150 : Aren't the temperature readings point measurements? I am a bit confused on what this re-sampling means in terms of the temperature and salinity profiles. I think this could be clarified.

RE: We will clarify. Salinity and temperature are point measurements. Depths of temperature measurement, and salinity section depths are not homogeneous throughout all cores. Thus we had to re-sample both salinity and temperature profiles in order to compare them all together.

L154 : Has this DD method been done before? If not, I think that this is a very interesting contribution and the wording should be changed to highlight this.

RE: DD is often used to estimate sea ice growth, however, to our knowledge it has not been used to classify ice cores prior.

Section 2.3: I would like some missing information to be added in this section.

RE: we will update the methods section.

- Which version of CICE are you using?

RE: Thanks for catching that. We will indicate that we used the developer's version of CICE 5, which allows a standalone mode, while CICE 6 can only be run in the global domain. There is no difference in the parametrization of the mushy layer thermodynamics between both versions.

- There is no information on the snow layers RE: The snow cover is treated as a single layer. We will specify this in the method.

- Is the dynamical component active, or turned off? If it is active, how did you determine (and define) whether the location is land-fast?

RE: The dynamical componentis active. We define the onset of formation of landfast sea ice as the onset of formation of ice which persist until the start of spring melt. The CICE standalone mode was designed for no horizontal ice motion, so it resembles landfast sea ice with no ice advection.

Section 3: This section is tedious to read and would benefit from being reorganized to avoid back-and-forth. This is especially true for section 3.2.

RE: As stated earlier, we will reorganize this section.

L 215: What is defined as the "median standard deviations? I am confused, as it implies a distribution of stds, which are themselves statistics of a distribution. This clarification is especially important given that there are many similar comments on this later in the manuscript.

RE: Thanks for the comment. We will clarify in the revised manuscript. By median standard deviation, we refer to the median value of all the standard deviations within a DD interval.

L245 : This wording is too strong. We cannot determine the performance of a model at reproducing the trends and variability only from the envelope formed by the simulated extrema.

RE: We will reformulate in the revised manuscript.

L246-252: Much of these observations are not presented.

RE: Figure 5a displays the modeled vs observed ice thickness, while Figure 5b displays the modeled vs observed snow thickness. In the figures, we display the correlation coefficient for both locations, and gives the correlation coefficient at each location in the plot.

L253-289: These paragraphs are difficult to follow, with a bit of back and forth between the different figures, general comments and details about the different layers. It should be reorganized to focus on the important points.

RE: We will reorganize and tighten the text and focus on the more important points.

L290: "inter-annual variability between observation and model": This is strangely formulated. I think the term "time series of the differences" would be more appropriate.

RE: Thanks for the proposed formulation

L298-299: This discussion on the differences between the IMBs and the core measurement should be assessed earlier, in the observation section. E.g. with L135-140.

RE: We will discuss this earlier in the observation section.

L304: I am guessing that you mean "snow thickness"

RE: We thank the reviewer for pointing this out.

L305-311: A description of the heat capacity computation should be included in the method sections. This paragraph is also confusing and may be a few more lines would be useful to clarify this. I was not able to validate the information given in the text from

the figure.

RE: Since we will remove Figure 8, this will be no longer needed.

L313: Where is this porosity value coming from?

RE: We will add text on how porosity was computed according to Cox and Weeks' formula.

L314: I guess that you mean Figure 9a

RE: We thank the reviewer for pointing this out.

L314: What do you define as "natural variability"? The "deviation from natural variability" is confusing to me. Do you rather mean "deviation from the observations"?

RE: Yes, we mean the deviation from the observation.

L315-326: A lot of the information presented in this paragraph is not shown. I believe that the information would be better conveyed with a figure showing the measured and modeled values of brine fractions and porosity.

RE: This information is contained in Figure 9, but the figure itself as pointed out is difficult to interpret. In response to guidance on tightening the text, we will remove this section altogether.

L334-339: I am not sure that I am following this reasoning. The previous lines were sufficient, in my opinion.

RE: Thanks. We will remove those lines.

L350: missing words: with respect "to the" climatology

RE: We thank the reviewer to point this out.

L351-369: This would fit better in the method section.

RE: We will consider this.

L373-374: How do you quantify the brine loss and the layer in which it is important?

RE: We look at the difference between model and observation. In addition, we compute the brine volume fraction using Cox and Weeks' formula, and found that the porosity in the lower 10 cm of the ice cover is above 5%. This value corresponds to the porosity threshold proposed by Golden et al. (2007) which corresponds to a permeability allowing vertical brine movement. The main caveat is that bulk porosity is computed from temperature and bulk salinity.

