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 excuse 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 summarize 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 sea ice thickness 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 tracking 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 summer and spring).

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

Point-by-point response (in order of occurrence):

is this now for both FYI and older ice??
The EM method allows us to measure ice thickness over large areas independent of age. The results presented here consist of ice thickness from the ice reaching the AOI.

summer sea
*We added that to the title***.**

exactly, potential, as well other factors have potential to contribute to this thinning, thus it is not balanced to emphasize this in the title when other factors are omitted in the study.
It seems like factors like, ice drift, delayed freeze-up(?) partly due to solar heating in summer, snow cover, etc. could also I contribute to thinning of the ice, but are not examined thoroughly.
You are right, that Atlantification is by no means the only factor influencing ice thickness in the Transpolar Drift and we do not suggest that it is. However, we investigate the potential impact of Atlantification as one of the factors with increasing importance for ice thickness variations and change in the Arctic in general and our area of interest in particular. The addition of 'Atlantification' to the title is a result of the focus of this study on this particular factor and therefore valid here. Especially by adding the word 'potential' we also clarify that it is not the only factor and possibly not a factor at all. Focusing on one factor does not mean that we are not recognizing other factors, as is seen in the study later (in fact even further down in the Abstract, where we specifically name increased ice drift and ice age). The ice tracking tool specifically accounts for length of the melt and freeze periods and the snow cover as well.
However, we made sure to acknowledge the other factors more prominently in the discussion (Lines 303-310), but leave the title as it is (with your suggested addition of 'summer sea').

how does this work out when many published work refer to ice thickness been decreased by nearly a half (i.e. 50%)?

This is a result from the data set we investigated (see Fig. 1b)). We are not correcting other publications. It is what we got from the data we have, the area we are looking at, the statistical investigation that we applied, and the length of our time series.

are there measurements in the region of MOSAiC starting point from many years? Or do you mean the summer SIT of MOSAiC floe? Please be more specific.
We are referring to the comparison of our summer time series at the end of the Transpolar Drift with data from the MOSAiC floe when it reached that same area (end of the Transpolar Drift) and the same time (summer: July/August). We clarified that in the text (Lines 8-9).

time-series of what exactly? what does "this" refer to?
The presented EM time series for the AOI. The changes we implement in the previous sentences will clarify what 'this unique time series' is.

how is increased solar heating of the ocean taken into account?
Uptake of solar heat by the ocean definitely influences ocean heat and ice growth as well, however, our estimates of ocean heat flux in the Laptev Sea (4 and 8 W/m2) are values calculated from moorings in the Laptev Sea and specifically are values of upward-directed ocean heat flux (originating from the deep Atlantic water). This does not mean that ocean heat originating from above did not also affect ocean heat between autumn 2014 and summer 2016, but for this investigation we focused on upward-directed ocean heat since we have estimates from the moorings available to quantify their potential impact on ice growth. We tried to more clearly state that we are focusing on upward-directed ocean heat flux in this section(Discussion 3.2, Lines: 276-277, 291, 302-304). Our investigation shows that the values of upward-directed ocean heat flux are not sufficient to explain the overestimation of our measurements fully and solar heating of the ocean is potentially one of the other parameters responsible for the large difference between modeled and observed thermodynamic ice thickness. We also made that clear with the help of your suggested references. (Lines: 250-251, 276-277, 436-437)

What about the difference of FYI and SYI/MYI, FYI growth will be more rapid and thus partly compensate for the need to cool SYI/MYI before it starts to grow?? Thus more open water could actually increase ice growth?
You are certainly right about the fact that the increased growth rate of FYI partly compensates for the need to cool SYI/MYI before it starts to grow and Fig. 4 c) indicates that as well. You are also right that more open water increases sea ice growth, however, with a faster drift this does not necessarily mean that the ice is also growing thicker, right? With the influence of more ocean heat (8W/m2 from Jan to May 2015) in the first winter, the following winter season starts with thinner ice and ice growth seems to be faster indeed (compared to the 2W/m2 run), however, Fig. 4 c) also indicates, that the difference in thickness between the different model runs is not overcome by stronger growth of the initially thinner ice.
With the changes in snow depth that we now apply for SYI (see your comment below) this becomes even clearer. With the now thinner snow cover the thinner ice grows faster in the second winter (2016 example, Fig. 4c)). This leads to a smaller difference between the three modelled SIT values in the AOI, however, our model still suggests that the difference is not overcome by this single freezing season.
So we think the question is: What is the thickness difference and can this difference be overcome by faster ice growth of thinner ice or other factors balancing the initial anomaly. For the presented example of 2016 our model study suggests that faster growth of thinner ice was not able to overcome the preconditioning thinning/deceleration of ice growth.
We will change the sentence to: '...winter ice growth will have less time to compensate the impact processes, such as Atlantification have on sea ice thickness...' to clarify that we are interested in the actual thickness of the ice at the end of the Transpolar Drift. (Lines15-17)

I think here you should also refer to e.g. Spreen et al. (2020) that shows that this decrease is not due to export with the Transpolar Drift thru Fram Strait. Thus it must be caused by?
Here you should also elaborate on how well the RS based changes are observed with direct observations (e.g. from ULS data) - are they comparable?
Thank you for the suggestion of the additional study, we added it here. (Line: 25)
As for your second suggestion: we think that this is a very important subject, however, not the focus of this study and especially not at the beginning of the introduction.

