Response to Editor

Dear Jenny,

Please find below our answers to the two reviewers. In our answers, we have included now line numbers referring to the revised manuscript for better orientation. Further changes in the manuscript not explicitly mentioned by the reviewers were motived by shortening and editing the manuscript to improve its readability. We have extended the “data availability” section substantially and included links to the AEM thickness and drift and deformation fields published now with Pangaea.

Best regards,

Luisa (on behalf of the authors)

Response to Anonymous Referee #1

Dear Referee #1:

Thank you very much for your time and effort you put into the detailed comments on our manuscript with the title “Linking sea ice deformation to ice thickness redistribution using high-resolution satellite and airborne observations”. We believe that your suggestions will help us to improve the readability of our manuscript significantly. Please see below our answers (blue) to your comments (black) and changes in the text (green).

You asked for shortening the manuscript, but also suggested to add more information on several aspects. To find a balance between those competing demands, we decided to remove information where we think it is not strictly necessary for the main message of our manuscript, as described below. We removed:

- - The detailed description of the deformation calculation (as it only repeats existing literature). It is now found in the supplement.
  - The description of the multi-year ice (MYI) surrounding the polynya and Figure 3 b
  - Introduction: We linked the description of thermodynamic and dynamic thickness change with the one of the ITD to remove doubling of content in the introduction.
  - detailed description of the deformation history of the different zones that can be also seen on Fig.5 and the video supplement 2 (former L. 390-411)
  Detailed description of Fig. 10 (former 468-474).

At the same time, we also added more references in the manuscript to studies that provide the information you were asking for.

Specific comments:

(1) Type of polynya:

- Thanks for pointing out where essential information are still missing. We agree that it is very important to make clear that we studied the closing of a polynya that was created primarily dynamically (Moore et al. 2018). While air temperatures were rising above 0°C, Moore et al. 2018 showed that the polynya was a latent heat polynya, created by the divergent ice motion, and the warmer surface air temperatures contributed only by reducing the sea ice production. Hence, we added a sentence on the type of the polynya in the introduction (l. 1 "an unusual, large, latent heat polynya", l 61: "of an unusual, latent heat polynya that…")

- However, we are aware that our manuscript is already long, which is why we added more and clearer reference to the preceding studies (Ludwig et al. 2019, Moore et al. 2018) that dealt with the formation history of the polynya instead of describing it in more detail in our manuscript.
- We replaced the description of the most likely origin, a "unusually strong and persistent atmospheric pattern", by its effect which where "unusually strong and persist northward winds over the Greenland Sea" (l. 63)
- The large-scale drift patterns associated with the opening and closing of the polynya are presented in detail by Ludwig et al. 2019, e.g. Figure 9 a, b. Here, the authors compared the unusual drift direction end of February with the long-term mean. We have referenced this publication at the end of the sentence (l. 65).

(2) Ice Type:

Thanks for asking for a clarification on the ice type. We follow your advice and differentiate between the ice surveyed by the campaign that comprised of both, young ice and MYI, and the ice for which we calculated deformation and modeled thickness, that was only young ice. First, we removed all information on the MYI outside the refrozen polynya. Second, to differentiate between the MYI floes and the young ice, we predominantly based our assumptions on their formation history which we could reconstruct by tracking the ice backwards in time. This way, we could distinguish between ice that had formed beginning of March and MYI that drifted into the open water /was located within the open water before. We combined this information with the thickness profiles and the backscatter of the SAR images on March 31/30. We have rephrased this in see e.g., L 117 – L 124.:

(3) SAR analysis:

Thanks for your comments that helped us to identify unclear points. We add short statements based on our explanations in the manuscript. Regarding …

(1) start of drift tracking: For the start point of the tracking, we down-sampled the GPS coordinates of the airborne flight campaigns to 250 m. Gaps in the thickness observations made it necessary to increase the distance between the starting points which lead occasionally to distances of 350 m. No additional selection process based on ice type or similarly was done here. The tracking started at the down-sampled GPS coordinates. See L183-185 in the manuscript.

(2) Derivation of deformation: To calculate deformation from drift, we followed the approach widely used in literature, described in details by e.g., Kwok et al. 2003, Kwok et al. 2008, for a review: Dierking et al. 2020. As you pointed out, the manuscript is long which is why we tried to remain as concise and short as possible. We have rephrased Sect. 2.4 (drift and deformation from SAR) and moved additional derivations to the supplement (see L.160-179). Further, we added more references so that the reader can find more detailed information in the cited literature (L. 175).

(3) SAR backscatter values and the classification of the ice type:

The radar backscattering coefficients depend on frequency, polarization, incidence angle, and season (freezing, melting, and effects of melt-freeze cycles), hence also the thresholds between ice types vary. Also, the influence speckle and instrument at low backscattering levels noise has to be considered. In recent work, automated sea ice segmentation and classification is therefore carried out e.g., using statistical methods such as maximum likelihood decisions, or machine-learning methods such as neural networks. This is far beyond the scope of our study here. Grey tone variations are good proxies for separating various ice classes visually (a practice common also in operational ice charting), in particular if complementary information is available, as in our case thickness properties and deformation history as described in lines L117-124. In this context, the qualitative description of "light" MYI and "dark" young ice in the caption of Fig. 1 was only used deliberatively to give the reader a quick guide for where to look for. In respect to the naming convention of the zones, the names (Fast Ice, Shear Zone, Inner Polynya, Northern Rim) were chosen to distinguish between the four zones. They only reflect one aspect of the deformation history. For example, ice in the Fast Ice zone became quickly immobile (red trajectories in Fig. 7a). The ice in the Shear Zone experienced strong shear during March 29-31 (Fig. 7d, L.339-341). For display of the shear fields, please see the video supplement (http://doi.org/10.5446/49540).

(4) Modal thickness:
Thanks for pointing this inconsistency out to us. We made sure that figures and text agree upon this point in the revised manuscript. We recalculated the modal thickness for a bin width of 20 cm.