L380-386: This is interesting and suggests that ice core measurements are limited for model validation. This could be discussed earlier when the model bias is presented. I think it would have helped making sense of figure 6.

RE: This is a good idea. We will discuss this at the end of our climatology section in our reorganized manuscript.

L393: missing words : [...] may be "due to?" differences in sample handling.

RE: We thank the reviewer for pointing this out.

L397: I would removed the ", know to be". It confused me on weather you were referring to your method or to something else.

RE: Yes. Good suggestion.

L400-405: I would have loved to read this in the method section. If the authors have more information on these differences between ice cores and thermistor string measurements, it would very interesting to include, as it would provide a better idea of the possible temperature bias.

RE: We propose to move this comment to a "sources of error" subsection within the climatology section in our reorganized manuscript.

L419-424: This feels un-necessary and out of place in this section.

RE: We will remove this text.

Section 4.1.2: The discussion on "acceptable differences" feel arbitrary and unnecessary given that it is barely mentioned later in the text. The rest of this section does not really describe uncertainties of the model, and more about the methods.

RE: We will remove this text.

L458-459: This statement is in contradiction with the lines below, where you indicate that smaller growth rates lead to thinner ice in the Van Mijen Fjord, and also at L224-226 where the temperature gradients are smaller in the ice growth season in this location. The larger growth rates are again mentioned at L529. This needs to be clarified.

RE: Yes, we will clarify in the revised document

L480: This could be shorter, as this was already described in the previous section.

RE: Yes, we will shorten this section.

L509: The end of the sentence is missing.

RE: Thanks, we will correct.

L513-517: This should be explained the first time this feature is mentioned, at L235. Is the attribution of the 0 salinity measurements to freshwater underplating speculated, or was it corroborated by other observations?

RE: We will bring this up this earlier on in the text. Limited under ice salinity measurements made at the sampling site during some of the sampling campaigns indicates that in spring freshwater underplating is common at this location.

L525: I think the "with respect to" is not the right expression... "considering"?

RE: We will follow your suggestion.

L532-542: Is this observed in your case or inferred from previous literature? This should be written in light of your results, or left out of the analysis. These speculations *should be supported or related to the results, or left out of the analysis.*

RE: Both snow ice and superimposed ice have been observed during the year the ice cores have been taken. However, since we observed preferentially a low-salinity layer at the surface, we speculate that superimposed ice is more common, and propose an explanation.

L543-546: The precipitation, snow depth or the presence in superimposed ice was not presented in the results for the Van Mijen Fjord. Is this observed or speculated?

RE: Superimposed ice and snow ice formation is very common in Van Mijen Fjord (Hoyland, 2009), and depends on local weather conditions. We decided not to include a detailed analysis of it.

L549-551: This is a very interesting and useful statement.

RE: Thanks.

L560: This is not shown in Fig. 7, but I think that it would be very useful to add this information in a figure, as the snow depth in often mentioned in the analysis.

RE: Difference between modeled and observed snow depth at maximum ice thickness is shown in Figure 7c. We will add the subplot number.

L574-575: It is unclear how land-fast ice is simulated in the model. This is an important point to cover in the method section. How was "land-fast ice" defined in the model and was it confirmed that the grid location remained land-fast during the observation periods?

RE: We will clarify that in the methods section. We run the CICE model on a single grid cell.

L592-594: It was previously mentioned that the salinity could also be underestimated by the drainage of brine even in the top layers (L480). Can it also be related to this?

RE: We do not think so. Salinity in the upper layer could be underestimated by brine drainage only in thin ice (< 30 cm), in which the porosity remains high throughout the whole ice cover.

Figures:

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

Figure 3: error in the labels (max in red, max in blue). The max and min lines are also difficult to see.

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

Figure 4: Too much text in the figure, it is hard to spot the a), b) c). 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.

Figure 5: Error bars are difficult 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.

Figure 6: Also too much text in the figure. If the upper half section of the cores are hatched when the sampling is from the ice bottom (bottom panels), why not doing the same for the lower-half sections of the cores when sampled from the top (top panels)?

RE: We will follow the suggestion, and only present the upper-half sections of the ice core in the top panels.

Figure 7: These plots are very small. I think that showing differences without known the actual value is difficult to judge whether these anomalies are important or not.

RE: We will increase the size of the plot, and as reviewer 1 suggests, stack them vertically as they all share the same x-axis.

Figure 8: I do not understand this figure... RE: Following the reorganization and shortening of the manuscript, we will remove this figure

Figure 9: I also have difficulty understanding this figure, but I suspect that I am mostly confused by the phrasing in the caption: if the plots are showing the actual values, not the differences. What means "as a function of the model bias?"

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-AC2-supplement.pdf

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Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2020-52, 2020.