Letter to Reviewer 2

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May 28, 2019

Dear Reviewer,

our sincere thanks for taking your time to assess our paper. Your constructive and encouraging feedback is greatly appreciated. Please find our detailed response to your comments below. Your comments are marked by an **R**, our answers are marked by an **A**.

R: Title : this paper is really too short on the merging methodology and its evaluation to really consider that we would now have a "new merged optical and passive microwave dataset". I suggest you change your title to underline this is an "introduction" or "initial work" on your product.
A: We have changed the title to "The 2018 North Greenland polynya observed by a newly introduced merged optical and passive microwave sea ice concentration dataset".

R: Abstract: “20° C above the average ” → “normal”
A: We have replaced "20° C above the average" by "20° C higher than normal".

R: Introduction “The recent sea ice retreat ...” : how recent? Also, please add a citation for this sentence.
A: We specified that we refer to the summer sea ice retreat since 2007 and refer to Dai et al. (2019).

R: “The 89 GHz sensor...” : change “sensor” with “frequency channels”.
A: We have followed your recommendation and exchanged the words.

R: “3 by 5 km” this is the instantaneous field of view, the effective field of view is closer to “5 by 5 km”.
A: We added in the text that "3 by 5 km" refers to the instantaneous field of view.

R: “Also, they are insensitive towards the sea ice thickness for thicknesses above 10 cm”: this depends strongly on which frequency enters in the algorithms (Ivanova et al. 2015). 10cm might be correct for “near-90GHz” algorithms such as ASI.

A: We now explicitly refer to 89 GHz measurements and included the reference to Ivanova et al. (2015).

R: About the SAR limitations. Once cloud cover is taken into account, there is much more SAR coverage than thermal infrared. So coverage (swath width and duty cycle) is not a good argument. Automated retrieval of sea-ice from SAR is however a challenging topic, and this might be noted here.
A: We included that automated SIC retrieval from SAR data is difficult and refer to Karvonen (2014). We agree that there is more SAR coverage than thermal infrared. We have added this in the paper. However, the SAR data do still not always allow daily Arctic-wide coverage, which is a disadvantage compared to passive microwave data and our merged product. If the reviewer agrees, we would thus like to keep the argument concerning the SAR coverage in the text.

R: Too many citations of Morales-Maqueda et al. (2004) in 3-4 sentences. Rewrite.
A: The paragraph now reads "There are two types of polynyas: sensible and latent heat polynyas. Morales-Maqueda et al. (2004) describe both types of polynyas in detail. We continue with the description of latent heat polynyas since the one we investigate pertains to this type. Latent heat polynyas normally develop close to the coast due to off-shore winds and/or ocean currents which cause divergent sea ice motion. Sea ice is pushed away from the coast and new frazil/grease ice forms. Single latent heat polynyas produce up to 800 km^3 per year of sea ice (Tamura and Ohshima, 2011). Heat fluxes are typically between 300 W m^{-2} and 500 W m^{-2} (Haid and Timmermann, 2013; Martin et al., 2004)."

R: The "oldest and thickest" in the entire Arctic... I would expect sea-ice north of CAA to be older. Change to "one of the oldest and thickest"?
A: Done.

R: The box used for Figure 1 might be too large, as it includes processes in the East Greenland current. Consider using the same box as Moore et al. (2018) which is better suited (alternatively, justify your box in the text and note it include processes in the East Greenland Sea).
A: Thanks for this suggestion, we adapted the box of Moore et al. (2018). The magnitude of the time series did not change much.

R: Section 2.1.1 : here again occurences of "3 by 5 km" should be annotated. You are mixing iFoV, eFoV, sampling in the swath, and sampling on the projected grid.
A: We changed it to "5 by 5 km" and add that this refers to the effective field of view.

R: OSI SAF data; 1) if you use OSI-450, the correct citation is Lavergne et al. (2019).
A: The correct citation is used now.