(5) Data:
We have published the AEM ice thickness and high-resolution drift and deformation data in the data repository Pangaea and added the links to the data availability section at the end of the text (L585-L595). There, we also provided more details on the large-scale ice drift and Operation Icebridge data that we used.

(6) Language:
We carefully revised the use of Language in our manuscript and hope that we have improved its readability.

Technical corrections:

L11: Change "by drift tracking along Lagrangian backward trajectories" –> by tracking ice drift along reverse Lagrangian trajectories to go back in time
Lagrangian ice drift trajectories backward in time
L9: We extended here a bit on the topic and rephrased this to "These closely corresponded to different deformation histories of the surveyed ice that we derived from Lagrangian ice drift trajectories backward in time. We constructed the ice drift trajectories from regularly gridded, high-resolution drift fields calculated from SAR imagery and extracted deformation derived from the drift-fields along the trajectories.

L16-17: Change "The computed ice thickness distribution resembles main characteristics like mode, e-folding, and width of the observed distribution" –> The computed ice thickness distribution resembles the main characteristics of the observed distribution including mode, e-folding, and width.
Done, L17

L17: clarify what is mean by width, do you mean "full width half maximum"?
Yes, done, L18

L19-20: Change "The similar first- and multi-year ice mean thicknesses elude to the large amount of deformation experienced by the closing polynya" –> the similarity between

the mean thickness of first- and multi-year ice suggests the scale of deformation that occurred within the closing polynya.
We removed this since we do not discuss MYI any longer.

L30-32: the use of “mean peak depth” to describe keel depths does not sound right. I suggest rewording this using a term such as modal depth. Also, I appreciate that there’s a lot of statistics presented in Strub-Klein and Sudom (2012) and so I would suggest here that you clarify which observational statistics you are quoting. I believe they relate to “maximum” sail heights and keel depths. Here you might also refer to Duncan et al. (2020) which updates and expands upon the Strub-Klein and Sudom (2012) work, and found that the average maximum sail height was 2.01 m for >17,000 ridges formed in FYI.

Thanks for the additional reference. From Strub-Klein and Sudom (2012) we cited the Arctic maximum sail heights and keel depths and referred to the largest value measured and the average (their Table 2). Since we do not want to extend the length of the manuscript, we shortened this paragraph and directed the reader to the two publications. We rephrased L34: For example, two studies reported Arctic mean maximum sail heights of 2.01-2.1m (Strub-Klein and Sudom, 2012; Duncan et al., 2020).

L45-46: Provide a suitable reference to support this statement.
We added another reference to Rampal et al., 2009.

L52: Remove “so far” since it is not needed in this sentence. Also change “changed” –> changes in
Done

L65: the text refers to the date of the maximum extent of the polynya as February 25th, but the figure (Fig 1c) shows a SAR image from March 1st.
We largest extent was on Feb 25, but we displayed March 1 in Fig. 1, because this was the date at which our drift and deformation calculations started. We have removed now this panel of Fig. 1.

L66: change “unprecedented” –> unusually
Done

L68: revise “coastward directed winds” with a vector direction
L.68: We added a vector direction: “coastward-directed, i.e., southward winds”,

L69-73: Is this a statement of work described elsewhere (i.e., previously published) or a summary of the work we are about to read about in this manuscript? If the former, provide a suitable reference; if the latter, move this statement from the introduction to the conclusion section.
We rephrased this paragraph in the following. We removed the mean and modal thickness from the introduction. We provided references for the observation that modal thickness is a good approximation of the thermodynamic thickness and moved it to L29.

L96: for completion I suggest you include the year to any dates provided within the text, in addition to month and day.

We considered this, but decided now to mention the year of the data acquisition in the introduction, data description, and figure captions. In the following text, however, we avoid repeating the year every time since it appears to be very formalistic.

L105: by "peak" do you mean modal, or maximum?
We rephrased: "The footprint smoothing underestimates the maximum ridge thickness but overestimates the ridge flanks." L100-101

L109-111: What Operation IceBridge data product for snow thickness is used here?
We use the Operation IceBridge (OIB) Sea Ice Freeboard, Snow Depth, and Thickness Quick Look data and indicated this now in L104 and in the data availability section.

What is the uncertainty associated with an airborne snow thickness observation of 4 cm?
Since there are no ground observations for this particular study site, we cannot present uncertainty estimates specifically tailored to our study case. However, King et al. (2015) found that OIB Sea Ice Freeboard, Snow Depth, and Thickness Quick Look data underestimate snow thickness by 5.3 cm in their study. We have quoted their results in our manuscript (L109).

L109-111: How much does uncertainty in snow thickness contribute to errors in the attribution of thermodynamic processes to the overall ITD?
In this paragraph we only described the contribution of the snow cover to the observed total thickness, since the laser signal is from the snow surface.
We are aware that snow has a strong effect on the thermodynamic growth of thin ice and have attributed the variability of the level ice thickness partly to this effect (see L. 280-281). For a more detailed answer on how uncertainty in snow redistribution affects our results, please see our answer to your question related to your question about L. 159-160.

L113-115: how does this assumption impact the uncertainty associated with the AEM thickness estimates, relative to that stated on L105?
We add a sentence in L.113-114: "The uncertainty of the AEM principle (0.1 m) and the snow thickness (0.04 m) add up to ,0.14m uncertainty of the AEM ice thickness measurements. "

L118-119: unfortunately this is not possible for the reader since there is no colour scale provided with the SAR data shown in Fig 1, nor is it clear what the units are.
The boundary of the young ice – MYI is identified visually based on the grey tone contrast. We found that the edge of the polynya, marked by the sharp transition of darker and brighter grey tones, was easy to identify in almost all images. We worked on backscatter data given in dB-scale, where we applied a histogram stretch for an improved visual interpretation. The knowledge of grey scale and related units is not required in this context.
We added a half sentence about the (stretched) backscatter values in dB-scale in the caption of Fig. 1 ("shown in dB-scale"). We provided an additional video supplement here (https://doi.org/10.5446/50650) to let the reader evaluate the manually created outlines.

