The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2020-303-RC2, 2021

The Cryosphere Discussions

Interactive comment on "Linking sea ice deformation to ice thickness redistribution using high-resolution satellite and airborne observations" *by* Luisa von Albedyll et al.

Amelie Bouchat (Referee)

amelie.bouchat@mail.mcgill.ca

Received and published: 26 January 2021

In this paper, the authors present the results of an airborne electromagnetic thickness survey over the anomalous polynya that opened north of Greenland in 2018. They link the observed ice thickness distribution of first-year ice that froze within the polynya after one month of its opening to (a) the area change of the polynya (assuming volume conservation) and (b) the reconstructed small-scale deformation history of the ice along the observed profiles. They show that both methods agree really well with each other, and also agree well with the observed mean thickness and ITD. By inspecting the three AEM thickness profiles and the reconstructed deformation fields from SAR im-

agery, four zones presenting different ITDs and deformation history are identified. The authors show that the main ITD parameters (i.e. e-folding, FWHM, and mean thickness) depend linearly on the deformation magnitude experienced on average within each zone. While it is commonly assumed in the modelling community that the ITD shape should depend on the thickness of the ridging ice, this observational case-study shows that a dependence on the deformation rate should also be introduced. This offers the possibility for further tuning of ITD parameterizations, which is known to affect the ability of sea-ice models to accurately represent the small-scale, sub-grid, dynamics.

The paper is very rich in new data and analysis/conclusions that will be useful to the sea-ice community. However, I find that it needs major editing, restructuring, and perhaps shortening, to improve its readability. I therefore recommend it for publication in The Cryosphere, after the following comments are addressed in the revisions.

Main comments:

---

1) Sections 2.4-2.6 should be clarified with the help of additional details about how the drift and deformation fields are obtained. Specifically, I did not understand how the deformations are obtained from the gridded drift data set described in Section 2.4, which resulted in further difficulty understanding the description of the deformation errors in Section 2.6.1. What I understood is that you have a tracking algorithm that you use to obtain ice velocity fields from SAR images, which you gridded on a regular 700m x 700m grid. Then, using this gridded velocity field, you obtain the deformations, but using a Lagrangian formulation (i.e. the contour integral in Eq. 3), which confuses me because I thought you were dealing with a gridded drift data set, not a Lagrangian one. Then it looks like you assume that you have a gridded deformation field (e.g. Fig. 2c), correct? And do you then use a gridded velocity field to integrate Lagrangian

trajectories backwards starting on the AEM profiles?

2) In the results section, the authors sometimes refer to results that were not already presented in statements like "We have seen this..." and then proceed to show what these results are. Whereas it should be reversed: present the evidence/observations first before you can say that we have seen it or what you conclude from it. In the "other comments" below, I have identified some of these places where I find that restructuring how the results are presented would help improve the manuscript.

3) The manuscript would benefit from text/grammar editing to improve its clarity. I have made some "editing suggestions" below to help with this.

Other 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.

p3 Figure 1:
a) Mention how the white trajectories were acquired. Insert: "20m ride" –> "20m ridge"
b) The general low-resolution ice drift does not match the trajectories seen in (a). Maybe mention something about this?
c) This panel does not seem useful. Could be removed and keep only the March 1st contour in red in (a)?
d) "Combined ice thickness distribution of the FYI shown"... add along the AEM tracks?

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?

p6. Eq (1):

Shouldn't this be $h(t_{i+1})$ =...? 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 $A(t_{i+1})$, $h(t_{i+1})$ with $A(t_n)$, $h(t_n)$ 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:

$A(0)h(0) + \sum_k A(t_k)\Delta h_{th}(t_k) = A(t_{30})h(t_{30})$; with $k = [0 : 29]$

where $A(t_{30})$, $h(t_{30})$ is the final area and thickness of ice after 30 days, $\Delta h_{th}(t_k)$ = thermodynamical growth between t_k and t_{k+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:

$\sum_k A(t_k)\Delta h_{th}(t_k) = A(t_{30})h(t_{30})$; with $k = [0 : 29]$

or, if we pose $n = 30$ as mentioned in the text, then:

$h(t_n) = \sum_k (A(t_k)\Delta h_{th}(t_k))/A(t_n)$; with $k = [0 : n - 1]$

We can also stop the integration before the end of the 30 day period. In this case, we have: $h(t_{i+1}) = \sum_k (A(t_k)\Delta h_{th}(t_k))/A(t_{i+1})$; $withk = [0 : t_i]$

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.

p7 L187: "For deformations in which velocities and their gradients are small in compar-

ison 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.

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. 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. 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.

p8 L205: You should mention what is the typical time interval used to obtain the Lagrangian trajectories/deformations here.

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?

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)?

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?

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..." 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")?

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 $div(u\dot{h})$ since u is a vector and h is a scalar?

p10 L266: "thermodynamic growth or melt..."
This should be a thermodynamic growth/melt RATE (i.e. $\Delta h/\Delta t$) to have units matching that of dh/dt in Eq (5).

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?

p11 Eq(6): Again, the units do not match. The divergence term should be multiplied by Δt (assuming $\bar{h}$ and Δh_{th} are given in meters).

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?

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.

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?

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."

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"?

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.

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. 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.

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.

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.

p16 Figure 5: This figure should come after the current Figure 7 since it is discussed

mostly after Figures 6-7 are discussed.

p19 Figure 7: Are all 3 profiles included in these ITDs? Please specify.

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.

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).

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).

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.

Editing suggestions:
————————-

p1 L9: "characteristic" –> significant?

p1 L18: MYI was not previously defined.

p2 L32: "results in the presence of very variable thickness" weird formulation... maybe write "results in large ice thickness variations"?

p4 90:"along Lagrangian backward trajectories..." –> using Lagrangian trajectories in-

tegrated backwards until its initial formation?

p4 L92: "forced by time series of SAR derived, small-scale deformation" –> forced by the time series of SAR-derived small-scale deformation history...

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

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

p12 L310: "to be evenly" –> "to be evenly distributed"? or change to "to be the same"?

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

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"?

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

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.

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... " ?

p18 L416: "base" –> "basis"

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

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

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

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

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.

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

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

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

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

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.

Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2020-303, 2020.