Authors' response to anonymous Reviewer #1

This manuscript presents a quantitative analysis of surface meltwater on the George VI Ice Shelf, Antarctic Peninsula, focusing on the most recent melt season (2019/2020) and setting this in the longer-term record of melt. The authors use data from a number of sources, including microwave radiometer and scatterometer, automatic weather station measurements and a previously published algorithm to classify surface meltwater ponding from Landsat 8 and Sentinel-2 imagery.

Meltwater has been linked to the instability and collapse of Antarctic Peninsula ice shelves. Although the focus and methods of this study are not novel, it quantifies the recent anomalous melt event on this ice shelf, which is important given that such record-high melt events are set to become more frequent in future. Therefore, it is my view that the findings from this manuscript are of broad interest to the cryospheric community.

In general, I would like to complement the authors on their well-written and clearly-structured manuscript, and the study rationale and methods are well-justified. The results build upon previous work that has reported surface meltwater lakes on this ice shelf by providing a time series of ponded surface meltwater together and analysing this alongside microwave observations of melt together with local climatic controls.

We thank this reviewer for their complementary remarks about our paper and we are pleased to hear that they think it will be of broad interest to the cryospheric community.

The authors use optical Landsat 8 and Sentinel-2 imagery to derive ponded meltwater volumes. However, I wonder why historical satellite imagery pre-2013 was not used to supplement this record and set 2019/2020 within the longer-term context of surface ponding? I suspect this may be related to difficulties applying the lake depth radiative transfer model to historical imagery, but it would be worth justifying.

Analysis of pre-2013 imagery would require tuning Moussavi et al's threshold-based algorithm for the Landsat 7 sensor (and for prior Landsat sensors). This could be done, however, with much of Landsat 7 data missing (due to SLC off gaps), lake volumes derived from this sensor would not be easily comparable to L8 and S2. In other words, the inclusion of Landsat 7 data will not necessarily extend the time series record, simply because its data is not comparable to Landsat 8 and Sentinel-2. We will briefly state these reasons in our revised paper.

There could be more of a discussion on the fate of surface meltwater during and at the end of the melt season. For example, is the decrease in meltwater volume in mid-late January associated with refreezing, or is there any evidence of englacial lake drainage? Similarly, do the authors observe any rapid drainage events, and if so at what point in the melt season?

This is a helpful suggestion and we will include some additional statistics in the main text (inc. mean depth and maximum depth of water-covered pixels). Additionally, we will add some sentences that describe links between the total volume of surface ponding and the air temperatures, particularly for the 2019/2020 season. However, to intercompare volumes of meltwater <u>between different austral melt seasons</u>, the optical image analysis part of this study focusses on temporal variations in the <u>total</u> area and volume of surface meltwater (and particularly the maximum values) each season. Therefore, we did not produce timeseries of data for individual lakes, and producing those data would involve re-running the algorithm and performing a substantial new amount of analysis that is beyond the scope of the current study. However, from visual analysis of the key images, we have not noticed any evidence of rapid lake drainage events, and due to the compressive flow regime (see current lines 84-86), we also would not expect such events to be common.

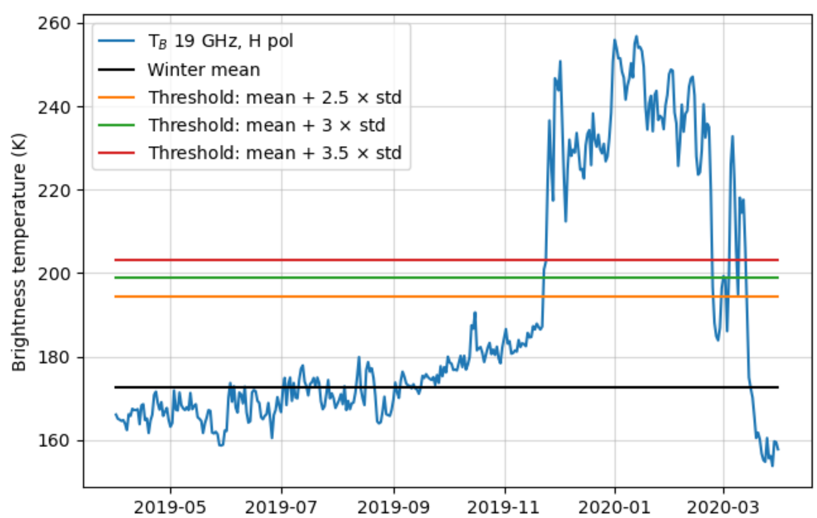
There is also a lack of discussion of uncertainties, especially melt detection uncertainty using the microwave brightness temperature product and the ASCAT product.

