

Review of:

**“Seasonal and interannual variability of sea-ice state variables: Observations and predictions for landfast ice in northern Alaska and Svalbard”**

by Marc Oggier, Hajo Eicken, Meibing Jin, Knut Høyland

This manuscript looks at a large number of ice cores sampled over 20 years to describe the seasonal and inter-annual variability of landfast ice temperature and salinity, in two different locations. They then evaluate the performance of the sea ice model CICE at reproducing these observations. The subject and content of the manuscript is well aligned with The Cryosphere Discussion, and I believe that it has potential for publication. In particular, the authors show that grouping the different cores in terms of Degree Days at the moment of sampling instead of dates improves the definition of the vertical profiles and clarify the climatology. They also show that aligning the cores from the ice bottom increases the accuracy in the measurements in the lower portion of the ice. I also think very relevant the discussion on how the field observation limitations can hinder model validation, especially for the modeling community.

However, the manuscript suffers from its lack of focus and clarity. It is overly long, and focuses too much on the validation of previous literature, or on unnecessary speculations on the presented data. In its current form, it is very difficult to extract the relevant information from the results.

For these reasons, I would consider this manuscript for publication but only after it is significantly re-written to address the following points.

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.
- 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.

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”.

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

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

L58-59 : This sentence is hard to follow.

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

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?

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.

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.

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

- Which version of CICE are you using?
- There is no information on the snow layers
- Is the dynamical component active, or turned off? If it is active, how did you determine (and define) whether the location is land-fast?

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

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.

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.

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

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 re-organised to focus on the 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.

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.

L304: I am guessing that you mean “snow thickness”

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 clarified this. I was not able to validate the information given in the text from the figure.

L313: Where is this porosity value coming from?

L314: I guess that you mean Figure 9a

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”?

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 fraction and porosity.

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

L350: missing words: *with respect* “to the” *climatology*

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

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

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.

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

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

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.

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

Section 4.1.2: The discussion on “acceptable differences” feel arbitrary and un-necessary 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.

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.

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

L509: The end of the sentence is missing.

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?

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

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.

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?

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

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 is often mentioned in the analysis.

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?

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?

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

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

Figure 5: Error bars are difficult to see

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)?

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.

Figure 8: I do not understand 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?"