L121-122, L149-151: I'm curious to understand what is meant by "visual interpretation of the SAR backscatter signature". Is this done using linear pixel greyness values, and therefore subjective, or by applying thresholds to the SAR data in dB? How are MYI floes defined and excluded?
Please see our answers to your general comments to the SAR analysis and the ice type, as well as the answer to your question related to L118-119. Please also refer to the manuscript in L 120-124.

L125: according to the figures, the bin width is 20 cm.

Thanks for pointing this out. We have changed this in the text to 20 cm.

L166: can you explain what is meant by “radar intensity”?
We rephrased to “radar backscatter coefficients” (L162)

L127: change “Large e-folding and FWHM” –> to large values of e-folding and FWHM

Done

L142: can you briefly describe what is meant by “two-category, zero-layer thermodynamics”?

A two-category, zero-layer thermodynamics refers to a model set-up that simulates only ice thickness and concentration, i.e. its thickness categories only consist of zero thickness (open water, given by the concentration) and mean thickness. Although there are also multi-category thickness distribution sea-ice models, the 2-category model based on Hibler (1980) is still most widely used and has proven to result in realistic simulations. The “zero-layer thermodynamics” refer to the fact that the model does not consider storage of heat in the ice. This two-category, zero-layer thermodynamics model set-up complies with a standard version of the MITgcm. Therefore, we provided several references that describe the thermodynamics of the MITgcm. We believe that adding more details in the text would unnecessarily prolong the manuscript.

L153: change “adding up” –> summing

We rephrased this paragraph and removed this part.

L153-154: does ‘t’ represent thickness, or time?

It represents time. We clarified in L243.

L159-160: Snow depth on thin ice has a large control on thermodynamic ice growth. How was thermodynamic growth impacted by snow thickness changes (and/or snow redistribution) over 30 days? Does imprecise knowledge of this impact the conclusions drawn?
The timing of snow fall events was considered in the thermodynamic modelling by forcing with precipitation from the ERA-5 reanalysis data. However, the local snow redistribution due to the wind is dependent on the ice surface topography and cannot be considered explicitly. Hence, individual trajectories (Fig. 10) include an uncertainty in the thermodynamic growth due to unknown snow cover variations, which contributes to the deviations between observed and modelled thickness. However, we based our conclusions on regionally averaged trajectories. On those larger spatial scales, we are confident that our thermodynamic estimates are valid thanks to 1) the agreement of the estimated overall thickness from the area change and the observed thickness (section 3.1) and 2) the agreement between the modal thickness of the ice and the modelled thermodynamic ice thickness in the four subregions (Tab. 1). Thus, we think that the imprecise knowledge of the snow redistribution does not impact our conclusions.

L174: Provide an example of the derived ice drift data so that the reader may evaluate the results for reasonableness

Three examples of ice drift data are displayed in Fig. 5 (arrows). We can now also provide the link to the video supplement (http://doi.org/10.5446/49540) where arrows indicate drift speed and direction. We have submitted all drift + deformation data to the data repository Pangaea where the reader may download and evaluate them as soon as it is published there.

L182: what are the variables u and v in equation 2?
They represent the x and y components of the velocity. We clarified in L.171-172.

L171: Provide an example of the derived ice deformation data so that the reader may evaluate the results for reasonableness
Three examples of ice deformation data are displayed in Fig. 5 b-d (colours) and in the video supplement (http://doi.org/10.5446/49540).

L191: Did the authors compute uncertainty in the derived divergence, shear and deformation fields?
We are aware of the different sources of uncertainty of deformation parameters, which we describe in section 2.6.1, where we explain how those propagate into our final modelled ice thickness. We did not compute uncertainty of the single deformation estimates since in particular the estimation of the tracking error requires an effort beyond the scope of this study, and directly applicable equations for the boundary definition errors have not been published yet. The uncertainty of the *drift* depends on the local conditions, and is difficult to judge for thinner, easily deformable ice. Therefore, we decided to provide a reference value based on the manual tracking of the MYI floes (described in l. 220). As major point, however, we assume that the uncertainty in thickness changes is more strongly influenced by the position errors of the reconstructed paths of ice drift than by the uncertainty of the deformation parameters.

L195: "In order to coincide with the surveyed ice" – this is quite awkward, consider rewording.
We rephrased this paragraph, see L. 181-182

L200: what is the delta time between the airborne survey and image acquisition?
The time difference was between 2 and 6 hours and we corrected for the respective drift. Also, we evaluated our correction by visually comparing the location of leads (from AEM) with the SAR images. We added in the text (L186).

L202: Change "at the time step before" –> at the previous time step
We rephrased this sentence, see 188-189

L210: how is the reliability of the tracking algorithm quantified?
We based our decision regarding the use or rejection of results on the criteria described in Hollands et al. 2015, that are the difference in backmatching and the confidence factor (CFA). The CFA consists of several quality criteria in respect to the texture of the SAR image and the correlation itself. For details we refer to their publication.

L212-213: another very awkward sentence that is hard to follow - consider rewording
Since this was only a summary, we removed the sentence.

L220-224: Can you show this assessment?
We have provided an additional figure in the supplement that presents the analysis of the reference tracks. The figure shows the difference between reference track and the calculated trajectory for

each time step. Also indicated are the mean of the differences at the first and the last time step. The dashed black line gives the assumed uncertainty for each time step as described in L. 206

L225: change “sums up” –> accumulates
We rephrased this sentence, see L. 208.

L291, L293: is there a reason why the number of combinations and iterations are reported to three significant figures? Are combinations and iterations the same thing?
Yes, combinations and iterations are the same. We rephrased this and moved it to Sect. 2.5. We are now only referring to 10 000 combinations, L. 223-224.

L306 and L332: how is open water fraction computed?
We considered all thickness observations that were thinner than the instrument thickness of 10 cm as open water. This is stated in L. 126

L307: “with most of the ice” – state the % here
Done, L. 271

L310” Change “evenly” –> even, or is there a word missing here?
Rephrased, see L. 272-273

L310: is uniform thermodynamic growth “expected” over such a large area? I think it is assumed (but not necessarily expected) since we do not know about snow distribution.
We rephrased this to “approximation”, see L. 271.