Note that Assmy et al present an indirect effect of the thinner ice cover (more dynamics, more openings equals more/enough light for PP). I would also refer to other studies that refer more to the direct SIT effect as well (e.g. Horvat et al.).
Also cite:
Horvat, C., Jones, D. R., Iams, S., Schroeder, D., Flocco, D., & Feltham, D. (2017). The frequency and extent of sub-ice phytoplankton blooms in the Arctic Ocean. Science Advances, 3(3), e1601191.
https://doi.org/10.1126/sciadv.1601191
This is a great suggestion, thank you. We added it here! (Line: 29)

Is it all melting, or simply less sea ice growth? I think it is quite misleading them emphasis is placed on melt alone, while the preconditioning in winter is as important, which is what you try to highlight in this paper, right?
We removed 'Intensified melt and' and start the sentence with 'Thinning of Arctic sea ice also impacts...'. This way we do not put the focus on any of the causes but on the fact that ice is thinning. (Line: 29)

A very vague statement and can be removed, sounds like a self-justification but has no value for the paper itself. Delete.
We agree, it is a self-justfication. But we do not consider it a bad thing here, because we are building up the introduction to show why the work we are doing is important. It gives the transition to the following paragraph, which is concerned with different efforts taken to measure sea ice thickness and we will therefore keep the sentence here.

Also cite:
https://doi.org/10.1029/2019JC016039
We will do that, thank you! (Lines: 41-42)

I would say that this is misleading, especially relative to long-term continuous time-series from e.g. moored ULS measurements in Fram Strait and Beaufort Sea. Although from fixed location they provide i) temporal resolution and ii) consistent observation over long periods. Thus I would say they are one of the few data sets that can actually be used for "investigation of long-term variability" - Please correct.
We changed the ending of this sentence to: '...are spatially OR temporally limited and therefore not sufficient for the investigation of long-term AND large-scale variability.'
We hope you agree that this will clarify the sentence. (Lines: 43-44)

while in fact you have data from 8 of these years. Please add the years with data into the sentence.
We adjusted this sentence to clarify (already at this stage) that data is not available for every one of these years. (Lines: 45-46, 51-52)

(with data from summers 2001, 2004, ... add years, and 2020)
Please see our response to the previous comment.

Based on Fig. 1b, the data from 2001 is not from airborne survey?? Please clarify.
IceBird is only the most recent campaign and the full data set consists of measurements conducted during IceBird, the previous TIFAX campaign, and shipborne expeditions.
We clarified that accordingly. (Lines: 46, 48-50)

and only from a number of years
Added (Lines: 51-52)

Consider changing the color scheme between the map (panel a) and the panel b) - now confusing to think red line in b) is associated with red line in panel a).
Please add the ice thickness from the Fram Strait ULS and Russian expeditions into this graph, this would be a very valuable comparison to see how well these independent observations capture the SIT changes.
Also indicate the location of Fram Strait ULS in the map.
Show location of MOSAiC ground and airborne survey shown in Supplements in the map.
We changed the color of the Russian data transect line from red to black here to avoid confusion (see also below).
As explained in the introduction of this response, we won't add the Fram Strait ULS data, nor positions here.
We feel that adding more symbols and colors to this plot would likely overwhelm the reader, however, we agree that it is more visual to see the locations of the presented MOSAiC reference measurements in a map. We therefore added an additional map to the Supplements indicating the relevant positions.
(Changes to Fig. 1a) and addition of a new Fig. S3 to the Supplements)

scale?
Do you mean the grid size? That would be 25 x 25 km and we will add that to the manuscript. (caption Fig. 1)

I suggest you use a color different from "red" for this line. Easy to confuse with red line in panel b).
Please add the data in Fig. 5 into Fig 1b for easier direct comparison.
Error bars?
Also interesting to see whether there is change in ice thickness from Russian observations in the east to those made in AOI. How much melt occurs between these locations?
We changed the line color indicating the Russian ship observations to black to avoid confusion.
Adding the Russian data here would most likely overload the figure and would make it very confusing, however, it is a very good idea to combine the data sets to make them easier to compare and we will add the EM time series to Fig. 5 to achieve that. We hope you agree that this is the best choice. (Changes to Fig. 5)
Reviewer #2 asked about the variation of the AOI EM SIT as well and we added vertical lines indicating the standard deviation of mean and modal SIT values of the individual profiles from each year. With the addition of the EM SIT distribution for each year the range of values that lead to the presented AOI mean an modal values will be easier to assess. Measurement uncertainty is given in 'Data and methods'. (Changes to Fig. 1b))
Concerning your last comment:
As you can see in Fig. 5 (even better with the addition of the EM data you suggested), Russian observations indicate thinner ice compared to the modal EM SIT measured downstream. As we already state in section 3.4 this is likely a result of the difference in the two methods in combination with the fact that we are looking at two very different regions. A fact that has also been mentioned by Renner et al., 2014.
Given these uncertainties we are not able to determine, for example the rate of melt between the two locations from the current data sets and require joint observations to understand the deviations and biases, as we have already stated in section 3.4.

