Authors' response to anonymous Reviewer #2

This is an engaging study investigating the 2019/2020 austral melt season on the north George VI Ice Shelf, Antarctic Peninsula. The authors combine different datasets such as satellite microwave radiometer/scatterometer data, optical satellite imagery and local meteorological station data. The authors highlighted exceptional melt and surface ponding conditions in 2019/2020, and they concluded that warm temperatures were likely triggered by warm northwesterly and northeasterly low-speed winds, rather than Foehn winds.

The paper is well written, and the results are supported by the data and methods. The study has the potential to become a fully relevant article to concept of The Cryosphere and my criticisms are with regards presentation rather than science.

We thank this reviewer for their very useful remarks.

Specific comments:

Lines 31-32: Could you please specify the rate of mass loss?

We will re-word this sentence to read: "*The rate of mass loss from the AP has tripled since the 1990s, with an average of 24 Gt yr-1 from 1979 to 2017 and an acceleration of 16 Gt yr^{-1} per decade (Rignot et al., 2019).*" We also note, that as these statistics are currently noted in the Section 2 of our paper ("Study Site"), we will delete them from that section in our revised paper.

Lines 53-54: Is there any projected estimation of contribution of the AP ice shelf melting to the global sea level rise? If yes, that information would be useful.

We will add information about the potential sea level contribution from the George VI versus Larsen C ice shelves. However, we suggest it will be more appropriate to add those details at the end of Section 2 ("Study Site"). The sentences we will add are as follows: "*Due to the strong buttressing forces that the GVIIS provides relative to the large volume of grounded ice in Palmer Land, if this ice shelf were to completely collapse, the resultant acceleration of the inland glaciers would add < 8 mm to global sea levels by 2100 and < 22 mm by 2300 (Schannwell et al., 2018). In contrast, sea level contributions resulting from the collapse of the much larger Larsen C Ice Shelf would be relatively low (< 2.5 mm by 2100, < 4.2 mm by 2300).*"

Lines 56-58: I would add a link of official WMO status on this evaluation: https://public.wmo.int/en/media/news/new-record-antarctic-continent-reported. In addition, to my knowledge, WMO is evaluation the value of 18.4◦C., not the 20.75◦C. There has been no official effort/information to evaluate the value of 20.75◦C. Further- more, this value was not recorded at the official Marambio station. I recommend the authors to make sure that whether this temperature is being evaluated or not. If not, I recommend them to remove the statement in lines 58-60.

Thank you, we will add the weblink. We also agree that this 20.75°C record does not now appear to be in evaluation by the WMO, so we will delete this sentence (in addition to removing the following sentence about the prior record on Signy Island, which is also no longer relevant).

Lines 60-63: Could you please quantify melt amount in this part too? Because, you are comparing GVIIS with the other AP ice shelves. Some numerical values would help the reader to see the differences.

Quantifying melt volumes on ice shelves in addition to the GVIIS is beyond the scope of this study, however we will add an additional sentence to this section that will qualitatively describe and compare meltwater ponding on AP ice shelves that is observed in the MODIS mosaic shown in Figure 1a.

Lines 87-94: What makes the northern GVIIS more vulnerable to high surface summer melt rates? I would expect to consider rates of basal melting, nonetheless, you state that rates of basal melting are greatest at the southern end of the GVIIS. Therefore, a clarification might be needed to have a better idea on different physical processes at the northern and southern ends.

The northern GVIIS experiences warmer air tthan the southern GVIIS (e.g. see Datta et al., 2018). Additionally, precipitation rates in the North are lower than in the south, which is at least partly attributable to the precipitation shadow of Alexander Island. We will reword the first sentence of this paragraph to read the following, which hopefully clarifies the key differences in the surface processes over the GVIIS: "*Compared to the southern GVIIS (~72°00′ S to 77°00), the northern GVIIS (~70°30′ S to ~72°00′ S) experiences higher surface summer melt rates (< 250 mm w.e. yr-1; Trusel et al., 2013; Datta et al., 2018) and lower accumulation rates (< 0.2 Mg m-2 yr-1; Bishop and Walton, 1981; Reynolds, 1981); the latter is attributed to the presence of a precipitation shadow down-wind of Alexander Island (Bishop and Walton, 1981).*"

Regarding basal melting, we are not sure what this reviewer means by "I would expect to consider rates of basal melting", as we do already give detailed information about the spatial variation in basal melt rates, and the reasons for high basal melt rates, in the current version of our paper (current lines 91 – 94). However in our revised paper, we will also state basal melt rates from Adusumilli et al. (2020), as requested by Reviewer 1.