L312-313: “Deformation has led to the presence of a long tail of the distribution up to 20 m thickness” – But the scale in in Fig 3a only shows data to 8 m. What % of samples in the tail span 8 m to 20 m? Consider adding AEM profile data here to substantiate this statement (similar to the data shown in Fig 6).
We provide both profiles in the supplement. Please also note that the filtered thickness data was presented in Figure 10.

L320: Are the modes identical? Looking at Fig 3a, it appears the mode of the level ice thickness distribution is less than that of the ‘complete’ ice thickness distribution.
We have recalculated the modes and changed this in the text.

L335: Isn’t the modal thickness 1.95 m (Fig 3b)?
This value was calculated based on a smaller bin size than displayed in Fig. 3b. However, we have removed the MYI from our publication.

L560: Did the authors consider the ice thickness distribution from CryoSat-2 for this region so as to substantiate their statement?
We believe that there was a misunderstanding in how we intended this reference to the CryoSat-2 ice thickness distributions. We did not mean to say that Kwok (2015) analysed CryoSat-2 data from the former polynya. Rather we wanted to express that both in our approach, as well as in Kwok’s (2015) radar altimetry, ITDs are compiled from highly averaged data with a comparable averaging length of 300 to 1400 m. We have reformulated this sentence to make this clearer (L489-493)

L564: double “of”
Done

L586: change “we suggest to couple the SAR deformation retrievals with: : :” –> we suggest coupling deformation statistics retrieved from SAR analysis with : : :.
Done

L630, 632: change “coverage” –> cover
Done

L645-655: this sentence is quite hard to follow - consider rewording
We slightly rephrased the sentence.

Figure 1:

In 1(a) the drift trajectories (thin white lines with arrows) are not defined in the legend.
We added them.
What is the reasoning behind the uneven increments used in the color scale for ice thickness? Why, for example, is the majority of ice (according to figure 1d) combined, and represented by only one colour increment (light green) while thicker ice is divided into four increments ranging between 0.15 and 0.24 m in thickness?
We have chosen the colour scale to stress the differences in the four zones (Fast Ice, Shear Zone, Inner Polynya, Northern Rim). As described in Tab. 1 the mean of the four zones varies between 1.4 and 2.4 m. This is why we have chosen this non-linear colour scale. We added a half sentence about this in the caption.
A scale bar for the blue arrows in 1 (b) is required, showing drift magnitude.
Done
Change “stippled” to “dashed” in the figure caption.
Done
Do the data in 1(a) and (d) show ice thickness (as stated in the figure caption) or ice thickness + snow depth (as stated on L98-99)?
They state snow + ice thickness. We have changed this in the caption and the legend. Please note also L. 113

Figure 3: Indicate in the axis labels for (a) and (b) whether you show ice thickness + snow depth or ice thickness only. From my reading of the text I think (a) is the distribution of ice thickness, but (b) is the distribution of ice + snow thickness. Is the “complete” thickness distribution shown in Fig 3a repetition of the data shown in Fig 1d? If so, remove one of these duplicate figures.
Thanks for pointing out that this caused confusion. We changed the label to make clear that it is snow + ice thickness. We also removed Fig. 1d.

References used in this reply:

Dierking, W., Stern, H. L., and Hutchings, J. K.: Estimating statistical errors in retrievals of ice velocity and deformation parameters from satellite images and buoy arrays, The Cryosphere, 14, 2999–3016, https://doi.org/10.5194/tc-14-2999-2020, 2020.

Hollands, T. and Dierking, W.: Performance of a multiscale correlation algorithm for the estimation of sea-ice drift from SAR images: initial700results, Annals of Glaciology, 52, 311–317, https://doi.org/10.3189/172756411795931462, 2011

Hollands, T., Linow, S., and Dierking, W.: Reliability Measures for Sea Ice Motion Retrieval From Synthetic Aperture Radar Images, IEEE Journal of Selected Topics in Applied Earth Observations and Remote Sensing, 8, 67–75, https://doi.org/10.1109/jstars.2014.2340572, 2015.

Kwok, R.: Sea ice convergence along the Arctic coasts of Greenland and the Canadian Arctic Archipelago: Variability and extremes (1992-2014), Geophysical Research Letters, 42, 7598–7605, https://doi.org/10.1002/2015gl065462, 2015.

Kwok, R., Cunningham, G. F., and Hibler, W. D.: Sub-daily sea ice motion and deformation from RADARSAT observations, Geophysical Research Letters, 30, 2218, https://doi.org/10.1029/2003gl018723, 2003.

Kwok, R., Hunke, E. C., Maslowski, W., Menemenlis, D., and Zhang, J.: Variability of sea ice simulations assessed with RGPS kinematics, Journal of Geophysical Research, 113, https://doi.org/10.1029/2008jc004783, 2008.

Ludwig, V., Spreen, G., Haas, C., Istomina, L., Kauker, F., and Murashkin, D.: The 2018 North Greenland polynya observed by a newly introduced merged optical and passive microwave sea-ice concentration dataset, The Cryosphere, 13, 2051–2073, https://doi.org/10.5194/tc-13-2051-2019, 2019.

Moore, G. W. K., Schweiger, A., Zhang, J., and Steele, M.: What Caused the Remarkable February 2018 North Greenland Polynya?, Geophysical Research Letters, 45, 13,342–13,350, https://doi.org/10.1029/2018gl080902, 2018.

Response to Amélie Bouchat (referee #2)

Dear Amélie Bouchat,

Thank you for your very detailed and thorough review. We highly appreciate the effort you made to follow our thoughts and results. Your specific questions and suggestions will help us to improve the manuscript significantly. Please see our answers to your main and specific comments below.