consider changing these colors so they are indepedent of the colors in the map.
We now distinguish between dark and light blue to underline the difference between the map and the data plot.

do you refer to the circles in the map or in the panel b)?? Perhaps elaborate so it is unambiguous.
We added an additional phrase to clarify that these circles refer to the dark blue circles in Fig. 1 a). This clarification as well as the change of the color of the Russian transect line will hopefully make clear what exactly is shown and where.

show location of these observations separately in the map (use different color for symbols on map and in the graph.
The aim of the map is to give an overview of where EM measurements have been conducted and a reference so that the reader can follow the regions that are referred to in the text. Showing the exact position of an individual measurement from one specific year is very difficult in this basic overview map. A lot of these measurements were taken at similar positions over the years and therefore overlap in this map.
In order to clearly show the coverage of EM measurements we have Fig. 2, which shows these measurement locations for every year individually and the corresponding starting points for the backtracking. In the framework of Fig. 2 the location of the MOSAiC floe measurements and tracking is clearly visible. We decided to separate this in order to avoid overloading Fig. 1 with too many different colors and symbols. We will therefore stick to this approach but change Fig. 1 and its caption according to your suggestions. We hope you understand and agree with this approach. We will also add an additional map to the Supplements that is dedicated only to the MOSAiC data. (See new Fig. S3 in the Supplements)

strike 'just'
deleted

what do you mean by gradients?? in space, and how is that different from the overall thinning of the SIT in Fram Strait?
Please elaborate.
We are referring to the thickness (SIT) gradients across Fram Strait observed by Renner et al., 2014.
We changed the sentence to:
'While previous studies recorded substantial thinning in Fram Strait in summer (Hansen 2013 and Spreen 2020 (adding your suggested study here as well)) and across Fram Strait (79N) SIT gradients in spring during the first decade of the 21st century (Renner 2014)...' (Lines: 57-58)
This way we hope to clarify who observed thinning and spatial thickness gradients and also improve the acknowledgement of similar studies (see one of your comments below). About the second part of your comment: We do not state that there is a difference between thickness gradients and thinning, we merely show what previous studies have found in the Fram Strait and indicate that we are looking at a region further upstream and with partly different methods. This is to put the current study into perspective and provide the reader with the links to previous studies of great interest for this general region.

This time-series is now extended by Spreen et al. (2020), and a closer comparison to your results from a limited time of year but larger area, compered to the ULS SIT observation would be valuable.
Also cite:
Spreen, G., Steur, L., Divine, D., Gerland, S., Hansen, E., & Kwok, R. (2020). Arctic Sea Ice Volume Export through Fram Strait from 1992 to 2014. Journal of Geophysical Research: Oceans.
https://doi.org/10.1029/2019JC016039
See overall response comment above.
We added this reference (see our response to your previous comment).

well one could argue that the sea ice passing by an ULS also cover a very long distance of the ice cover, not?
There are benefits to each approach, so downplaying the other at expense of your approach not necessary very balanced.
You are quite right. The right setup of multiple ULS also allows for the recording of large 'areas' of sea ice (as has been done in Spreen 2020, Renner 2014,Hansen 2013), given a certain movement of the ice. This sentence is supposed to show one of the advantages of the AEM method, so that the reader understands why this method is useful for the presented study. Rather than downplaying anything, we are showing the strength of the method applied here. We are not competing with anything or anyone, we are merely explaining our approach. And as you say, different methods have different benefits and stating that the areal coverage of the AEM is a benefit compared to other methods is factually correct, without taking anything away from other methods.

An even more valuable comparison is the SIT ULS observations in Fram Strait (Hansen et al, and Spreen et al.), how well does the ULS measurements in Fram Strait represent what is observed with the EM data in the AOI - since one could argue that since you have only a number of years with data from AOI cou cannot really capture interannual variation.
You are correct, the comparison to ULS data further downstream is in fact interesting, however, the selection of the Russian data provides information about what happened to the ice we measured before it reached our measurement positions. The goal of the presented study is to complement our data and explain where changes and anomalies potentially originate from. We think the sentence makes that clear.
The comparison to data from further downstream would help investigate processes that affect ice thickness as the ice exits the Arctic, however, to understand the mechanisms driving ice thickness variability at the end of the Transpolar Drift (our selected AOI) the Fram Strait ULS data are not suitable.
Although your proposed comparison is outside the scope of the presented study, we agree that it is very interesting and we are in contact with the colleagues in Norway to conduct a separate study combining their ULS and our EM data sets to investigate what happens further downstream (see our overall comment in the introduction to this response).

