

tc-2020-305:

Interannual variability in Transpolar Drift ice thickness and potential impact of Atlantification

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Dear anonymous Reviewer #2,

thank you for reviewing our manuscript. We are very thankful for your input and hope to improve the manuscript following your comments and suggestions. Please find our point-by-point response to your comments below and be aware that more changes will be made in response to comments from Reviewer #1.

Minor comments:

- (1) *There's a mistake in L2 of the Abstract, which I'm sure is use of language: 'most of the sea ice exits the Arctic Ocean through Fram Strait'. No, most of the sea ice remains within the Arctic: an indicative Arctic sea ice volume of 15,000 km³ and a Fram Strait export flux of 3,000 km³/yr gives a residence time of 5 years (round-number estimate). The correct statement is that most of the sea ice exiting the Arctic Ocean does so through Fram Strait.*

-Response:

You are quite right. We will change the sentence accordingly.

- (2) *Section 2.3, minor comment. I read the first couple of paragraphs on model setup and thought 'what about uncertainties in snow depth and ocean heat flux?', questions that are answered in following paragraphs. This section might read a little better if the statements of those values are joined directly to the text on the authors' approach to uncertainties.*

Response:

Thank you for this comment. We will add a sentence about the estimated uncertainty resulting from the Warren snow depth climatology to the respective data and methods section. However, we are really not able to provide an uncertainty estimate of ocean heat fluxes along these varying trajectories. As we state in the Discussion, ocean heat fluxes can vary significantly in time

and space and long-term measurements are not available. We will therefore leave the paragraph on ocean heat flux as it is here and rely on the discussion of it in the discussion section below. We hope you agree with this assessment and our implementation of your comment.

- (3) *Section 3.1.1 and description of Figure 1 (b,c): there's no indication of uncertainties here; how can we be confident that described differences are meaningful?*

Response:

The assessment of the uncertainty of these values is challenging. We state measurement uncertainties of the EM methods in the respective section, however, your comment is more concerned with the variability of EM SIT values over the AOI in the individual years. This issue has also been raised by Reviewer #1 and we will provide additional information for an estimate of the variability. First, we will add a plot showing SIT distributions for each year individually in the Supplements. This way the range of values is more accessible. In addition we will add vertical lines to the mean and mode values in Fig. 1 b). these lines will indicate the standard deviation of mean and mode of the individual profiles from each year. As for Fig. 1 c), these fractions are based on the Lagrangian tracking done for each year. The standard deviation of these age values for each year are provided in Fig. 3 a).

Changes:

Addition of EM SIT distributions in the Supplements and vertical lines indicating standard deviations (mean and mode) for profiles from each year in Fig. 1 b).

- (4) *Section 3.1.1 on p. 8 and place names: it's usual to put place names on a map near the start of a paper; Severnaya Zemlya, Taymyr Peninsula, Laptev Sea (even Fram Strait itself). There are others elsewhere in the paper, e.g. Beaufort Sea.*

Response:

This is a very important comment. Thank you! We will add names to the most important Arctic seas and Fram Strait in Fig. 1 a). For simplicity, we will remove the mentioning of Severnaya Zemlya and Taymyr Peninsula and use 'western Laptev Sea' instead. Together with the labels in Fig. 1 a) this should be sufficient to properly indicate the region in question to the reader. We hope you agree.

- (5) *Section 3.2, setup of interpretation of 2016 conditions. I am slightly uncomfortable with how this is presented. Apart from 2016, there is an approximate bias of 20-30 cm between model and measurements (measurements*

higher), as the authors state. Why is this? If it consistent and if there is a reasonable explanation, then the 2016 case, including the offset, and assuming that the cause of the offset also applies in 2016, shows a 70-80 cm difference between 'expected' model result and measurement.

Response:

Thank you for bringing this up. We recognized this offset as well and found (on a separate study) that the selected Warren snow climatology likely overestimates snow depth not only over FYI (which we have accounted for already), but based on snow buoy observations also over SYI. This has also been a discussion point with Reviewer #1, who actually suggested even thicker snow depth. However, we adjusted our model and reduced snow depth by 50% over SYI as well. As you will see from the new Fig. 3 b) this measure partly accounts for the offset. Of course, this does not mean that the offset was caused by snow depth over SYI entirely but our comparison with snow buoy data suggests that our snow representations has improved. We now also end up with a larger offset in 2016 (as you rightly assumed) and we will add a more detailed discussion and mentioning of the other mechanisms that likely contribute to the anomalously thin ice in the AOI in 2016 (also something that is done in response to Reviewer #1).

Changes:

This edit changes multiple paragraphs and Figures 3 b) and 4 c).

- (6) P. 11 / L239, awkward phrasing ('still investigated'); I suggest this. 'Ocean heat flux is the main source of bottom melting; it is a parameter that is widely debated and is still being investigated.'

Response:

We will change the sentence following your suggestion.

- (7) L313, if you're going to mention the pandemic, you should probably say 'Coronavirus' in full and not just 'corona'. Maybe the Editor can advise as to whether a reference or citation is needed here.

Response:

Following the official WHO naming we will change 'corona pandemic' to 'COVID-19 pandemic'.

- (8) A small general worry about the presentation of basal melting as the cause of the 2016 anomaly (section 3.2): it might be worth presenting some simple evidence to eliminate increased heat input from above as a possible cause of the reduced sea ice thickness, e.g. by showing (or providing references that show) that insolation / cloudiness / surface air temperatures were not unusual.

Response:

This is a very important point! We already tried to provide that by showing the temperature anomaly for the period from May to August 2016 (Fig. 4 d)), however, we will provide another figure in the supplements giving a comparison of 2016 temperatures with the climatological mean for the 2016 tracks. We hope this will provide the additional information you are asking for here.

Changes:

Additional figure to the supplements.

On behalf of all authors I would like to thank you again for your efforts and input. 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. Given the opportunity by the editor, we will implement your comments and suggestions into the manuscript. We hope that we have met your concerns to their full extent and that you will approve of the changes.

Kind regards,

H. Jakob Belter