Dear Stef Lhermitte,

We thank you and both reviewers for your second round of comments, and further time spent reviewing our paper. We appreciate the additional constructive feedback on the paper made in response to our initial attempts to respond to your points. We have provided detailed responses to the comments made by yourself and reviewer 2 in this document. If you require any further information, please do not hesitate to contact me at d.clarkson@lancaster.ac.uk.

All the best,

Daniel Clarkson (On behalf of all authors)

1 Response to Editor and Reviewer's comments

We thank the editor and both reviewers for their comments and effort taken to review our paper. Their comments were carefully considered and have been incorporated into the improved, revised version of the paper. We have addressed only the reviewer's comments below since the editor's comments are a summarised version of the same points. The original comments made by the reviewer are in italics, and our responses are in bold. Specific changes to the paper can be seen in the revised version, and any line references made in the below responses relate to the revised version of the paper.

Thanks for this revised version and your responses. But I'm continuing to think that a comparison with a microwave based melt product is needed, although I agree that your product is well more than a simple binary melt extent data set. It is nice to show and discuss melt and temperature evolution but we don't know how your product performs. Is it reliable and robust? Is it able to detect the extreme melt events (mostly driven by clouds) knowing that only the cloud-free days are considered here? In the accumulation zone, melt occurs when there are clouds and in the ablation zone, only 50% of days are cloud free.

As cloudiness has been changing and is different between the 2 decades 2001-2009 and 2010-2019, can these 2 decades be compared in Section 3.3 if you consider only the cloud-free days? How do your temperature statistics compare with the ones using surface temperate from ERA5 for example ?

We first note that our choice of the Moderate Resolution Imaging Spectroradiometer (MODIS) data was for (a) high resolution spatial coverage and (b) the availability of a full range of ice surface temperature measurements. The latter is of particular importance as it allows us not just to investigate melt itself, but also to study the behaviour of those temperatures in the range just below the melt threshold and to investigate these for trends and patterns. An unfortunate limitation of using MODIS is that our results are constrained to inferences under clear sky conditions only. Notwithstanding this, we can still make useful inference about long term temperature and melt trends under these conditions. To address the reviewer's concern, we have added a further investigation looking for evidence of changes in cloud cover both spatially and over time. We found minimal inter-year cloud cover variability between the two periods for which clear-sky day temperatures are subsequently compared: 2001 to 2009 and 2010 to 2019, periods which were in any case originally chosen to limit the impact of inter-year variability of climate and weather on any trends observed. In particular, differences in cloud cover over time are negligible for those months with the greatest proportion of melt, providing further confidence that weather conditions/patterns have not significantly changed over the range of the data and thereby that surface conditions are sufficiently similar over time to be modelled by a single distribution.

Therefore, you need to show, at least over one season (e.g. summer 2019) what is the interest and robustness of your complex model with respect to a microwave data set (Can your model be applied to the microwave brightness temperatures?). An other validation, if your prefer, would be to compare the melt at the PROMICE AWS's using measured temperatures as the "true" *melt data set.*

A direct comparison between either the Automatic Weather Station (AWS) and MODIS data or between the fit of our model to the two sets of data is severely limited by a fundamental difference in the characteristics between the two datasets: namely that MODIS measurements contain a non-negligible number of positive values, and AWS observations do not. Indeed, it is precisely this characteristic of the data that informed the final form of our proposed model, since the mass of observations close to and just above 0°C meant that a routine statistical model for the upper tail of the data was inappropriate. Since the AWS does not display this trait, we would not expect the proposed model to fit as well. Adaptation, such as removing the fourth 'melt' component from the mixture distribution, and replacing it with a right-censored third component, could be made but are beyond the scope of the paper. We would also like to point out that a primary objective of our model is to describe and predict the full temperature distribution at each sample location, with estimation of melt extent following as a secondary consequence.

Nevertheless, as recommended by the reviewer, we have provided an additional subsection in the paper comparing the melt found by applying our ice surface temperature model to MODIS Ice Surface Temperature (IST) data with that of the Programme for Monitoring of the Greenland Ice Sheet (PROMICE) AWS data. Given the good fit of our model to the MODIS dataset, this acts, to a high degree of approximation, as a comparison of the melt observed at the AWSs and that captured by MODIS as opposed to a comparison of the former with the melt estimates from our fitted model. We further reference previous studies comparing the accuracy of MODIS and AWS data sets to establish a foundation for the discussion, and compare the estimated melt at all PROMICE AWSs to the closest of our sub-sampled MODIS cells. These comparisons must be considered with caution since the distances between the individual AWSs and their nearest neighbour MODIS cell ranges from 4–59 km.

Finally, with respect to Casey et al. (2017), how could your product be impacted by the changes of sensors in the MODIS data set knowing that your model needs no change in the surface conditions?

Ref: Casey, K. A., Polashenski, C. M., Chen, J., and Tedesco, M.: Impact of MODIS sensor calibration updates on Greenland Ice Sheet surface reflectance and albedo trends, The Cryosphere, 11, 1781–1795, https://doi.org/10.5194/tc-11-1781-2017, 2017.

The paper referred to makes note of sensor calibration updates with respect to surface reflectance and albedo, but not for surface temperature. The paper examines data from MODIS bands 1 to 7, whereas ice surface temperature is derived using bands 31 and 32 (https://nsidc.org/sites/nsidc.org/files/technical-references/MOD29_C61_UserGuide.pdf).