Lines 123, 208-212: Is there any elevation difference between the AWS station and ice shelf?

This is a good question. The AWS elevation is 66 m, which is similar to the ice shelf surface elevation in this region (50 – 60 m, given that the ice shelf thickness is 500 – 600 m). We will state the AWS's elevation along the AWS's coordinates in Section 3.4.

Line 213: What does it mean exactly "using 12-hour data" for the 1999-2020 period? I understand that you use only one time step as a daily mean temperature for the 1979- 1999 period. If yes, what is the exact hour of the observation (morning, late afternoon)?

We apologize for being unclear. By "12-hour data", we are referring to data that were recorded 12 hours apart; at noon and midnight. Therefore each daily mean value from 1979 to 2020 (NB. not just 1999 to 2020 as this reviewer states) is calculated from two data points. We will add all these details to the manuscript.

For the second period, did you pick up the same hour with the 1999-2020 period or did you take a temporal mean of 12-hour data?

We perform high temporal resolution analysis is from 2007 to 2020, which is (as it currently stated in the manuscript) when "*when AWS data are at a high frequency (10 minute intervals) and data gaps are minimal*". Therefore, for this period, we use the 10 minute data (not the 12-hour data) to calculate the length of time when temperatures are continuously at/above 0°C. We will ensure we make this clearer in our paper.

Line 223: Could you please specify why there exist lower mean speeds over north GVIIS?

Although the study by van Wessem et al (2015; their Figs 10 and 11) show that in general, wind speeds over North GVIIS appear to be similar to those in the Scar Inlet/Larsen C region, foehn winds will likely be slower in speed at Fossil Bluff compared to the Cabinet Inlet due to the lower mean elevation of Alexander Island's topography compared to the AP mountains to the west of the Scar Inlet (e.g. see van Wessem et al (2015); their Fig 1). Therefore, we will edit the current sentence in the manuscript to read: *"We use a wind speed threshold of 1.5 ms^{-1} instead of the higher threshold of 3.5 ms^{-1} used by Datta et al. (2019) for the Cabinet Inlet AWS to account for lower foehn wind speeds over north GVIIS, which result from the lower mean elevation of the mountains on Alexander Island compared to those on the AP west of Cabinet Inlet (van Wessem et a., 2015)*."

We also note that our wind speed threshold of 1.5 ms^{-1} is actually more liberal than the 3.5 ms^{-1} threshold used by Datta et al (2019). In other words, although we only calculated 9 hours of foehn conditions in the 2019/2020 melt season, if we had used the higher Datta et al. threshold, we would have calculated even fewer hours of foehn conditions for the northern GVIIS.

Lines 238-239: In your analysis, I understand that Larsen C shows a record high melt year in 1992-1993, not in 2019-2020 on the contrary to the findings of Bevan et al. Is that related just with the different datasets? Can you please specify potential uncertainties of each product?

We will add the following sentence to our revised manuscript, which we hope will more clearly describe the discrepancy: "*This finding is contrary to the results of Bevan et al. (2020), who report that Larsen C experienced a 41-year record high melt year in 2019/2020. Bevan et al's (2020) results are based on microwave radiometer (SMMR/SSMI) data for melt seasons from 1979/1980 until 2016/2017, followed by microwave scatterometer (ASCAT) data from 2017/2018 to 2019/2020. In contrast, we use SSMR/SSMI data over the AP for the full 1979 to 2020 period to preserve consistency. As we explain in Section 3.2, ASCAT C-band radar is likely to be more sensitive to melt at depth than microwave radiometers, thus resulting in Bevan et al's (2020) higher calculated melt over Larsen C in the 2019/2020 season after combining different data sources into one time series."*

Regarding the potential uncertainties of each of the melt products, this has now been addressed in response to one of Reviewer 1's main comments, so please refer to our long reply there.

Lines 326-334: Why not to compare the longest warm periods with the volume changes? For instance, are the longest periods coincide with the largest volume change?

We thank the reviewer for this useful idea, and we will insert the following sentence into the Results section (4.3): "*In 2019/2020, volumes of meltwater ponding are highest in early January and then again in early February; corresponding with periods when air temperatures are ≥ 0^{o}C for extended periods."*

Additionally, we will insert the following sentence into the Discussion section (i.e. '5.3. Local climatic controls of the 2019/2020 melt event'): "*For example, we note that sustained warm temperatures in early January (Fig. 7b, periods A and B) and early February (Fig. 7b, periods C and D) coincide with periods when surface meltwater volumes derived from optical imagery are relatively high (Fig. 5b).*"

Lines 335-339: I think the authors should also discuss the potential role of the warm air advection given the low foehn conditions. I suspect that regional sensible heat transport seems to be one of the main contributors of the 2019/2020 melt season, particularly for the first two weeks of February.