R: 2) it is unclear if you use the box in polar stereographic coordinates (as shown in Fig 1), or in lat/lon box. Clarify. As noted above, consider using the same lat/lon box as Moore et al. (2018).
A: We use geographic coordinates and now state this in the text. Also, the same box as in Moore et al. (2018) is used.

R: 3) you are stitching together different products (OSI-450 and OSI-401) and should document that the time-series (of average SIC) are consistent at their transition.
A: Done.

R: 4) the uncertainty are probably not spatially independent (Lavergne et al. 2019) and your approach to error bars seem simplistic. Error bars could rather show the variability (standard deviation) of the SIC in the box, this would also be valid.

A: Thank you for pointing out that the assumption of spatial independence does not hold. We have removed the error bars. We think that, if we include the standard deviation within the box, we would also need to include error bars for the mean standard deviation of the climatology for comparison. This would, however, overload the plot in our opinion. Therefore, we suggest to add a second panel to the Figure. The second panel would show the time series of the standard deviation in the box for 2018 together with the mean standard deviation in the box between 1979 and 2017. Figure 1 would then look like this:

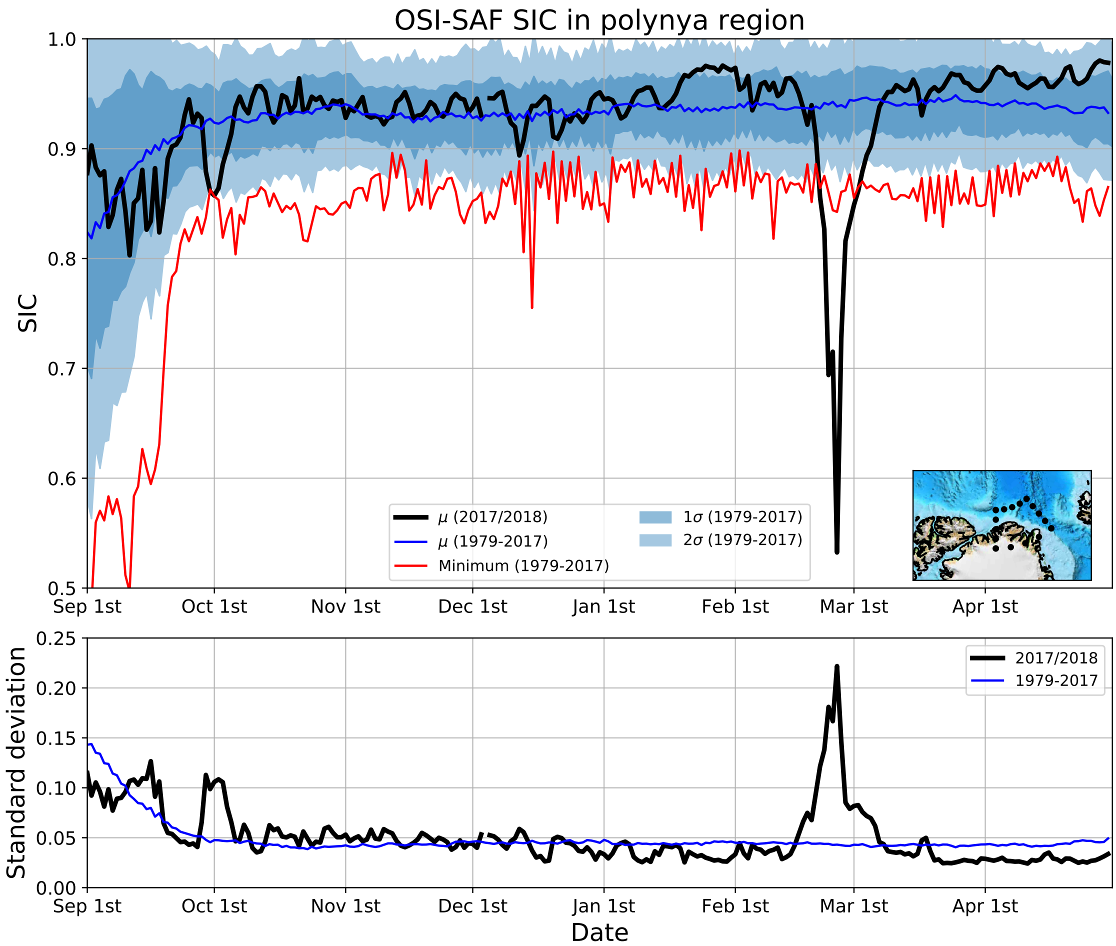


Figure 1: Upper panel: Mean OSI-SAF SIC (Lavergne et al., 2019) in the polynya region (indicated by the dashed box on the map in the lower right corner). The black line shows the mean SIC in 2018. The blue line shows the mean SIC between 1979 and 2017. The dark/light shades indicate the 1-/2σ interval, respectively. The red line shows the minimal mean SIC between 1979 and 2017 for each day. Lower panel: Time series of the standard deviation in the polynya region for 2018 (black). The blue line shows the mean of the standard deviations in the polynya region between 1979 and 2017.

R: Description of OSI-SAF sea-ice drift ; “the single-sensor sea ice drift vectors are merged by an optimal _interpolation_ scheme”. Also you could cite Lavergne et al (2010) JGRO rather than the ATBD (grey litterature).
A: Both done.

R: End of 2.2.5: “The results therefore represent the thickness of 5 weeks old first-year ice”... add “...in those specific environmental conditions”.