Addressing uncertainties associated with microwave data products of binary melt/no melt information is challenging. In-situ measurements (e.g., of snow temperature, liquid water content) for true validation purposes are rare. Assessment against air temperature measurements have been done in the past. In our study, two distinct microwave remote sensing techniques and algorithms were used to bring confidence in our conclusion; we used both microwave radiometer (SMMR/SSMI) data and microwave scatterometer (ASCAT) data. Although the figure showing the ASCAT-derived results is currently in the Supplementary Information (current Fig. S3), we propose to move this figure into the main paper.

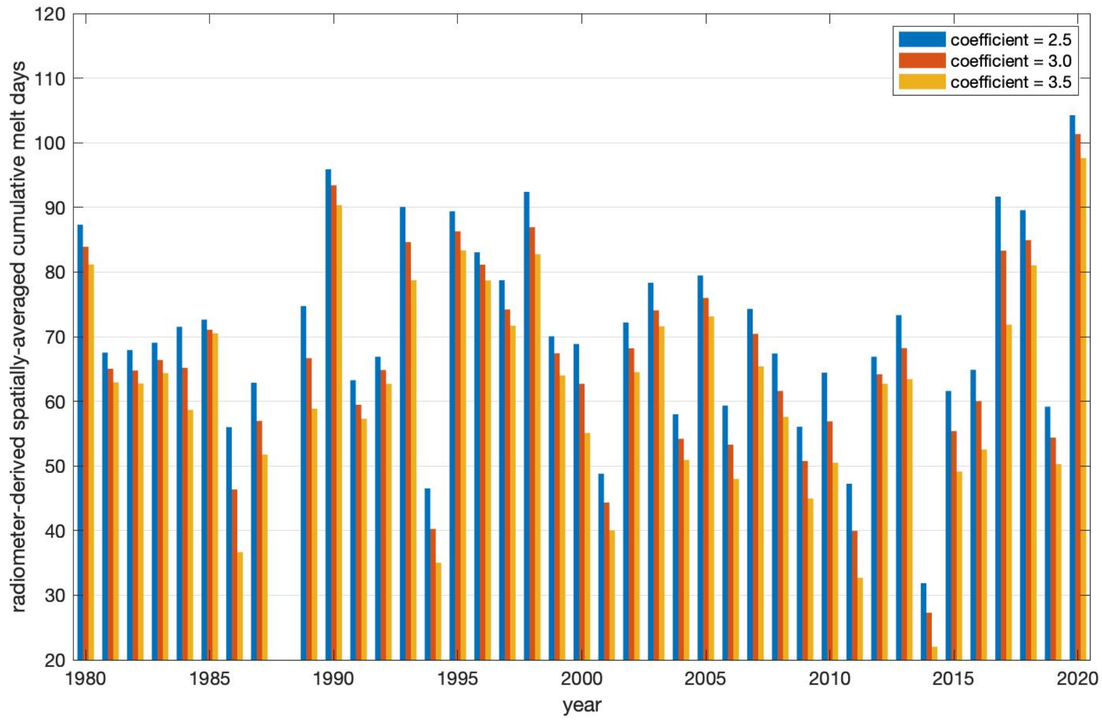
Additionally, to further address the reviewer's comment and to bring new content to our study, we have analyzed the sensitivity of the melt detection algorithms to their threshold values; the most uncertain part of the algorithms. Please see below for results of this analysis for both melt products, which confirm that our conclusion of the exceptional 2019/20 melt year is robust.