Please add the MOSAiC drift track into the map in Fig 1, and use a highlight color to indicate the measurement along the path used in this study.
As stated in the corresponding comment above, we will add a map dedicated to the measurements relevant for the MOSAiC part of this study to the Supplements.

Length of 2001 data compared to the other years when done with airborne EM?
Profile length is given for data from every year in Tab. 1.

please clarify? If the ice is thinner, wouldn't this cause a larger "error" when assuming a constant snow depth?
As stated in this section our study analyses EM SIT, which means the total thickness (snow plus ice thickness). This 0.1 m snow layer assumption merely shows, that the fraction of snow in these EM SIT values is very small due to the season. However, you are right; the relative fraction for thin EM SIT is larger than the relative fraction for thicker ice. Given the fact that we investigate the total ice thickness anyway we consider the snow layer negligible.

How representative is the 14 km of MOSAiC data, is this independent data or repeated measurement over the same floe of a period of time??
It is obviously not as representative as 500 km data profiles, however, as shown in the Supplement (new Fig. S4 and new Fig. S5) helicopter EM measurements of the wider area around the floe confirm the modal thickness for this wider area (also much longer profile

lengths). Due to this limitation we are only able to extend the modal time series and not the mean.
The limitations of the MOSAiC GEM data set and their reasons are presented in section 3.3. Surveys were conducted over a similar but not the same transit four times (with varying SIT distributions) within five days, which is why this can only be considered a point measurement, which is stated in section 3.3 as well. The justification for the comparison to the time series comes from the larger scale measurements conducted with the helicopter in spring and shortly before our designated sampling period (mid-July until mid-August). All of this is provided in section 3.3.

if there happens to be several modes (could be from FYI and MYI?), the thinner mode is selected? - Is this the representative of FYI or all ice ages? Please elaborate and provide a better explanation.
We investigated SIT distributions for every year individually and didn't find a multi-mode distribution (This will be visisble in the additional plot we will provide in the Supplements, see new Fig. S1). This value is the most frequently occurring EM SIT value.

strike 'even'
changed.

show in Fig 1
Please see our responses to your previous comments on this subject above.

but this is the age of the oldest ice, right? So in fact the age of the ice is on average less than this?? Please clarify.
This age is calculated from the tracking (length of each track determines age), however, individual pieces of ice sampled by the AEM over the area could have been older or younger. But we average their thickness to determine a thickness for the starting points. Our investigation looks at an area of ice (majority of which is of the estimated age) rather than every single floe sampled by the AEM. The ice age and FDs (see later comments) given in Fig. 3 are averages of ages and number of days over all tracks from the specific year.

what is the origin of this data?
We added the reference to the User Guide here so that the reader can look up the origin of this data. We are not using these data sets for the presented study and this reference is therefore sufficient here. (Line: 136)

Seems like unnecessary self-citation, remove.
These are well recognized publications using this approach. They have been peer-reviewed and we use this sentence to justify using this approach here. However, since we can only provide examples and not a full list of studies that have applied this method we will shorten the list of examples here and remove the publications the first author has been involved in. (Lines: 136-138)

Is this done for FYI only? Please be specific througout the manuscript.
We simulate ice growth along a trajectory so we start from no ice and thickness increases along the trajectory. This is done independent of the age of the ice. The calculated daily increase/decrease in thickness is dependent on the ice and snow thickness (and other things, see Eq. 1) and is added to the initial ice thickness of that day leading to growth and melt along the trajectories (See Fig. 4 c) for an example).
In theory, we simulate the growth of a single ice floe from the source region to the position of our EM measurements and compare the simulated value to the mean and mode observed from the position of the EM measurements.

what is the ice melt based on, solely the ocean heat flux, or also surface melt from air temp and solar radiation? Please clarify since by late summer the surface melt can make a significant contribution.

Growth and melt are based on the result of ocean heat flux F plus conductive heat flux (second term in the bracket of Eq. 1). Negative growth is considered melt (at the bottom) and whenever melt occurs the model reduces the thickness by an additional 0.005 m for that day to parametrize surface melt. This provides variable total melt per day. We clarified this in lines 151-152.

Is ice expected to form only at the beginning of each trajectory? I .e. the simulations are conducted for the full length of the trajectory? Please clarify.

That is correct. Please also see our response to your previous comment.

are you using montholy vlaues for snow depth in the model, or is this Warren data interpolated somehow to a daily time-step?? Please clarify.

The snow thickness from the Warren climatology is based on season and geographical location (among other things, please see the provided reference for more details). Given that our simulated ice drifts and changes position every day we calculate a daily variation in snow thickness that is applied in the Thorndike model. However, given the comparably slow drift it occurs that the same snow depth is used for multiple days in a row.
We added the word 'daily' to the sentence to clarify. (Line: 146)

is this CFS version 2?
Note that this product has significant positive 2-m temperature bias in winter, which could account for some of the negative "bias" of the modeled SIT. See e.g. Graham et al 2019. J. Clim.

