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 specific and the technical comments (black) that do not address language edits. We will address the remaining technical comments with the throughout copyediting you asked us for in the final revised manuscript.

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 will remove:

- L. 176 to L. 186: The description of the deformation calculation (as it only repeats existing literature)
  L.300 to L. 339: The description of the multi-year ice (MYI) surrounding the polynya and Figure 3 b

We also add 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 will add a sentence on the type of the polynya in the introduction (l. 2 "an unusual, large, latent heat polynya", l 60: "of an unusual, latent heat polynya that…")
- However, we are aware that our manuscript is already long, which is why we suggest to add 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 replace 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. 62)
- 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. (1) 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. (2) 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.

(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.

(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. More information can be found in the references provided. We add in l. 184-190 that the reader can find more detailed information in the cited literature. Indeed, for the sake of keeping the manuscript as short as possible, we are considering to move the complete description of the deformation derivation into a supplement.

(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 365-366. 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 (see L. 405-406, red trajectories in Fig. 5a). The ice in the Shear Zone experienced strong shear during March 29-31 (L. 407-410). 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 make sure that figures and text agree upon this point in the revised manuscript.

(5) Data:
We have submitted the airborne electromagnetic (AEM) ice thickness, high resolution drift and deformation data to the data repository Pangaea and add the reference as soon as we receive it. We also add details on the specific products we used for the large-scale drift and operation ice bridge data in the revised manuscript.

Technical corrections:
We address all of the technical corrections with the revised manuscript. Please see our suggestions and answers to the ones that do not address language edits below.

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 describe 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. 321-324). For a more detailed answer on how uncertainty in snow redistribution affects our results, please see our answer to your question related to L. 159-160.

L113-115: how does this assumption impact the uncertainty associated with the AEM thickness estimates, relative to that stated on L105?
The overall uncertainty of the airborne electromagnetic (AEM) ice thickness survey increases to the sum of both, i.e. 14 cm. We add a sentence in l. 115: "Hence, the combined uncertainty of the AEM thickness is +- 14 cm. "

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 add a half sentence about the (stretched) backscatter values in dB-scale in the caption of Fig. 1. We provide an additional video supplement here (https://doi.org/10.5446/50650) to let the reader evaluate the manually created outlines.

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.

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.

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.

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.

L220-224: Can you show this assessment?
We have provided an additional figure (Figure RV1) attached to this answer 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. 225.

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, they are the same. We will reword this to make it clear.

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 consider to provide additional figures of the Eastern and Central profile line in the supplements.

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 will reformulate this sentence to make this clearer.

Figure 1:

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 can add a half sentence about this to the caption.

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 will mention in the caption that we both times present total thickness, i.e. snow + ice thickness. Fig. 3a contains information from Fig. 1d, but provides additional information on the level ice thickness distribution.

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.