Authors' response to anonymous Reviewer #3

This is a well-written and evidenced paper that uses several independent datasets to explore the precise characteristics melting over the George VI Ice shelf. The authors demonstrate that melt extent and duration over GVIIS was higher in 2019/20 than in any other melt year analysed, and suggest that localised meteorological factors were responsible. Namely, persistent north-westerly and north-easterly low-speed winds, which allowed temperatures to rise above freezing and for melt to occur.

The manuscript is presented in a logical structure, with interesting and informative figures. The methods used are justified and largely given in sufficient detail to be reproducible. The results are supported by the data presented and the conclusions drawn are consistent with the evidence given. I recommend the publication of this manuscript in TC subject to minor revisions.

We thank this reviewer for their positive remarks about our study and we are very grateful for the useful comments that they have provided.

Specific comments:

[L49-52] You make reference to the collapse of Larsen A (and Larsen B in a previous sentence) and then in the next sentence to the partial collapse of Larsen A and B – could you revise this to be clearer/more consistent with the above?

We will make these sentences clearer by initially talking about the near-complete collapse events of the Larsen A (and Larsen B), before then talking about the partial collapse events of these (and other) ice shelves.

[L57-61] I think the 20.75 record has been thrown out by the WMO so you can probably remove this part of the sentence. The justification can be made using the Esperanza record alone.

Thank you for highlighting this, which was also noted by Reviewer #2. We will remove all reference to the potential 20.75C record in our revised manuscript.

[L62] You could link this back to the Bevan et al. paper again.

As we do not have confidence in the results of the Bevan et al. paper (i.e. for the reasons discussed in Section 4.1 of our paper), we would rather not add an additional reference to this paper.

[Section 3.4] Can you comment on the data processing for the AWS – e.g. is it an instantaneous value every 12 hours, or an average?

A similar question was asked by Reviewer #2, so we apologize for not being clear. The 12-hour data include instantaneous temperature measurements at noon and midnight, so we calculate the mean of those two data points to calculate the daily mean temperatures. We will clarify this is our revised manuscript.

[L172] extra "to" after parenthesis

We will correct this.

[para starting 187] Is there precedent for this type of methodology?

This methodology follows Moussavi et al. (2020), which we will more clearly state in our revised manuscript.

[para starting L320] It may be worth commenting on the fact that surface temperatures can differ from 2 m air temperatures and so 0 degrees at 2 m may not actually mean surface temperatures are at the melting point / vice versa. How might this influence the length of periods of >0 temperatures and consequently refreezing?

We thank the reviewer for this good suggestion, and will add the following sentence to the Methods (section 3.4): "*Although the air temperature measured at a height of 2-m by the AWS will vary slightly to that at the ice surface, for the purposes of this study, we assume these temperatures to be equivalent (Kuipers Munneke et al., 2012)"*

Additionally, we would like to state that we did experiment with alternative thresholds of -2°C and -1°C (accounting for the possibility of melt occurring below 0°C), and by applying smoothing to temperatures and the overall result was unchanged; 2019/2020 still experienced the longest periods of high air temperatures and thus we elected to use the simplest threshold of 0°C.

[para starting L335] During how much of the time series in Fig. 4a was the temperature above 1 standard deviation above the mean? This might be another interesting way to think about this anomalously warm period.

This is certainly true and we will add the following sentence to this section to clarify this: "*We also note that the temperature during these five periods is often more than one standard deviation greater than the multi-year daily mean*"

[para starting 335] Was any sensitivity testing conducted regarding the foehn detection method? Different thresholds and methods can yield very different results, e.g. isentrope-based method of King et al. (2017, doi: 10.1002/2017JD02680) vs surface method similar to the one used here. Varying the thresholds used can also make a difference (e.g. Turton et al., 2018, doi.org/10.1002/qj.3284).

Sensitivity testing regarding the wind threshold in the foehn condition calculations was conducted. We note that by decreasing the windspeed threshold, the sensitivity of the foehn-detection algorithm increases substantially. Therefore, we choose a liberal threshold of 1.5 ms^{-1}, and with that threshold, we calculated just 9 hours of foehn conditions in the 2019/2020 melt season. With the 3.5 ms^{-1} threshold used by Datta et al (2019), there were even fewer hours of foehn conditions calculated in this melt season. To further refine this wind threshold for the GVIIS, a more in-depth study, using additional AWS data from elsewhere on the ice shelf (e.g. following the approach of Turton et al. 2018), potentially in combination with high-resolution modeling (e.g. following the approach of King et al. 2017), would be required.

Additional references that we will add

Kuipers Munneke, P., van den Broeke, M. R., King, J. C., Gray, T., and Reijmer, C. H.: Near-surface climate and surface energy budget of Larsen C ice shelf, Antarctic Peninsula, The Cryosphere, 6, 353–363, https://doi.org/10.5194/tc-6-353-2012, 2012.