Thank you for asking this question, we accidentally gave the wrong citation. We corrected it to Kalany et al., 1996. So it is the NCEP/NCAR Reanalysis 1 Product that ICETrack uses here. (Line: 147)

Define "summer".
Please examine how this compares to the large year to year variation of surface melt shown in Perovich et al. 2014.
There is fairly large year to year variation in surface melt. On the order that this variation could also inflict quite some errors to the results of modeled SIT?

We removed the word 'summer'. In the model the parametrized surface melt (0.005 m per day) occurs whenever negative growth occurs in the model.
You are already mention that surface melt underlies strong year to year variation, which is not accounted for by an assumption like the one we are making here. This is also why we mention melt processes being a considerable source for uncertainty for the SIT values simulated by the Thorndike model (Line: 241). We will reference the Perovich et al., 2014 paper in the discussion to point the reader towards their study. (Line: 306-310)

but didn't the model include a snow layer, what is left of snow in the model, should be kept as is?

Thank you for this comment, you are right. We changed that and adjusted the respective sentence here (line 155). This change also affects Fig. 3 b) and Fig. 4 c), however, Warren snow depth values used are very similar to the 0.1 m snow layer we used before and changes between the previous simulation and the 'new one' are marginal.

only FYI? the older ice in the area is not taken into account, so this is the FYI mode? Please clarify.

The model calculates thermodynamic sea ice growth along the trajectory (see your previous comment above). Depending on how long the ice travels along its trajectory it is either FYI or SYI or MYI. As you mention above, ice thickness resulting from thermodynamic growth is different for ice of different ages. We consider the modal EM SIT to be the thickness of level ice (grown purely by thermodynamics), therefore thicker modal values usually indicate older *ice (leaving out the dynamic influence on ice growth). We only compare modelled and modal EM SIT values further below.*

exactly, have you provided a sensitivity analysis of the magnitude of the "snow effect"? Before without must justification from observations halving the snow depth in transpolar drift by 50%.
We conducted a sensitivity analysis and added the thickness uncertainty resulting from the maximum snow depth error provided for the Warren snow product (see error bars in Fig. 3 b)). We also added your suggested reference Merkouriadi et al., 2020 to point readers towards this important study on the impact of snow on sea ice growth. (Line: 159)

what about SYI?
I believe that this can be very different from the Warren climatology in parts of the transpolar drift and thus with too little snow ice growth can be significantly over-estimated.
Thank you for raising this very important point here. We also investigated snow thickness differences between the Warren climatology and data from snow buoys from multiple years for a separate study and intensified it following the investigation of the offset between modeled and observed AOI SIT (as a response to a comment from Reviewer #2). Following this investigation we found that the Warren climatology tends to overestimate snow depth over second year ice as well. Based on this analysis we adjusted the Warren snow depth values not just for FYI but for SYI as well (so 50% Warren snow over FYI and SYI). We agree with you that the Warren snow depth can be significantly different compared to measurements over FYI and SYI, however, the buoy data suggests too thick a snow cover rather than too thin. Following the comments from you and Reviewer #2 we adjusted modeled SIT values in Fig. 3b) and the average modeled SIT in Fig. 4c). We also updated the numbers given in the text and the results paragraphs describing Fig. 3 b). (Lines: 230-234)
The results for the differences between the 2W/m2 and the two adjusted runs (4 and 8W/m2) are slightly different from our previous calculations. While the differences between the model runs increase in the end of May 2015 they are slightly lower when the ice reached the AOI. The increased ice growth of thinner ice in the second winter as compared to thicker ice is now even more visible (again please see Fig. 4c)). this confirms your previous comment, however, it also confirms that depending on how large the induced anomaly is, one freeze cycle might not be enough to overcome the initial thickness anomaly.
All of this clearly underlines the importance of reliable snow depth data set to force sea ice growth models and also confirms that snow depth is one of the major sources for uncertainty of the applied model.
As for the usage of the Warren snow depth climatology rather than direct snow measurements conducted over individual parts of the Transpolar Drift and during limited periods of time, please see our response to your major comments.
Changes in lines: 230-234, 284-288 and Fig. 3 b) and 4 c).

here e.g. observations show that this can be muck lower in winter, at least in western part of the - ice growth can be very sensitive to selection of this value.
It in fact is very sensitive to the selection of this value, we conducted sensitivity analysis with different values as well. This is definitely one of the limitations of the applied model and we clearly state that. But again it comes down to what we have. You are right there are years and seasons with very low values, there is also regions with very low values (as McPhee et al., 2003, one of your suggested studies also shows), but we do not have consistent data to resolve these differences in space and time and therefore use a parameterization to investigate the processes behind the observed variations in thickness. One of the conclusions of this study is that we need a better representation of ocean heat fluxes for the prediction of sea ice thickness. Please also see our general comment on the parametrizations used in the presented model approach.

what is the accuracy of these observations?
Visual observations have uncertainties of +/- 10 cm and STK measurements of approximately 4% of the thickness of the floe sampled. We added this information to section 2.4 with the appropriate citations (unfortunately they are only available in Russian).

summer
changed (Line: 190)

summer
changed (Line: 191)