A: Done.

R: Move 2.2.5 to section 3 “methods” (since you compute it yourself).
A: Done.

R: However you could add a short subsection to introduce ERA5.
A: We have added the sentences "The ERA5 reanalysis is run at the European Centre for Medium-Range Weather Forecasts (ECMWF). It is the fifth generation of reanalyses from ECMWF. Hourly reanalysis data of 2 m air temperature and 10 m wind are available in near-real time at a spatial resolution of 31 km (Hersbach and Dee, 2016).". Since we describe the data in the same subsection, we decided to describe ERA5 in the same subsection and not in a separate one.

R: Section 2.2.7, please update your reference to Patilea (now published).
A: Done.

R: 2.2.8 NOASIM: “with the help of an genetic algorithm” change to “a genetic”
A: Done.

R: Please adopt a more descriptive title for section 4.
A: We changed the title to ’Sea Ice Concentration’.

R: Page 10, line 20. If you use OSI-450, the correct reference is Lavergne et al. 2019.
A: The reference was changed to Lavergne et al. (2019).

R: Line 21: there are only 30 days in April.
A: Corrected, thanks.

R: One would expect a nearly 100% average SIC north for Greenland in winter. So it is either an artefact of the OSI SAF data, or of your box that is too large and includes the East Greenland sea region. Discuss.
A: The magnitude of the OSI-SAF SIC did not change much after adapting the box of Moore et al. (2018). We visually inspected the time series of average SIC for each year of the climatology. We found that the mean was seldom close to 100 %. The years before

1987 had a slightly lower mean than the years after 1987. Before 1987, SMMR data were used, while SSM/I data were used after 1987, so that there may be a problem with the SMMR data. However, years with mean SIC close to 90 % also occurred after 1987, so that this can not be the only reason. We conclude that this is a shortcoming of the OSI-SAF SIC, but does not contradict our statement that the SIC of this year was very significantly lower than normal.

R:Also here: you stitched together two SIC products (OSI-450 and OSI-401) and should first comment the temporal consistency of the two, before comparing winter 2017/18 to the climatology.
A: Done.

R: Avoid "this year's event" and rather refer to the 2017/18 winter season explicitly.
A: Done.