Main comments:

1) Derivation of (1) drift, (2) deformation, and (3) trajectories:
Thanks for expressing your questions so clearly. It helped us a lot to identify where our description in the manuscript lacks further details. (1) The output of the pattern matching algorithm is a regularly spaced velocity field with a spatial resolution of 700x700 m. (2) We calculate deformation from this velocity field. In the manuscript we used the description of Green's theorem because this is a commonly known approach in the community, but we have realized now that this choice led to confusion, probably because it is normally used for buoys. We can simplify the Green's approach for our gridded fields (see below). For example, the 8pt ring-integral (Eq. 3) for a subset of the u-component of the velocity field is:

$$\mathbf{u} = \begin{pmatrix} u_1 & u_8 & u_7 \\ u_2 & u_9 & u_6 \\ u_3 & u_4 & u_5 \end{pmatrix}$$

$$\frac{\partial u}{\partial x} = \frac{1}{2A}\,[(u_2+u_1)(y_2-y_1)+(u_3+u_2)(y_3-y_2)+(u_4+u_3)(y_4-y_3)+ (u_5+u_4)(y_5-y_4)+(u_6+u_5)(y_6-y_5)+(u_7+u_6)(y_7-y_6)+(u_8+u_7)(y_8-y_7)+ (u_8+u_1)(y_8-y_1)]$$

Due to the regular grid, we can simplify.

$0=(y_4-y_3)=(y_5-y_4)=(y_8-y_7)=(y_1-y_8)$

$\Delta y =- (y_2-y_1) =- (y_3-y_2)=(y_6-y_5)=(y_7-y_6)$

$\Delta x = \Delta y$

A=$(2\Delta y) \times (2\Delta x)$= $4\Delta y \Delta y$
with $\Delta y = 700$ m.

Then, the derivate is:

$$\frac{\partial u}{\partial x} = \frac{1}{8\Delta y \Delta y} \Delta y[(2u_6 + u_5 + u_7) - (2u_2 + u_1 + u_3)] = \frac{(2u_6 + u_5 + u_7) - (2u_2 + u_1 + u_3)}{8\Delta y}$$

This is equivalent to calculating the convolution of u with a 3x3 Sobel kernel

$$\mathbf{k} = \begin{pmatrix} -1 & 0 & 1 \\ -2 & 0 & 2 \\ -1 & 0 & 1 \end{pmatrix}$$

and normalizing it by a factor of $8\Delta y$.

To clarify, we have:

- restructured the section 2.4 and 2.5. By combining them into one section (now section 2.4) we strengthen the point that deformation was calculated from the drift fields.
- used not the Green's theorem any longer, but the description with the convolution with the Sobel kernel. We moved the long explanation into the supplement.
- We moved the sentence "We calculated sea ice deformation from the spatially filtered velocity fields" to a more prominent location at the start of the paragraph.
- We used "Lagrangian" only with ice drift and not any longer with the term "deformation" to avoid any confusion

(3) To derive the trajectories, we do exactly as you suggested, we use the gridded velocity fields to integrate Lagrangian trajectories backward starting on the AEM profiles. We reconstruct the position of the trajectory for each time step by interpolating the regularly spaced velocity field to the location of the trajectory and adding the respective displacement (velocity*time). The trajectories were only used to identify the position of the ice within the deformation field, but not for calculating deformation. We rephrased as you suggested the Section "Lagrangian ice drift trajectories" and added a sentence at L193 "The trajectories were only used to identify the position of the ice within the deformation field, but not for calculating deformation."

2) Restructuring and text/grammar editing.
   Thanks for your detailed notes on where we could restructure and also edit the manuscript for clarity. We have restructured the following:
   - Information on ITD: We rephrased the introduction and cited literature (L. 27). We also shortened the description in the results because it is not a new information and only used to support the modelled thermodynamic thicknesses (L. 271)
   - Spatial variability in the deformation: We rephrased this as you suggested.
   - Methods: We combined the Section on Drift and Deformation to underline that deformation was calculated from the drift fields (L. 160).
   - Results: We split Section 3.1 in two subsections, one on the thermodynamic and one on the dynamic thickness change.

Specific comments:

p3 L71: "Since modal thickness is considered a good first guess for the thickness of the thermodynamically grown" Reference? If this comes from your results, then mention it.

We add a reference to studies that provide details on this interpretation of the ITD (Wadhams 1994, Throndike, Parkison, Rothrock, 1992, and add a sentence in the introduction that explains why the modal thickness is a good first estimate for the thermodynamic growth (L. 27)). We shortened the information in the results (L. 271).

p3 Figure 1:

a) Mention how the white trajectories were acquired.

We modified the caption: "Sequence of white arrows illustrates four ice drift trajectories derived from daily velocity fields (section 2.6) representing the typical south-easterly ice movement during the convergent closing of the polynya.

Insert: "20m ride" –> "20m ridge"

Done

b) The general low-resolution ice drift does not match the trajectories seen in (a). Maybe mention something about this?

For display reasons, we have chosen a coarse spacing of the arrows displaying the low-resolution drift in (b). That is why the change in drift direction from a coastward-directed (b) to a partly coast-parallel motion (a) cannot be identified in the low-resolution drift product. We extended the caption of b. "Overview map with monthly averaged, low-resolution sea ice drift in March 2018 (not showing the small-scale drift variability in the polynya)"

c) This panel does not seem useful. Could be removed and keep only the March 1st contour in red in (a)?

We have restructured Figure 1 by increasing (a) and removing (c) and (d).
Readers interested in the SAR images on March 1-March 30 are directed to the video supplement (https://doi.org/10.5446/50650).

d) "Combined ice thickness distribution of the FYI shown"... add along the AEM tracks?

Ok

p5 L125: "a bin width of 10 cm." Not sure if this will affect the results greatly, but the bin size should probably be larger than the instrument uncertainty. Also, the bin width looks like 20 cm in the figures. Can you clarify?

Thanks for pointing the inconsistency out to us. We recalculated all modal values with a bin width of 20 cm.

p6. Eq (1): Shouldn't this be h(ti+1) =...? Or at least the index of the left-hand side should be the same as the one for the denominator on the right-hand side. The summation index should also be replaced with something else than i, or instead you can replace A(ti+1), h(ti+1) with A(tn), h(tn) if n = 30 as mentioned in text.

