

Interactive comment on “The MOSAiC ice floe: sediment-laden survivor from the Siberian shelf” by Thomas Krumpfen et al.

Anonymous Referee #1

Received and published: 21 April 2020

The MOSAiC ice floe: sediment-laden survivor from the Siberian shelf

by Thomas Krumpfen and others

Submitted to The Cryosphere Discussions

Review

April 20, 2020

Summary

This manuscript presents a detailed analysis of the origins of the sea ice found within the neighborhood of the Polarstern at the beginning of the Multidisciplinary drifting Observatory for the Study of Arctic Climate (MOSAIC) drift experiment. It is the most

C1

detailed provenance study for any region of sea ice that I know of and, as such, it is important both as a foundational paper for the vast body of sea ice work that will result from MOSAiC in the coming years and as an example of how provenance studies of drifting sea ice can be done. I congratulate the authors on preparing this manuscript so shortly after the MOSAiC floe was chosen. Overall the manuscript is concise and well written, but I have identified four areas where I feel the text is a little too brief and the reader would benefit from more detail. These are described in my major comments below and I believe they should all be easy to address.

Major Comments

1. Missing details for snow and ice thickness measurements

Section 2.5.1 omits important details regarding the measurement of snow and ice thickness. I understand that many of the details are described in the two references cited on lines 155-156, but as a foundational paper, I think it is important to provide the reader with more information. In particular, the text should state that the GEM method measures total snow-plus-ice thickness and the measured snow depth must be subtracted to derive the ice thickness. In addition, the text should describe the method used to align GEM and Magnaprobe measurements on a drifting floe and the resulting uncertainty in the ice thickness calculation

2. Missing details and uncertainty estimates for back trajectories

The estimated back trajectories for the ice in the DNR and EMR form the backbone of this manuscript. As a reader, I would therefore like to see more detailed describing the underlying methods and their associated uncertainties. I appreciate that the text includes citations for other studies that have used this method, but of the 5 citations listed on lines 99-100, only the final reference (Krumpfen et al, 2016) contains any further detail. I recommend bringing this reference forward (i.e. replacing the reference on line 99) and including more of the relevant details in the text of this manuscript. In particular, I think it would be important to describe the uncertainties in the method, how

C2

they vary during the season and how uncertainties are propagated backwards in time. It would also be helpful to illustrate this uncertainty in Figure 4, perhaps with an ellipse indicating the confidence bounds of the freeze-up region.

3. Ambiguity regarding use of high resolution satellite data to validate back trajectories

The close agreement between the back trajectory path and the black Xs in Figure 4a suggests that the uncertainties in the derived back trajectory of the MOSAiC ice are small. However, it is not clear from the text how the locations of the Xs were derived. The caption for Figure 4 states that the Xs represent locations determined from Sentinel-1, TerraSAR-X, and MODIS imagery. However, on lines 266-269, the text states that it was "extremely difficult to manually track the exact position of individual floes" and "only the approximate positions of individual floes could be determined". I recommend including further details regarding the nature of these difficulties, the manner in which approximate positions were determined, and the resulting uncertainties in ice position.

4. Location of Hass and Eicken data relative to study region

It is great to see that these early EM ice thickness data being used again for MOSAiC. However, I think it would be appropriate to either describe their location more precisely in the text (e.g. how close to the edge of the EMR) or show the location of these data on a map. For comparison with the GEM data collected here, it also be useful to state the total length of Haas and Eicken's measurement profiles.

Minor comments

Line 53 and all subsequent body text: Please add a vertical gap between each paragraph. This improves readability, particularly in longer sections with multiple paragraphs.

Line 100: Ricker et al (2018) is not included in the reference list

Line 145: Please clarify: does this mean that freeze-up and break-up are defined

C3

according to when ice concentration exceeds and reaches zero, respectively?

Line 214: I suggest replacing "following" with "subsequent" to avoid leading any readers to think that the temperature anomalies will be described in the text that follows

Line 235: Assuming the authors are following standardized WMO nomenclature, I think it would be helpful to clarify that they are using "residual ice" as a shorthand for "residual first year ice", which does not graduate to become second year ice until January 1.

Line 248: I suggest replacing "closer nearby" with "closer to its location on September 25"

Interactive comment on The Cryosphere Discuss., <https://doi.org/10.5194/tc-2020-64>, 2020.