R: Figure 3 : in what respect is your merged MODIS+AMSR2 SIC much better than the OSISAF SIC (Figure 1)? The OSI-SAF curve on Figure 1 also reaches 0.7 mean SIC. Maybe Mean SIC is not the most appropriate metric here, did you try "open water extent" (1 – sea-ice-extent)? And -again- your box seems very large wrt to the polynya (extending to the East Greenland Sea). Bring the OSI SAF curve from Figure 1 onto Figure 3, and discuss.
A: We have calculated the open water extent for the merged SIC and the AMSR2 SIC and included in a Figure of its own (Figure 2 in this document). The same box as in Moore et al. (2018) was used. We constrain our comparison to cloud-free pixels and to scenes where at least half of the box was covered. The open water extent is normalised to the number of cloud-free pixels. The time series in Figure 2 shows that the absolute differences are between 1 and 3 %. As the benefit of the merged product's higher resolution lies mainly in the retrieval of the leads which form when the polynya starts to open, we also show the quotient of the merged and the AMSR2 SIC during the opening phase of the polynya (lower panel in Figure 2. Here, we see that the merged product is able to retrieve up to 60 % more open water than the AMSR2 SIC alone. This reflects the leads which the AMSR2 SIC do not retrieve.
It is hard to compare this to the OSI-SAF SIC for two reasons. The first one is the low bias of the OSI-SAF SIC which we mentioned above. This makes it more likely for OSI-SAF to have a high open water extent despite its coarse resolution. The second one is that we would need to compare daily means (OSI-SAF) to single overflights (merged SIC). Since we want to focus on the benefit introduced by the merged product's higher resolution, we decided to not include the OSI-SAF and hope that you agree with us in this point.

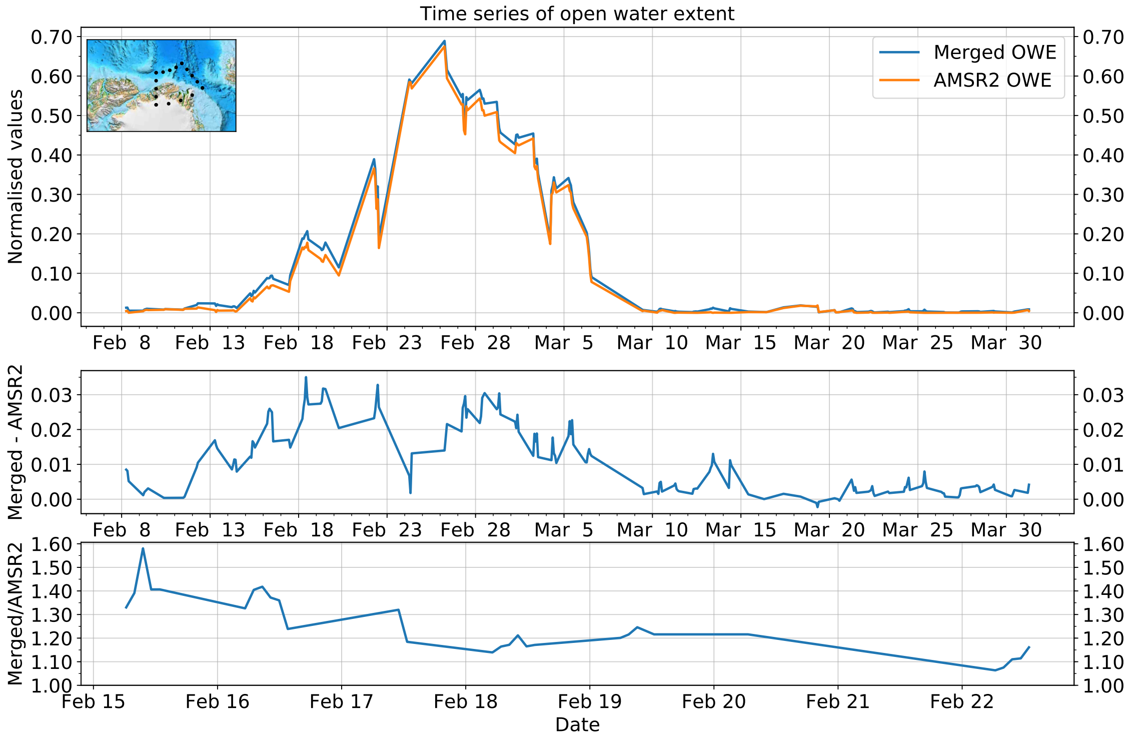
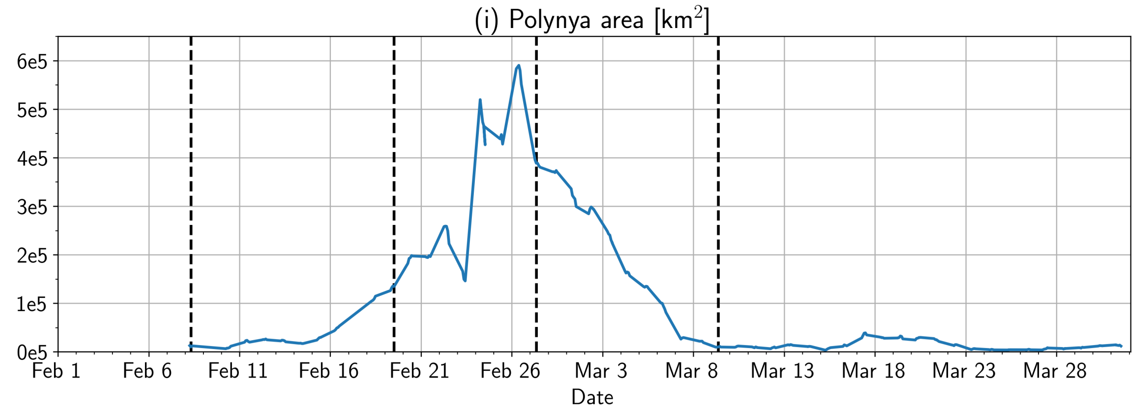