My reasoning is as follows:
If we assume ice volume conservation over the whole 30 day period, then we have:
A(0)h(0) +Pk A(tk)_hth(tk) = A(t30)h(t30) ; with k = [0 : 29]
where A(t30), h(t30) is the final area and thickness of ice after 30 days, _hth(tk) = thermodynamical growth between tk and tk+1 (k is an index representing the days of integration) and A(0),h(0) is the initial ice thickness and concentration which are in fact both zero. So we have:
P

k A(tk)_hth(tk) = A(t30)h(t30) ; with k = [0 : 29]
or, if we pose n = 30 as mentioned in the text, then:
h(tn) =
P
k(A(tk)_hth(tk))=A(tn) ; with k = [0 : n ⍰ 1]
We can also stop the integration before the end of the 30 day period. In this case, we have: h(ti+1) =
P
k(A(tk)_hth(tk))=A(ti+1); withk = [0 : ti]

We went back to our code and agree with you that the index of the left-hand side should be the same as the one for the denominator on the right-hand side. We will modify Equation 1 to:

$$\overline{h(t_k)} = \frac{\sum_{k=1}^{n} A(t_{k-1})\Delta h_{th}(t_{k-1}, t_k)}{A(t_k)}$$

with n=34. k=0 refers to the 25th of February 2018 and k=34 to the 31st of March 2018. Dynamic thickness changes between e.g., the 25th and 26th of February are counted at time step k=1 and so on. We have also shortened this Section substantially.

p7 L175: "Outliers in the velocity data were reduced by a 3x3 point running median filter covering an area of 2.1x2.1 km."
I am not super familiar with signal pre-processing filters, but does this filter simply smooth the drift to "reduce" the outliers, or does it remove the outliers? If the drift is smoothed, then it will also affect deformation calculations later, which will then have an effect on the reconstructed thickness.

The median filter removes outlier but also smooths the velocity field. We have chosen a median filter because it is better than e.g., Gaussian smoothing at removing noise whilst preserving sharp gradients in the velocity field that represent the physical deformation zones. Physically incorrect outliers in unfiltered data would bias the results even more. We added this information in the manuscript, L. 169 “We filtered the data with a 3x3 point running median filter covering an area of 2.1x2.1 km, which efficiently reduces outliers, whilst preserving sharp gradients in the velocity field that extend over more than two pixels.”

p7 L187: "For deformations in which velocities and their gradients are small in comparison to the reference length scale, the strain rates can be linearized and transformed into two invariants of the 2D strain rate tensor"
I a not sure why this specification is needed. The shear rate and divergence can be written in terms of the trace and determinant of the 2D strain rate tensor (i.e. its invariants) making them also invariants. This is true regardless of their magnitude compared to the scale of measurements.
We agree and omit the statement. We have moved the remainder of this paragraph to the supplement.

p7 L188: "We calculated the spatial derivatives from the averaged velocity fields"
Averaged temporally and/or spatially? I don’t think this is specified in Section 2.4.
Here we refer to the spatially averaged (3pt median filter) velocity fields. We clarified this in the text (L. 171)

Also, for consistency, you should make sure that these integrals are calculated only if

the positions/drift values are obtained at similar times in your data set.
They are all calculated for the same period in time given by the two satellite scenes that were used to derive the drift fields.

It is also not clear what trajectories you are using for the positions (x,y) in the integral formulation of the strain rates. Are you seeding drifters at the cell's corner and integrating their trajectories in time using the gridded drift data set described in Section 2.4? Maybe I did not understand the format of the drift data set of Section 2.4... is it a list of trajectories with positions and velocity or is the resulting product a gridded velocity field? So far, I understood that your final drift data set is a gridded product.
Thanks for pointing out here that further clarification is needed. Our drift fields are gridded fields that result from the pattern matching using the two SAR scenes. For the positions (x,y) in the integral formulation, we use the grid points of our gridded velocity fields. This results in a regularly gridded deformation field. Please see also our detailed answer to your general comments on the derivation of drift, deformation, and trajectories.

p8 L205: You should mention what is the typical time interval used to obtain the Lagrangian trajectories/deformations here.
We add "For each time step, which was typically one day, we extracted …" (L. 191)

p8 L218: "deviation of the reconstructed trajectory"
I am confused now which trajectories we are talking about. The ones used to obtain the drift field that is then used to derive the deformation field, or the Lagrangian trajectories that were reconstructed backwards from the AEM tracks?
Thanks for pointing out that further clarification is needed. Please see our answer to your general comments on the derivation of drift, deformation, and trajectories. Deformation was calculated from the gridded drift fields. The term trajectory always refers to the Lagrangian trajectories that were reconstructed backward from the AEM tracks.

p8 L218: I am used to the term "tracking error" for the error resulting from incorrect pattern matching between two satellite images used for deriving the drift field (which will then affect the deformation estimates). But I think here you are talking about the Lagrangian position uncertainty that results from uncertainty in the drift field you used to integrate the Lagrangian trajectories backwards, and not a mis-match of patterns at the pixel-level in the satellite images. Correct? Maybe you could add this distinction here. And maybe discuss the "other" tracking error (i.e. the one resulting from incorrect pattern matching in your algorithm for obtaining the drift data set)?
Thanks for pointing out this confusion and for suggesting a better term. Yes, we describe what you termed "Lagrangian position uncertainty". We modified the text to clarify this point, see L. 200-210

p9 L229: "Those values are averaged and saved."
So you use the averaged deformation within this uncertainty range as the deformation history along the Lagrangian paths?
We have rephrased this part to clarify our procedure, see L. 205-209, 214, 221-224

p9 L239: "Hence, we calculated for every time step a forward and backward field and extracted deformation from both."
Add "a forward and backward DEFORMATION field..."
Done.

Do you average both the backward and forward deformation estimates and use that as an averaged deformation field from which you extract the deformation history along the Lagrangian trajectories (which is then also averaged in the "tracking uncertainty circle")?