I would expect here a thorough comparison to the ULS/EM data from the Fram Strait presented in Hansen et al., Renner et al, and Spreen et al. Please consider whether this is a paragraph on its own, or massaged into the text in appropriate places.
We refer to our overall comments above.

again, a comparison to the more continous observations by ULS in Fram Strait would be in place here, was a drop in SIT detected in those observations and when?
see above

again, a comparison to the more consistent time-series in Fram Strait should be included (or in a separate subsection).
see above

To evaluate this it would be really useful that the thickness distribution with indicate mode and mean are shown for every year in the supplements. Please add this information.
Could the variation also be due to different fractions of SYI and FYI? FYI can grow as thick as SYI in years the initial SYI thickness is low (because freezing from zero thickness is much more rapid than for self-insulating SYI). Then the modes of the two ice types are no different. Or is all due to the tail (deformed ice)? I am not convinced it is.
We followed your suggestion and added the sea ice thickness distributions to the Supplements (new Fig. S1).
Given the single mode distributions we would consider the difference to be mainly determined by the tail.

It would be very useful to have this information + ice age (based on trajectories) in a tabulated format.
We think the way of presenting this information is a question of preference here. We decided to show trajectories and source regions in Fig. 2 and ice age (derived from ice tracking) in Fig. 3 a). We also put both figures right next to each other to simplify the comparison. Although tables have their advantages in showing this kind of information too, we think showing both would lengthen the manuscript unnecessarily. We will therefore stick to the figures, as they provide the information you are requesting here.

you mean sum of daily mean temperature when temperature below freezing? Please clarify.
We are referring to the number of days with surface temperatures at the positions of the tracked ice being below freezing (independent of how far below freezing they are). This provides a measure for length of melt and freeze cycles along the trajectories (one of your comments above). We changed the name to freezing days (FDs) to avoid confusion with actual freezing degree days (FDDs). FDs used here are a measure for the potential of freezing to have occurred along the trajectories. We will also change the freezing temperature from 0 to -1.9°C.
Changes in Fig. 3 b) and caption as well as lines: 205, 216, 218, 220.

still unclear to me is this the freezing degree days, or simply number of days below freezing, please clarify. If it is the latter, I do not see a physical justification to use that.
Please see the comment above. We clarified that and the unit of FDs is days.
Given the varying lengths of the trajectories within and between the investigated years usage of FDDs would not allow a proper comparison and we therefore use FDs (see above).

Full track, so there are melt seasons included. How well are these actually taken into account with constant F and surface melt?
Include some graphics of the SIT simulations (at least in the Sup mat), preferably there are some IMBs in these years that could be compered to?
That is correct, melt seasons are included and simulated for each track. Fig. 4c) gives an example for the year 2016, which is a good representation of what a typical simulated cycle looks like.
Please also see our overall comment that discusses the usage of parametrizations over varying melt for some years and areas.

The values (circles) in Fig 3 are they average values of number of runs?
Fig. 3 a) FDs and age: these are average values over all trajectories investigated for each year.
Fig. 3 b) modal values: most frequently occurring value for the year in question.
modelled values: averages over all trajectories that have been simulated for the specific year.

please explain this better in the methods part. is error a stdev from measurements, or some other approach? and only 505 snow depth from Warren climatology was used?
This issue has been raised by Reviewer #2 as well and we now provide the information that the uncertainty estimate is based on the root mean square error of the Warren snow depth climatology values (see Warren et al., 1999 for extra detail) already in the corresponding paragraph in the Data and methods section (Line: 163-165 and caption of Fig. 3 b)).

also in 2020
Following the changes of Fig. 3b) this comment is settled.

You mean the backtracked trajectory or the actual drift trajectory, please specify. If the satellite derived track is shown, how does in compare to the actual trajectory?
Highlight when the data given/used in this paper from MOSAiC was collected, was it all in the red box (AOI), or also outside in data shown in Sup. mat??
The track shown here is a combination of actual position recordings from the floe (Oct 4, 2019 - July 21, 2020) and backtracking from the MOSAiC starting position to the region of ice formation (see Krumpen et al., 2020). This is already stated in Section 2.2. However, we clarified that in this caption (Fig. 2) and also provide this information in the caption of the new map dedicated to all the MOSAiC data used in this study, that we will add to the Supplements (new Fig. S3).

please clarify,. is this FDD over the whole trajectory, i.e. could be over two winters, or?
What are the units for FDD? (days?)
Please see our comments above. We adjusted the figure and caption accordingly.

please explain. this should have been described in the methods part, how this "error" is estimated. One model run with "full" Wareen snow depth, and one with 50%??
Please see our response to the similar comment above. This has be added to the Data and methods section.

needs to be better explain how the SIT is modeled, and what FDD is. Is SIT modeled over the whole trajectory, and how well is the summer melt represented for ice older than 1 year, when constant

ocean heat flux and constant surface ice melt is used. This basically means that it is automatically FDD that controls the result as only that is changing the outcome?
Please see our overall comment above.