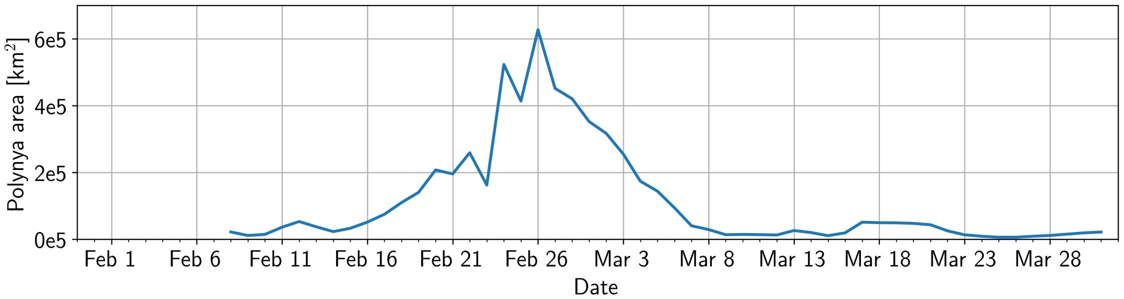
Figure 2: Time series of the merged and AMSR2 open water fraction. The upper panel shows the absolute value of the open water fraction for both datasets. The middle panel shows the difference of the merged and the AMSR2 open water fraction. The lower panel shows the quotient of the two datasets during the opening phase. Mind the different time span of the lower panel. Only points where MODIS data were available were considered.