(1) we extract all values (from the backward and forward deformation field).
(2) we randomly picked 10 000 combinations from the extracted values.
(3) we calculated ice thickness for all 10 000 combinations
(4) we averaged the thickness to obtain a mean thickness.

We have rephrased this part to clarify our procedure, see L. 221-224 and 254-259

Section 2.7: This section could be shortened by going straight to Eq (5) which gives the continuity equation for the mean ice thickness as done in many dynamic thermodynamic sea-ice models (e.g. Hibler 1979, Tremblay and Mysak 1997).
p10 Eq (5): Is the "dot" necessary in div(u_h) since u is a vector and h is a scalar?
Thanks for suggesting how to shorten the methods. We have followed your advice. We removed the “dot”.

p10 L266: "thermodynamic growth or melt..."
This should be a thermodynamic growth/melt RATE (i.e. _h/_t) to have units matching that of dh=dt in Eq (5).
Thanks for pointing out this inconsistency. We rephrased this (L.235).

p11 L277: "Second, we approximated the thermodynamic ice growth within the grid cell in Eq. 5 by the growth of the undisturbed, thermodynamically growing ice (see Fig. 2 a,b)." Which you estimate from the thermodynamical simulation described in Section 2.2?
Yes. We added half a sentence there, “obtained from the thermodynamic MITgcm run” (L. 245).

p11 Eq(6): Again, the units do not match. The divergence term should be multiplied by _t (assuming_hand _hth are given in meters).
Yes, this is exactly what we did. We are sorry, that we have lost the factor Δt during our writing process. We have added the term.

p11 L291: "Mean thickness converged to the first decimal after approximately 1000 iterations." It is not clear what iterations are here. I thought you have 10 000 different representations of the possible ice thickness evolution along the trajectories. Why would these "converge" to something?
With this sentence, we wanted to express that 10 000 are a sufficiently large subset of all the potential realizations. Already after 1000 realizations, the mean thickness calculated from all the individual realizations is not changing any longer to the first decimal. We omit the statement in the text as it is not strictly necessary for the reader to know (L. 254-259)

p12 L310: "Since the thermodynamic growth is expected to be evenly over the polynya region, it leads to rather uniform, level thicknesses of most of the surveyed ice." This sentence is not clear and needs to be rephrased. It also seems to contradict the sentence just above stating that most of the ice is in the thicker bins of the ITD due to significant deformation over the whole polynya.
Thanks for letting us know that this is confusing. We only refer here to the level ice thickness and would like to make the point that the thickness of the level ice is rather uniform. We have moved this sentence to the introduction (L. 27).

p12 L314: "Since the sole interpretation of mean and mode with regard to dynamic and

thermodynamic contributions may miss underlying processes, e.g., the potential contribution of deformation to the observed modal thickness, we will investigate different aspects in the following sections."
Is it necessary to discuss the above paragraph then?
The sentence only limits the reliability of the interpretation of the modal value, but not the reliability of the other parameters describing the ITD (maximum thickness, e-folding, open water fraction, …). We understand that this sentence is misleading at this location, which is why we restructured the whole paragraph. Please see L. 266-274.

p12 L320: "The modal thickness of the level ice is also identical to the mode of the overall ITD, supporting our assumption that it represents best the thickness of thermodynamically grown ice."
The order should be reversed: you don't need to assume anything if you show this result first. This is really what allows you to speak of the overall modal thickness as representative of the thermodynamical growth of level ice. This should be presented first in the manuscript, or at least you can mention that "AEM results show that the modal thickness is representative of the thermodynamical growth of level ice in the polynya."
We agree that "assumption" is not the right choice of words here. As indicated in our answer to L. 71, we build here on previous results and thus rephrase this part, see L 271.

p12 Section 3.1: This section is titled "3.1 Overall, large-scale dynamic thickness change due to area decrease of the closing polynya" and therefore hints at a link with Eq (1) presented earlier, but it starts by describing mostly the thermodynamical growth... The link with the area change and dynamical growth comes only later in a subsection (3.1.1). Maybe you could have two subsections instead to separate the discussion around large-scale thermodynamical (3.1.1) and dynamical (3.1.2) growth and rename this section "3.1 Large-scale thermodynamical growth and dynamical thickness change due to area decrease of the closing polynya"?
Thanks for suggesting how to restructure this part. We followed your advice.

p13 L333: "Divergence on March 30/31 and the occurrence of open water and very thin ice are visible in the divergence time series in Fig. 4 and in the ITD of the closing polynya (Fig. 1d, 2a), respectively."
There is no time series of divergence in Fig 4. If you refer to the time series of "Area extent FYI", then please introduce the relationship between the Area change and the divergence.
Thanks for pointing this out. You are right, we referred to the time series of "area extent FYI". We removed this sentence as we are not covering the MYI any longer.

p14 Figure 4: In the label, please mention that the thermodynamic contribution (red) is obtained from a simulation, and not observations. The title could also be changed to "Dynamic and thermodynamic contributions to mean thickness from model and observations" or " Observed dynamical and simulated thermodynamical contributions to the mean ice thickness", or something like that.
We followed your suggestions and added "modelled" where applicable.

Are the contributions presented in (b) calculated for the trajectories only? At least, from the text in section 3.3 (p.20), it seems like the error bars are derived from the trajectories. Please specify.

The thermodynamic contributions are the output of the MITgcm runs. The dynamic contributions (starting from the 2nd of March) are from the trajectories only. We specified this in the caption. We also marked in Fig. 4b that the dynamic thickness change is only for the period March 1 to March 31.

