

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

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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

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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

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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.

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

Anonymous Referee #2

Received and published: 3 May 2020

Does the paper address relevant scientific questions within the scope of TC?: Yes Does the paper present novel concepts, ideas, tools, or data?: Yes Are substantial conclusions reached?: Yes Are the scientific methods and assumptions valid and clearly outlined?: Mostly yes, but some clarification needed in places. Are the results sufficient to support the interpretations and conclusions?: Yes Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? Do the authors give proper credit to related work and clearly indicate their own new/original contribution?: Yes Does the title clearly reflect the contents of the paper?: Yes Does the abstract provide a concise and complete summary?: Yes Is the overall presentation well structured and clear?: Yes Is the language fluent and precise?: Yes Are mathematical formulae, symbols, abbreviations,

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and units correctly defined and used?: Yes Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated?: I've highlighted a few places where minor clarification would be helpful. Are the number and quality of references appropriate? Yes Is the amount and quality of supplementary material appropriate? Yes, though it might be interesting (if non-essential) to include some of the “not shown” work mentioned on line 314 here.

General comments: This is a well-written and very interesting manuscript exploring the origins of the ice floe that Polarstern was moored in for the MOSAiC expedition. Using a Lagrangian tracking technique in conjunction with a thermodynamic sea ice model, the authors establish the location of origin for the MOSAiC floe: North of the New Siberian Islands. The results described here will provide important context for future research based on MOSAiC. There are some places where the manuscript could be improved or clarified, but in general, these are minor. Occasionally, imprecise language is used – quantifying “a few” and “multiple” would be helpful, for instance. My main comment concerns the validation of the Lagrangian technique against satellite-derived floe positions. The computed trajectories appear to agree well with the satellite data (Figure 4a), but there is no measure of uncertainty in the satellite data included in this plot. Quantifying these uncertainties would be an important improvement. Additionally, neither the Lagrangian trajectories nor the satellite-derived positions have any indication of what time they were at each position in Figure 4a. Perhaps including a multicoloured trajectory line, with colour corresponding to day of year (and equivalent for the crosses) would aid the interpretation of this plot, and would give a better indication of how well the computed trajectories agree with the observations. Discussing how the discrepancy between computed trajectories and satellite observations evolves with time would provide important validation for the back-tracking results. Other places where I feel more explanation would strengthen the manuscript are detailed below in the specific comments section. I feel these comments are minor, and I look forward to seeing an updated version of the manuscript published in The Cryosphere.

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Specific comments: Line 63: Have they really only been temporarily discontinued? With Arctic sea ice continuing to decline, surely the old style of drift stations have been permanently discontinued and replaced with new approaches like MOSAiC? Figure 1a: a box highlighting the region shown in Figure 1b would be helpful. Figure 1b: The term DNR is not mentioned until line 230 of the main text – as Figure 1 is discussed before this, better not to abbreviate in the caption, or to spell out what DNR stands for ~line 70. Line 76: Key citations(s) for the ‘previously described methods’ would be useful here. Line 82: Which atmospheric reanalysis data? Line 90: Which historical forcing data? Line 100: A sentence or two briefly summarising the findings of the cited papers would be nice to better put into context what IceTrack has previously been used for. Line 107: Is the 25km grid high enough resolution to capture smaller scale circulation features (e.g. eddies)? If not, how are the results likely to be impacted by not including these? Line 109: Why 40%, rather than the 20% threshold used in Krumpen et al. (2019)? Line 125: How many ice categories / layers? Table 1: I assume the numbers in parentheses after the means are a measure of the uncertainty, but it isn’t clear from the caption whether this is standard deviation, range, or some other measure. Clarification needed. Line 187-190: I appreciate that that unavailability of oceanographic results means that it is necessary to focus on atmospherically driven processes affecting the retreat of the ice edge in 2019. However, a short overview of the oceanic processes that play an important role in general (rather than specifically 2019) would help to better put this work into context. Line 204: In Figure 2a, there is a positive thickness anomaly immediately around the New Siberian Islands. The trend for the region in general is clearly negative, but ‘negative thickness anomalies throughout the entire coastal zones’ is phrased too strongly – the exception to the trend should at least be noted. Line 245: How many is “a few” days? It would be great if an estimate is possible, if not perhaps better to drop. Line 269: Given that only approximate locations of floes could be determined from the high (how high?) resolution satellite data, it would be good to see some measure of the uncertainty attached to the crosses shown in Figure 4a, or at least discussed in the text. Figure 4: Linked to my previous comment, it would also

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be helpful to quantify how closely the satellite-derived positions match the computed trajectories. It is already clear from Figure 4a that they follow similar paths, but this doesn’t necessarily show that they’re in (approximately) the same place at the same time. Including a measure of the distance between the Xs and the corresponding point on the Lagrangian trajectories would answer this question, and would be interesting to compare to the (36 +/-20 km after 200 days) deviation found in previous work. Line 314: It would be interesting to include a brief overview of the ‘not shown’ analysis in the supplementary material. Line 318: How close was the ARK-12 cruise data “in the surroundings of the DNR” to the DNR?

Technical corrections: Line 187: I think a word is missing (e.g. “In the following section, ...”) Line 192: This reads a bit strangely. Perhaps “Ice dynamics and ice export in winter are important preconditioning mechanisms for the ice retreat in summer” would be better.

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

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