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. We will address your editing suggestions in the revised document.

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 chose 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 this 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:

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

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

   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 $8\Delta y$.

   We will clarify in the manuscript that deformation was calculated on the regularly spaced grid and not from the Lagrangian trajectories.

(3) For 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). We will add a sentence at the beginning of Sections 2.4 to make this clearer. 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 will follow your suggestions and address them in the final revised manuscript.

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. We remove this information previously found in the results (L309-310).

"The modal thickness of an ITD represents the thickness of the undeformed, level ice (Wadhams, 1994). Level ice forms by thermodynamic growth and melt whose atmospheric and oceanic forcing varies only on large spatial scales (Throndike, Parkison, Rothrock, 1992, Haas et al. 2008). In contrast, deformation results in strongly varying ice thickness and ice which is thicker than can be attained by thermodynamic growth. Consequently, we attribute the large difference between mode and mean to dynamic ice growth by deformation."

p3 Figure 1:

a) Mention how the white trajectories were acquired. Insert: "20m ride" –> "20m ridge"

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.

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 agree and consider restructuring Figure 1 completely 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 make sure that figures and text agree upon this point in the revised manuscript. We will use 20 cm as bin width for the numbers stated in the text.

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.

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 plan to add a sentence in the revised manuscript.

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.

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 will clarify this in the text.

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 defined 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 …"

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 will modify L. 218:

“The tracking error accounts for the deviation of the reconstructed trajectory from the true one due to erroneous pattern matching. Hollands and Dierking (2011), e.g., found tracking errors between 0.8 and 1.6 pixels (their Tables 3 and 4, standard deviations), for pixel size of 50 m this corresponded to 40-80 m.
In the case of trajectories in an inhomogeneous velocity field, there is an accumulated trajectory position error that describes that a deviating trajectory results in the extraction of deformation that was not experienced by the surveyed patch of ice in reality, but by ice nearby. We estimated the accumulated position error from manual tracking of MY ice floes that were located in the polynya (see Fig. 1a). After the first step, the position error was of magnitudes between 51 and 210 m, which at some places is already larger than the expected tracking error. At the end of the tracking (March 1), the magnitudes of the accumulated trajectory position error are significantly larger (1050-2150m). Hence, the tracking error can be neglected.”

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 took all deformation values from the forward and backward deformation field and average them for the deformation history along the Lagrangian paths. We will change this in the manuscript and write “To account for this spatial uncertainty, we extracted divergence, shear and total deformation from all deformation cells with their center points falling into the uncertainty circle (Fig. 2 c). The averaging included the individual deformation magnitudes both for the uncertainty circles along the forward and backward trajectory.”

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..."
Ok.

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")?
First, we extract all values (from the backward and forward deformation field). Second, we averaged the extracted values to get a value for the deformation history. Please see our answer to Question Line 229

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 considered this but would like to keep it because otherwise, we need to re-introduce the redistribution function in the discussion (L. 512). We can omit 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 will unify the equations, units, and text, also related to your remark on Eq (6).

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 will add half a sentence there.

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.

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 rephrase:

"To account for the tracking uncertainty, we created for each of the 715 trajectory random combinations of the potentially experienced deformation within the uncertainty circles described in Sect. 2.6.1. For each time step, we randomly choose one of the observed divergence states and calculated mean thickness change along each trajectory. We repeated this 10.000 times and calculated mean thickness and standard deviation as uncertainty from the resulting 10.000 thickness estimates. For almost all of the 715 trajectories mean thickness changed little already after the first 1000 computations."

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 will rephrase this, e.g.: "Since the thermodynamic growth of level ice is expected to be equal everywhere in the polynya region, it leads to rather uniform, level thicknesses of most of the surveyed ice."

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 move it to the next paragraph and rephrase it.

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 to: "The modal thickness of the level ice is also identical to the mode of the overall ITD, confirming the results of previous studies (e.g., Haas et al. 2008) that the modal thickness represents the thickness of thermodynamically grown ice well."

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 will follow 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" and will rephrase L33.

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 will follow your suggestions.

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 will specify this in the caption. We will also mark in Fig. 4b that the first calculation of dynamic thickness change is for the period March 1 to March 2.

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?
We will change the sentence as you suggested.
Or present the observations for the different zones first, and then conclude about their regional variability.

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 in the text: "Based on the degree of deformation, as well as mean and variation of ice thickness along profiles, we separated four regions with clearly different deformation histories. To be more specific, the criteria for separation were: (1) the running mean of the ice thickness (see Fig. 1), the areal fraction of level ice, and the frequency and thickness of deformed ice (Fig. 6, Tab. 1). (2) the deformation history of the ice, i.e. path length and origin of the trajectories (Fig. 5a), and timing, magnitude, and type of deformation that the ice experienced (see Fig. 5b-d, video supplement)."

p16 Figure 5: This figure should come after the current Figure 7 since it is discussed mostly after Figures 6-7 are discussed.
We follow your advice but this results 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 will omit 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 will follow 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 will add this information to the table.

Editing suggestions
Thank you for your effort in listing your editing suggestions that we will implement in the revised manuscript.

References:

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