What about year 2018, ice seem to originate from the eastern Eurasian basin where Atlantification could be in effect? Why is nothing seen in SIT in 2018?
The ocean stratification was strong in winter 2016 (when the 2018 AOI ice passed the region where Atlantification was observed in the previous years). This strong stratification prevented ocean heat (from depth) from reaching and affecting the ice that year. We already explain that and reasons why we did not observe it for any of the other years at the end of section 3.2. (Lines: 318-331)

I would also add the summer surface melt, which can vary significantly from year to year, is this really properly taken into account? See Perovich et al 2014 and Wang et al 2016 on variation in surface melt from year to year. This also affect to summer ocean heat flux.
In summer ocean heat flux can be much higher than 2 Wm2 from solar heating alone, this would also add to thinner ice, in this case ice concentration along the track could also matter.
We added surface melt as one of the major sources for uncertainty here and also refer to it later in this section when discussing potential reasons for the observed differences between model and modal EM SIT. We also added your suggested references in this section. However, as we explain in our overall answers above this section is specifically dedicated to the potential influence of Atlantification on SIT in the AOI, without suggesting it is the only one. (Lines: 241, 302-310)

Figure 5. What about the other along trajectory periods, was summer 2015 or winter 2015/16 or summer 2016 anomalous in some way? Here you assume the only "diverging" effect would be from the first winter.
Atlantification is particularly pronounced during winter (Jan to May) and on the shelf break (see Fig. 4 a) and Polyakov et al., 2017, 2020). Given the position of the ice in the second winter, we do not assume a similar upward-directed ocean heat flux in the central Arctic. We also do not have the data to make any such claims. We state that areal and temporal extend are not well known and that we rely on observations from the shown moorings (Polyakov 2017, 2020).
I want to clarify, we do not assume only one effect on sea ice growth along these long trajectories but in this particular section we look at a single mechanism: Upward-directed ocean heat flux as a result of Atlantification. Given the data and knowledge we have we adjusted our model to use larger ocean heat flux values during the relevant periods and the relevant regions.
However, also following a comment from Reviewer #2 we will provide an additional figure (new Fig. S2, Supplements) to provide temperature information during the transition of the ice sampled in the AOI in 2016.

direct contradiction to the previous sentence were you say it actually varies a lot.
We respectfully disagree with your assessment here. We acknowledge that ocean heat flux is variable in time and space in the previous sentence, but given the limited long-term data, we have to use a parametrization for this parameter. This parametrization is an average value that accounts for the combination of regions and periods with much higher and regions and periods with much lower ocean heat flux values. This is of course an arguable approach and we acknowledge that in the conclusion section. We also state (and provide references) that this has been done before and provided promising results, which is true.

could also becasue the year to year variation in other factors are not taken into account. Surface melt can be equally large as bottom melt and thus keeping this relatively constant

This is true and definitely one of the limitations of this basic model approach, which is why we will state more prominently how important more sophisticated models are for the prediction of sea ice thickness changes. (Lines: 435-439)

Error bars?
To ease comparison, part of this data should have been shown in Figure 1b. Or vice versa, add info from Fig 1b into this to ease direct comparison.
Along with SIT from Fram Strait ULS.
We added EM SIT data here and provide the uncertainties in the Data and methods section (Lines: 173, 179-180). As for the ULS data, see comment about this comparison in the introduction of this response.

strike '(SIT)'
deleted

Equally important to include a comprehensive comparison to the downstream measurements in Fram Strait provided in Hansen et al, Renner et al. and Spreen et al. Do the observations downstream reflect the same as this study? Aka are the Fram Strait observations (ULS, EM) representative of the Transpolar Drift?
I would expect that the authors add a section to the Results and Discussion to do this exercise.
Please see our comment above. The focus of this study is on the driving mechanisms upstream of the AOI. The Fram Strait ULS data is not suitable to answer our research questions here.

This needs to be rewritten once the revision have been completed. should include a better comparison of
And a discussion on the other factors that possibly contribute to the modeled SIT noted in the above comments.
We assume the first part of this comment is requesting the comparison to the Fram Strait ULS data? If so, please see our previous comments.
As for the second part we added the importance of other factors (302-310, 435-439) for model predictions but this conclusion focuses on our investigation of one of the mechanisms: Atlantification (not insinuating it is the only relevant one).

I believe that Renner et al 2014 also presented this, although from "ground" based EM,, so "first" is very subjective. Remove "first".
Removed

important to mention what assumptions and simplifications are taken, and what factors are not properly taken into account.
This conclusion provides an overview, brief summary and concluding remarks. Following your comments the limitations and simplifications have been addressed in more detail in the previous sections.

Already shown in Hansen et al and Spreen et al, this should be acknowledged.
A respective sentence was added here. (Lines:417-418)

although a heavy snow load could have the same magnitude of effect than change in FDD by insulating the ice. Especially for SYI which might grow much less than FYI along the trajectories.
Please see our overall answers to your comments above. We also added this to the Discussion section (Lines: 302-310).