We completely agree that warm air advection likely drove the sustained high air temperatures in 2019/2020, and therefore the high melt in this season. However, we are wondering if the reviewer wrote this comment before reading the following two paragraphs in our paper (current lines 349 – 359), which describe the results of our analysis of wind speed alongside air temperature data (i.e. warm air advection). In particular, our current manuscript states: "*in the 2019/2020 melt season, we show that both northwesterly and northeasterly winds show warmer temperatures at lower wind speeds*" (current lines 358/359). Melting due to the increase of warm, low-speed winds from the NE and NW is also discussed in the Discussion, and mentioned in the Conclusion and Abstract. In our revised manuscript, we will ensure that we clarify that what we are describing is indeed warm air advection. For example, we will add the following sentence to the beginning of the paragraph starting on current line 349: "*Finally, we analyse the potential role of warm air advection, resulting in sensible heat transport, on the high melt in 2019/2020.*"

Lines 349-359: Can you specify where the calculated 9 hour of foehn is located in this time series (Fig. 6)?

The time periods when foehn conditions are present are already described in the previous paragraph that specifically focusses on foehn winds (current lines 335 – 337), and these periods are already labelled in Fig 6b (blue circles), so we are wondering if the reviewer missed this. The relevant sentence currently reads: "*Foehn conditions (as described in Section 3.4) are only present for about 9 hours over the entire 2019/2020 season (Fig 5, blue line), and occur in early and late summer (Fig. 6b, blue circles) when winds are typically stronger*."

Lines 431-438: I am a bit disconnected here. So, I understand that rather than local conditions (i.e., Foehn winds) one should expect to see regional- and/or large-scale warm advection. In this case, I would expect to see similar warming rates over the GVIIS and Larsen C. However, in Fig. 2c there are notable warming differences be- tween these two regions. Do the authors have any idea for these differences?

We would first like to clarify to this reviewer that Figure 2 shows surface melt, not warming (though surface melt is obviously driven by the surface energy balance). Second, we agree that this figure shows significantly higher melt over the George VI and Wilkins ice shelves compared to the NE areas of the AP (inc. Larsen C) in 2019/2020, and we will add the following extra sentence to Results section 4.1 to make this fact clearer: *"For the AP, cumulative melt days in the 2019/2020 austral melt season are highest in the southwestern areas of the AP (including the Wilkins and George VI ice shelves), in addition to the northern area of the Larsen C Ice Shelf (Fig. 2b). In comparison, cumulative melt days in 2019/2020 are relatively low over the southern areas of the Larsen C.*" However, as the main aim of this study is to focus on melt on the northern GVIIS, and the local atmospheric controls of this melt, it is outside of the present study's scope to perform modelling to establish why the SW area of the AP experienced more melt than the NW area of the AP in 2019/2020.

Fig. 1 and Fig. 2: Could you please use a larger font size for the lat/lon coordinates?

We will do as suggested for Figs 1 and 2, as well as for Fig S1.

Overall suggestion: As you use different datasets with different spatial and temporal resolutions, I would add a table for the information given in data and methods (e.g., name of the product, resolution, temporal coverage etc.). This makes it easier for reader to follow the result.

As we actually only use four data products in total, i.e. two microwave products (SMMR/SSMI and ASCAT) and two optical image products (Landsat 8 and Sentinel-2), we do not think that a table of details about these products will be sufficiently beneficial, especially as the details about each product are clearly stated at the beginning of each section in the Methods (i.e. SMMR/SSMI is described in Section 3.1, ASCAT is described in Section 3.2, and Landsat 8/Sentinel-2 are described in Section 3.3). However, we are happy to include such a table if the Editor would like us to.

Additional references that we will add

Bishop, J. F. and Walton J. L. W. Bottom melting under George VI Ice Shelf, Antarctica. Journal of Glaciology. 27(97), 429-447, 1981

Datta, R. T., Tedesco, M., Agosta, C., Fettweis, X., Kuipers Munneke, P., & van den Broeke, M. R. Melting over the northeast Antarctic peninsula (1999–2009): Evaluation of a high-resolution regional climate model. The Cryosphere, 12(9), 2901–2922. https://doi. org/10.5194/tc-12-2901-2018, 2018.

Schannwell, C., Cornford, S., Pollard, D., and Barrand, N. E.: Dynamic response of Antarctic Peninsula Ice Sheet to potential collapse of Larsen C and George VI ice shelves, The Cryosphere, 12, 2307–2326, https://doi.org/10.5194/tc-12-2307-2018, 2018.