15 L362: "we have also observed"
This has not been shown yet in the figures, so we don't know what this means. Change to "we also observe" and then refer to the figure where we can see these differences? Or present the observations for the different zones first, and then conclude about their regional variability.
We have removed this sentence as it was only repeating information that is explained further down in the text in more details

p15 L365: "Based on variations of mean ice thickness along the profiles"
Can you give more details about how you separated the regions? i.e. an increase/ decrease in the mean thickness along a moving average, or was it heuristic?
Please specify.
Thanks for asking. We realized that our text lacks some information here. Our decision was based on several aspects of thickness and deformation history. 1: thickness: the running mean of the ice thickness (see Fig. 1), the occurrence of level ice, and the frequency and thickness of the deformed ice (Fig. 6, Tab. 1). 2: deformation history of the ice: path length and origin of the trajectories (Fig. 5a) and the timing, magnitude, and type of deformation that the ice experienced (see Fig. 5, video supplement). For example, ice in the Fast Ice zone is the thinnest and has the largest percentage of level ice. It had short travel paths and experienced weak deformation only during the early deformation phase (Fig. 5b-d). All four zones with their characteristic thickness and deformation properties are described in the text. We rephrase this in the text, L.310-316

p16 Figure 5: This figure should come after the current Figure 7 since it is discussed mostly after Figures 6-7 are discussed.
We followed your advice but this resulted in referencing the original Fig. 5 (p.14, large-scale drift in the insets) before Figures 6-7.

p19 Figure 7: Are all 3 profiles included in these ITDs? Please specify.
Yes, all 3 profile lines are displayed in this figure. We will specify in the caption "ITDs of all four FYI zones of all three AEM lines on March 30/31."

p20 L456: "The mean thicknesses of all 715 trajectories or grid cells, respectively, were combined to compute the ITD of the modeled ice thicknesses." I imagine that you only compiled the simulated thicknesses after the full integration of the trajectories was complete. Correct? Please specify it.
Thanks for pointing out the unclear wording. We rather mean the "integrated thickness of all 715 trajectories on March 30/31 were combined to compute the ITD of the modeled ice thicknesses."

p21 L476: "Underestimation of observed thicknesses is larger in the less deformed Fast Ice and Inner Polynya zones."
I don't see this in FIg. 10. The modeled ice thickness is almost right on top of the observation sin the Fast Ice region in (c).
We agree that in Fig. 10 c this is not visible. We refer here to the mean (see Tab. 1, last column). Since we do not conclude from this observation, we omitted the sentence to avoid confusion.

p22 Figure 10: For clarity, the dashed line for the modeled uncertainty should be the

same colour as the data is belongs to (i.e. blue instead of black).
Thanks for this good hint! We followed your advice.

p25 L569: "Apart from those differences in the shape of the ITD, we have found that modeled mean ice thicknesses were generally smaller than the observed ones."
But the reported simulated mean thicknesses in Table 1 always fall within the uncertainty of the observations.
Thanks for mentioning this, because we realized that more information is needed in the caption. In Tab. 1, the mean is given with its standard deviation which describes the spatial variability. The uncertainty of the thickness observations is assumed to be +/- 10 cm (L. 105), plus an additional 4 cm from the snow cover. We added this information in the caption of the table.

Editing suggestions

p1 L9: "characteristic" –> significant?
Done

p1 L18: MYI was not previously defined.
We removed this sentence. MYI is now first mentioned in L.49
p2 L32: "results in the presence of very variable thickness" weird formulation... maybe write "results in large ice thickness variations"?
Changed, see L. 25

p4 90:"along Lagrangian backward trajectories..." –> using Lagrangian trajectories integrated backwards until its initial formation?
Done, see L. 86-87

p4 L92: "forced by time series of SAR derived, small-scale deformation" –> forced by the time series of SAR-derived small-scale deformation history...
We rephrased this, see L. 89

p7-8 L189-90: "We relate the result to the center of the four grid cells." It is not clear what this means.
We rephrased this, see supplement L. 27.

p9 L239: "considering both deformation estimates.." –> "considering both deformation estimates calculated with the forward and backward drifts"
done

p12 L310: "to be evenly" –> "to be evenly distributed"? or change to "to be the same"?
We rephrased this, see L.27

p13 L343: "we relate the overall area decrease of the polynya to the observed thickness change." using Eq. (1)?
Done, see L.298

p14 L350: "deformation within the polynya was regionally variable and distinctly different in certain zones" This means the same thing twice. Change to "deformation within the polynya showed significant regional variability"?
Done, See L. 296

p15 L354: "the observed mean thickness" –> "the observed mean thickness along the AEM tracks"?

Done, see L. 300-301

p15 L368: "The ice within each zone had similar mean thicknesses and similar ITDs." I think you mean "The ice within each zone had similar mean thicknesses and similar ITDs across all 3 profiles." or something like that, otherwise it sounds like the different zones have the same characteristics, which defeats the purpose of defining them.

Done, see L. 318/319

p15 L381: "To do so, we derived ice drift trajectories of those 715 sections by means of the SAR imagery (Sect. 2.6)." The wording isn't clear. Remove and say in the next sentence: "The general motion of the 715 reconstructed trajectories (see Sect. 2.6) was South-South-East... " ?

Done, see L. 332

p18 L416: "base" –> "basis"

Done

p18 L419: "deformation parameters" add what they are in ()?

We rephrased the sentence, see L. 348.

p20 L435: "of the simple volume-conserving model" –> add "(Sect. 2.7)" or Equation no.

done, see L. 367

p20 L437: "our thickness model" –> "this thickness model"

Done

p20 L437-438: "they reproduce" –> "it reproduces"

Done

p23 L480: "This way, within a month thermodynamics and dynamics restored a first year ice cover that was almost as thick as the surrounding MYI." The wording is not clear. Please rephrase.

We removed this sentence.

p23 L491: "Magnitude of deformation shapes ITD" –> "The magnitude of deformation shapes the ITD"

Done

p23 L502: "0.39 cm" –> 39 cm or 0.39 m

Done

p23 L506: "Taking advantage of the fact that the strongest deformation event left the largest impact on h" Not clear. Please rephrase.

We rephrased this, see L. 431-432

p24 L518: "We test whether the here observed..." –> "We test whether the range of

e-foldings observed here..."
We rephrased this, see L. 445-446

p25 L558: "However, the derived ITDs are composed of mean thicknesses in the 1.4 km, long grid cells of our model, which are too large to resolve individual ridges or ridge clusters." Not clear.. Change to "However, the simulated ITDs are obtained with a spatial resolution of 1.4 km..." or something like that.
We rephrased this, see L. 489-491.

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