R: Page 15, line 11-13: "Note that the area of the opening is still visible as dark/new ice in the Sentinel-1 mosaic. This shows the limitations of AMSR2 data for the observation of polynya events". I do not understand. If a uniform cover of new-ice is seen in the Sentinel-1 mosaic, then it is correct for AMSR2 SIC to show 100% SIC (irrespective of its thickness if $>$ 10cm). If one is interested in SIC, the SAR image might be hard to turn into 100% SIC, so I would see this as a limitation of the SAR technique. Explain further what you mean, or remove.
A: What we meant was that the SAR data allow identification of the polynya region even after the polynya is refrozen, while the AMSR2 SIC do not. This is, however, not the focus of our paper, so we decided to remove this sentence.

R: Figure 4: lower panel: it would be good to align the design (e.g. yticklabels, gridlines, with that of Figure 5).
A: Done. The only remaining differences are that Figure 4 shows a time series of all merged overflights while Figure 5 shows daily means. Also, the acquisition times of the four maps shown in Figure 4 are marked in Figure 4 i). The next Figure shows the new version of Figure 4i (upper panel) and Figure 5 for comparison.



(a) New version of Figure 4i: Time series of the polynya area. The polynya area is calculated as sum of the open water fraction (1 - merged SIC) in the map area, multiplied with the respective grid cell size. All available granules are shown. The acquisition times of the 4 maps shown in the paper are marked by the vertical dashed lines.



(b) Lower panel of Figure 5: Same time series as in upper panel, but with daily means instead of all granules.

Figure 3: New version of polynya area time series: The layout of Figure 4i (upper panel here) has been adapted to match that of Figure 5 (lower panel here).

R: You could consider merging together section 4 and 5 because section 4 is quite short.

A: If the reviewer agrees, we would like to keep the sections separated. We like the structure of having one section (section 4) which presents and discusses the SIC products and the polynya itself, one section (section 5) which explains how the polynya opened and one section (section 6) which focuses on the sea ice production in the polynya.

R: Section 5: "Given the time of year and the location, only anomalous sea ice drift can be the driver. Nevertheless, a warm-air intrusion (Fig. 5 and 6) contributed to maintaining the polynya open." here you are stating the conclusions of your analysis. Move them to the end of this section, after the presentation of the results.
A: We have moved them to the end, as suggested.

R: "We conclude that the sea ice must have been broken up by sea ice drift." Since Moore

et al (2018) exists, you should acknowledge them here, e.g. by stating that you confirm (or not) their conclusions.
A: We have added to Section 7 (Discussion) that our findings are consistent with those by Moore et al. (2018). They find that the sea ice thickness loss due to sea ice motion is much higher than the thermodynamical sea ice growth. Also, their time series of ice loss due to ice motion is consistent with our drift time series.

R: Figure 5: fix unit of y-axis for lower panel (should be km^2)
A: Fixed.

R: Figure 6 and 7 (d) and (e): please regrid the winds to a polar grid before plotting. Here the lat/lon original sampling is evident and disturb the interpretation (e.g. no vectors in the central Arctic Ocean).
A: We have regridded the winds.

R: Table 1 and Figure 8: consider re-stating the source of sea-ice drift information (OSI SAF) in the legend, like done in Figure 6 and 7 (ERA5).
A: Done.

R: Section 6: We need more details on how the NAOSIM, FDD, and SMOS/SMAP products are compared. The sentence "For consistency, only grid cells with a SIC minimum beneath 50% during the polynya on the respective grid are considered for the calculation. " in the caption to Figure 9 should be re-written (I did not understand what you mean), and this should be in the text of section 6 (not in the caption).
A: When comparing the modelled (NAOSIM, FDD) to the observed (SMOS/SMAP) values, we wanted to avoid interpolation from the model grid to the observation grid. We therefore defined the polynya area on each grid as the area of all pixels which had less than 50 % at least once during the polynya period. The SIC from the NAOSIM model was used for the determination of the criterion. For deriving the polynya area of the FDD thermodynamical growth, we used the ERA5 SIC. For deriving the area of the SMOS/SMAP sea ice thickness, we used ASI-AMSR2 values at 12.5 km grid spacing. This information has been added in section 2 (Data).