So the second winter ice might go through has not effect on this? Only the first winter matters? Does the sea ice have that long of memory, I doubt it, at least in case when ice gets younger and does not reach the equilibrium thickness of MYI.

I.e. if ice is thinner when it becomes SYI, it could in fact grow more because self-insulation is less?? But I am uncertain this is in fact correctly taken into account here.
Please see our answers above. The presented model does take stronger growth of thinner ice in the second winter into account, however, the presented results indicate that a preconditioning might last until the ice reaches the AOI. As we state above we believe this 'memory', as you call it, is very dependent on the drift speed and the conditions the ice meets along its trajectories. However, our conclusion from this study is that the preconditioning of Atlantification is potentially measurable downstream at the end of the Transpolar Drift. We state that it is our conclusion and point towards what might be needed to strengthen this conclusion.

but still constant?, so not taking into account the large seasonality in ocean heat fluxes (even in absence of Atlantification) from solar heating in summer.
We added 'upward-directed' here to clarify that we are talking about this component of ocean heat fluxes. But other than that, this is exactly the point we are trying to make here. This assumption does not reflect the true state, which is why we require better representations of ocean heat fluxes. Please also see our answers to your comments above. (Lines: 434-439)

strike 'growth'
deleted

does not this exists in Fram Strait?, but you largely omit acknowledge that, and to make a comparison to more consistent temporal SIT data. This would strengthen the analysis.
This would strengthen our analysis assuming that the two regions are very similar which is a question for the separate study we are currently coordinating. We now add a reference at the end of the Conclusion as an outlook for the separate study we are initiating, which the Norwegian colleagues. (Lines: 451-454)

error estimates for these?
Uncertainty estimates are given in the Data and methods section now (see response to your comments above).

only surface air temperature, nothing else was used?
Thank you for pointing that out. We already referenced the other data sets in the text and now added their acknowledgements here as well. (Lines: 457-465)

As noted in the review, I think many relevant publications from the transpolar drift are not acknowledged nor discussed in the manuscript, it would be good practice to do so, and benefit for completeness of the study as well.
We thank you for your suggestions of additional references and we will add most of them to the manuscript (we refer to the answers to your comments above for specific lines).

what is upper 10%, please explain.
This refers to the thickest ice sampled, so the mean of the upper 10% of the full SIT distribution (for each year). We added this clarification to the table.

Please also add the Russian campaigns and similar info to this table.
Also the FYI/SYI/MYI fraction would be useful information.
We added a second table providing information about the Russian observations. Fractions of FYI/SYI/MYI are given in Fig. 1 c).

Indicate the mode and mean with vertical lines
added.

Please indicate this position in the map in Fig 1.

As mentioned before, we do not think that Fig. 1 should be filled with more markers and lines. We therefore added an additional figure to the Supplements (new Fig. S3) which is dedicated to the MOSAiC track relevant for this study. It will show position markers for: ice formation, start of the drift, Leg 3 and Leg 4 AEM data, AOI, and GEM measurements.

Indicate the mode and mean for this case, for GEM you have a very broad primary mode, how is the mode decided here?
Mode and mean are now indicated by vertical lines and corresponding values. Mode is calculated using the most frequently occurring GEM SIT value (using two decimals).

location of floe during these measurements?
The positions of these measurements are already shown in Fig. 2. They will also be visible in the new figure in the Supplements (see the response to your comment above).

Point-by-point response to comments from reviewer #2 by
corresponding author: H. Jakob Belter
March 1, 2021

tc-2020-305:
Interannual variability in Transpolar Drift ice thickness and potential impact of Atlantification
https://doi.org/10.5194/tc-2020-305:
Belter, H. Jakob, Thomas Krumpen, Luisa von Albedyll, Tatiana A. Alekseeva, Sergei V. Frolov, Stefan Hendricks, Andreas Herber, Igor Polyakov, Ian Raphael, Robert Ricker, Sergei Serovetnikov, Melinda Webster, Christian Haas

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 have been 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 changed the sentence accordingly.
  Changes:
  LINE: 2

- *(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 added a sentence about the error 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 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.
Changes:
Addition on Warren snow climatology see **LINES: 163-165**.

- *(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 now provide additional information for an estimate of this variability. First, we added a plot showing SIT distributions for each year individually in the Supplements (new Fig. S1). This way the range of values is more accessible. In addition we added vertical lines to the mean and mode values in Fig. 1 b). these lines 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 (new Fig. S1) 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 added names to the most important Arctic seas and Fram Strait in Fig. 1 a). For simplicity, we removed 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.
  Changes:

LINE: 209 and Fig. 1 a).

- *(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 added a more detailed discussion and mentioning of the other mechanisms that likely contributed 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 (**LINES: 230-234, 276-277, 284-288, 291-310**) 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 changed the sentence following your suggestion.
  Changes:
  LINES: 248-249

- *(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 changed 'corona pandemic' to 'COVID-19 pandemic'.

Changes:
LINE: 333

- *(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 now provide another figure in the Supplements (new Fig. S2) 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 (new Fig. S2).

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. 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