The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2020-305-AC1, 2021

The Cryosphere Discussions

Interactive comment on "Interannual variability in Transpolar Drift ice thickness and potential impact of Atlantification" *by* H. Jakob Belter et al.

H. Jakob Belter et al.

jakob.belter@awi.de

Received and published: 26 February 2021

Response to Reviewer #1 (tc_2020_305): Dear anonymous Reviewer #1, first of all we would like to thank you for reviewing our manuscript. Please apologise the late reply. We wanted to wait for the discussion to close, so that we could answer all reviewer, editor, and community comments at once. Your comments are most helpful and constructive and we feel that with your input and the input from Reviewer #2 this manuscript will improve further. Before we go into the point-by-point response to your comments (see supplements) we would like to summarise our responses to the most important and prominent issues raised in your review.

1. Comparison to Fram Strait ULS observations: First and foremost, we would like to

agree with you that a thorough and detailed comparison between the presented EM and the Fram Strait ULS data sets would be very beneficial for both data sets. More importantly it could potentially improve our understanding of the processes governing sea ice thickness (SIT) variability between our selected AOI and Fram Strait. Although both regions are comparably close geographically they are still very different, especially when it comes to melt process (oceanic and atmospheric) that affect SIT further south from the selected AOI in summer. We feel that this comparison is very important and desirable, however, it is also very complex and beyond the scope of the presented study. The presented study desires to achieve three main goals:

(i) extension and analysis of the EM SIT time series at the end of the Transpolar Drift

(ii) connection of the observed SIT variability to processes affecting sea ice growth along the pathways towards the selected AOI and in the regions of sea ice formation

(iii) investigation of SIT of the MOSAiC floe and its immediate vicinity and the comparison to a long-term time series of SIT measurements to determine whether the SIT during the MOSAiC year deviated noticeably from the long-term trend

All three of these objectives focus on, or are limited to (in case of MOSAiC) the AOI itself and/or the upstream Transpolar Drift and source regions. This was one of the reasons for us to decide to rather compare the presented EM data set to the shipborne observations from the Russian cruises than to ULS data further downstream. We considered whether the admittedly better temporal coverage provided by the Fram Strait ULS time series would have given us a more solid data basis for the intended investigation, but found that the strong impact of summer melt on SIT variability this far south (79°N) would have potentially masked the mechanisms acting further upstream and limited our chances of investigating them. The following selection of the AOI was then based on the EM data coverage, but also on the uncertainties of satellite sea ice motion products in Fram Strait. The applied track- ing approach is highly uncertain in the Fram Strait south of the selected AOI (compare red lines in Fig. S1 supplement

to Krumpen et al., 2019). Therefore, the cooperation with the Norwegian colleagues was considered at the beginning of this study, but abandoned in order to ensure less uncertainty for the planned Lagrangian ice tracking, which is not only vital for the determination of the regions of ice formation but also for the applied model simulation. Given the presented reasons we do not feel that this additional comparison would improve the presented study of mechanisms acting on sea ice growth in and upstream of our selected AOI. However, as mentioned above we agree that these two time series provide a great opportunity for comparison and we are in contact with our Norwegian colleagues again to tackle it in a separate study. Such a study, specifically dedicated to this comparison, is likely to exploit the full potential of both data sets (ULS and IceBird data are also available in spring, which would allow for an additional investigation of these time series in both of theses seasons).

2. Sea ice growth model and parametrizations of some of the relevant mechanisms: The challenge of simulating sea ice growth along the tracks from the presented years is that although measurements of the relevant parameters (snow, ocean heat, melt) exist for individual years and regions (examples from N-ICE, but also McPhee et al., 2003 and Perovich et al., 2014) but not for all of them. So the presented approach tries to find reasonable values and parametrizations to simulate ice growth, compare the results to the measurements we took in the AOI, and go into the investigation of the parameters that might have caused the differences between observed and modelled values (like the 2016 example). However, this does not mean that we are not aware of the fact that these processes are much more variable in time and space. This is exactly what we are trying to find out with our 'multi-tool approach'. Given the agreement between modelled and observed SIT values in the AOI we specifically focus on the one year that shows the largest difference (2016), but we also realise that the agreement seen for the other years can be a result of mechanisms that balance each other out, which is why we will make it even more clear that more sophisticated models are required to ultimately predict sea ice growth and thinning in the Arctic. The presented model approach was specifically selected to investigate the potential effect of Atlantification

on SIT in the AOI. We show that the results suggest a potential influence, which is not able to explain the observed difference fully and we will make use of your suggested references to clarify and better mention other potential factors that could have caused the anomalously thin ice in the AOI in 2016.

In the following we will answer your major specific comments (in the order of occurrence):

1. We will use your suggested references more prominently in the text to also point the reader to other studies relevant for the general area of interest. As for the comment about validating the model with data from Perovich et al., 2014, please see our general comment above (2.). The evaluation of the representation of melt processes in the model used here is a separate study and beyond the scope of this manuscript.

2. Please see our first general comment above.

3. We also refer to our general response comment (2.) above. We are well aware that snow is variable in time and space and certainly along trajectories that span from the eastern Arctic to Fram Strait, but again we require a long-term and Arctic-wide data set to provide a meaning full comparison to our EM time series and investigate mechanisms like snow cover and their potential impact on our observed thicknesses. As for the 50% reduction of snow depth over FYI and now SYI as well. This is done along the trajectories and based on how long the ice is traveling, which means that we have differences in snow depth along the trajectories and an increase especially towards the end of the Transpolar Drift.

4. This comment is very similar to the previous one. We are not developing a model to predict ice growth, which would require the best possible representation of the relevant parameters involved. We apply a basic one to study the impact of all the parameters that you have been mentioning here and the regions where they potentially act. We are aware that our constant value and even the adjusted ones are not at all representative of the true state. We clearly state that and we use this approach that has been used

before, to identify regions that might have significantly different ocean heat fluxes. This is exactly the point of the 2016 case study.

5. As we state in the overall comments above the goal of applying this basic model is to investigate the factors influencing sea ice thickness, especially Atlantification. The presented model is driven by the variation in FDDs, that is correct. However, this does not mean that we neglect the other relevant factors (the model does, that is true). We are not presenting a model that takes all the factors and their temporal and spatial variations into account to their full extent. We use a basic approach that allows us to see that factors like snow and ocean heat fluxes have such an influence that the simulated values would deviate significantly from our observations. Given a large deviation we go into the investigation of what could have caused it. But we see that in general the basic model provides little deviation from our observations. This is even more true when adjusting the snow depth over SYI (see detailed responses to your comments in the Supplements here).

Please also find a point by point response to your more detailed comments in the attached PDF and please also consider the comments given by the second reviewer and our respective answers. We would also like to draw your attention to the addition of Gerit Birnbaum to the author list. We added her contribution in the respective paragraph at the end of the manuscript. Finally, we would like to thank you again for your support and input. On behalf of all authors,

kind regards

Jakob Belter

Please also note the supplement to this comment:
https://tc.copernicus.org/preprints/tc-2020-305/tc-2020-305-AC1-supplement.pdf

Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2020-305, 2020.