R: The SMOS/SMAP curve is unsettling because it first drops while the two others grow steadily. Yet, you write "The SMOS/SMAP sea ice thickness evolved synchronously to the accumulated thermodynamic sea ice growth" which is maybe correct after the re-freeze has started, but not before. We need more details on how you extracted the SMOS/SMAP curve (average over the same box)? What you probably want is to show SMOS/SMAP only there there is newly forming ice (not where there is still old sea-ice).
A: Based on the ASI-AMSR2 SIC at 12.5 km grid spacing, we derive a mask of pixels which had less than 50 % SIC at least once during the polynya event. This is the same procedure

as described in our answer above. Meaningful comparison between the SMOS/SMAP sea ice thickness and the NAOSIM and FDD sea ice thickness is only possible after the re-freezing has started and before the sea ice thickness exceeds 50 cm. We therefore plotted the SMOS/SMAP sea ice thickness in a light shade and dashed during the opening phase of the polynya and after it reached 50 cm.

R: Also, the SMOS/SMAP algorithm uses the hypothesis of 100% sea-ice cover (this is not the case during the whole event).
A: Especially for very low sea ice thickness, it is not possible to disentangle sea ice concentration and sea ice thickness. This decreases the reliability of the SMOS/SMAP sea ice thickness during the very early freeze-up. It is also partly responsible for the decrease of the SMOS/SMAP sea ice thickness while the polynya breaks up. This has been added to the paper.

R: And L-band radiometry might (or not, discuss) be affected by the warm air intrusion if there is surface melt.
A: The warm air intrusion happened mainly when the polynya was opened. Even then, surface melt likely only occurred sporadically because the 2 m air temperature was only above the freezing point occasionally (Fig. 5, upper panel). The period which is relevant for the comparison of the thermodynamical growth starts on February 25th. At this time, 2 m air temperatures were already -10/-15 °C (daily maximum/mean). Afterwards, it got even colder. We conclude that surface melt did not influence the SMOS/SMAP sea ice thickness retrieval during the phase which we focus on. A corresponding sentence has been added.

R: Please re-work this section.
A: The section has been rewritten as described above. We hope that it is better understandable now.

R: Sea-ice volume computations and Fig 9 (b): first, methodology: "by multiplying the accumulated growth rates from Fig. 9 with the area covered by the polynya": where do you find the area of the polynya? Is it from Figure 4 and 5 (lower panels)? If the case, then after March 8th the area is 0 (according to the merged product), so we would expect the volume curves to not grow anymore?
A: We have used the same area which has been used for the sea ice thickness time series in Figure 9. It was derived as the area of all pixels which have been beneath 50 % SIC at least once during the polynya event. Because this is a fixed area, the volume curves increase until the end of March.

R: Looking again at Figure 9 (b) it almost seems it is a scaling from Figure 9 (b). So did you use a fixed polynya area? If a fixed polynya area, the plot does not bring infor-

mation compared to your sentence "The freezing degree day parameterisation yields a sea ice volume of 33 km3 , NAOSIM yields a sea ice volume of 15 km3 ." Please rework the description of your "new ice volume" computation.
A: We did indeed use a fixed polynya area, therefore it is a scaling from Figure 9 (a) (we assume that you meant Figure 9 (a) and not Figure 9 (b)). We agree that this plot is not necessary and remove it.

R: Page 23 line 6: This would be Figure 9(c).
A: Thanks for pointing this out.

R: Figure 10: panels to the right with AEM. Interesting, but difficult to know where we are. Suggestion to plot a larger area or add a larger map as inlet to show where the sampled region is.
A: We have reshaped the plot and added an inset to Fig. 10a):

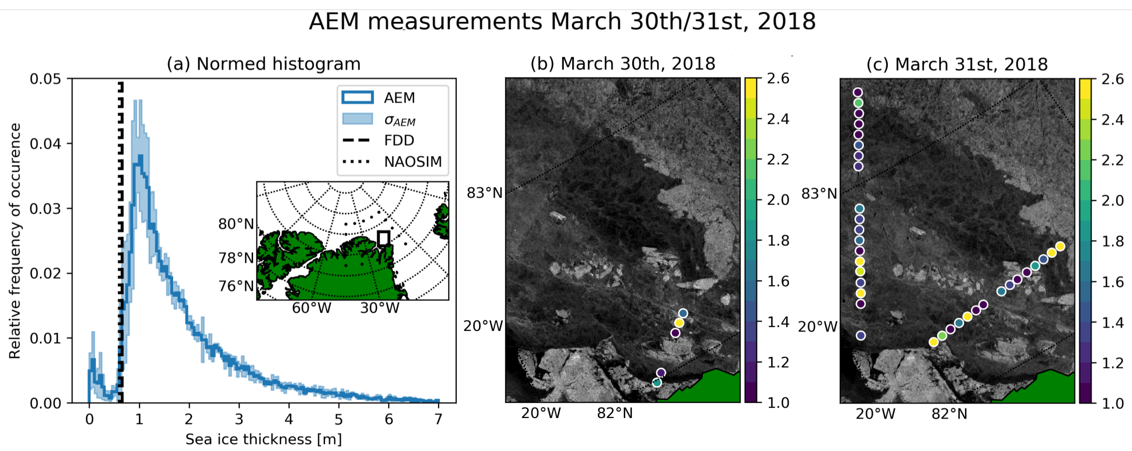


Figure 10: AEM measurements of the ice thickness of young first year ice formed in the polynya until the end of March. (a): Normalised histogram of the AEM measurements. The shades indicate the standard deviation of the three single flights. The vertical lines show the mean sea ice thickness calculated based on the freezing degree days (dashed) and the NAOSIM model (dotted) since February 14th. The dotted polygon shows the region of the polynya. The small black rectangle shows the region of the AEM flights. (b) and (c): AEM flights on March 30th and 31st. The dots show the AEM measurements, averaged every 5 km. The background shows a Sentinel-1 mosaic for the respective day.

R: Figure 10: Also, merge the two maps and use a different symbol (or a label) to show the difference of date.
A: If the reviewer agrees, we would like to keep both maps. The reasons is that they show S1 mosaics for the respective date and it seems more consistent to us to show the mosaic for the corresponding day for each flight.

R: Figure 10: The AEM frequency distribution has a first peak around 0.1m. What is this, an artefact? Describe and comment.
A: The peak is not an artefact. It is indicative of a small number of leads and very thin ice that was identified by visual inspection. We added the sentences " The small mode is caused by leads which refreeze rapidly to form dark or light nilas. This explains the presence of classes of very thin ice adjacent to the open leads."

R: Section 7, discussions: “extraordinary” is often (always?) used in a very positive sense, while you mean here this is a first-time event.
A: "an extraordinary event" has been replaced by "a first-time event".

R: Again, Moore et al. (2018) already compared with the 1978-2017 climatology, so your should put your findings in perspective of their study.
A: We have added to our paper that we confirm and extend the results of Moore et al. (2018). While they show that the mean February SIC in 2018 was lower than any mean February SIC from 1978-2018, we show that this statement holds for the entire period from October 1st to April 30th.

R: Page 29, line 30: “European Copernicus Sentinel-2” add “Union”.
A: Done.

R: Acknowledgements: since you use both SIC and SIDrift from EUMETSAT OSI SAF, you could add them in this section. Also, you could credit DMI for running (and sharing data from) the weather station.
A: Done.

We hope that we have addressed your comments in a satisfying manner and thank you again for the time you took to assess our manuscript.

Yours sincerely,
Valentin Ludwig (on behalf of the authors)

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