SMMR/SSMI uncertainty analysis

The figure below shows the brightness temperature temporal evolution for one pixel in North GVIIS (lat, lon = -72.287, -67.579) over the course of the 2019/2020 melt season. Three algorithm thresholds, defined by the mean brightness temperature plus the standard deviation multiplied by a coefficient, are also indicated. The coefficient of 3 is nominal (Torinesi et al. 2001) and was used our study, as in other past studies. For this single pixel, the resulting cumulative melt days for each threshold are: coef: 2.5 -> 107, coef: 3 -> 103, coef: 3.5 -> 99.

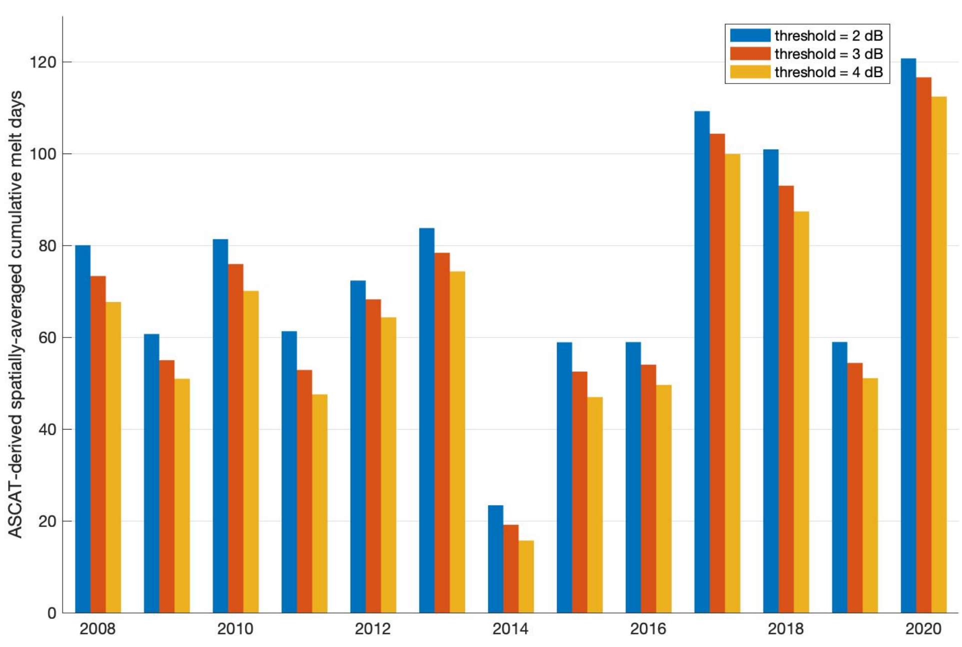


Using each of these three coefficients (2.5, 3, 3.5), we have also calculated the spatially-averaged cumulative melt days over the northern GVIIS for all melt seasons from 1979/1980 to 2019/2020 (apart from 1987/1988 due to missing data), the results for which are shown in the bar plot below. We estimate that changing the coefficient from 2.5 to 3.5 produces a change in the cumulative melt days in the order of 10%. Importantly, as shown by the plot below, the 2019/2020 melt season is exceptional regardless of the choice of coefficient used in the SMMR/SSMI analysis.



ASCAT uncertainty analysis

The bar plot below shows the ASCAT-derived spatially-averaged cumulative melt days over northern GVIIS for melt seasons from 2007/2008 to 2019/2020 using three thresholds; 2 dB, 3 dB (the value we use in our study), and 4 dB. As this plot shows, varying the threshold does not alter the conclusion that the 2019/2020 melt season experienced the highest cumulative melt days.



The revised manuscript will include the following sentences in Results section 4.2: "*Since addressing uncertainties associated with microwave data products of binary melt/no melt information is challenging, this study uses two distinct microwave remote sensing techniques and algorithms to build further confidence in our conclusion. Moreover, our analysis of the sensitivity of the melt detection algorithms to decreasing/increasing their threshold values confirm that the 2019/2020 melt season was a 32-year record."* We will also include the two bar plots above in the Supplementary Information for our revised paper.

In addition, the reader should be made aware of the limitations in using the depth retrieval algorithm applied to optical satellite imagery (see Sneed and Hamilton, 2011 and Pope et al., 2016).

Regarding limitations associated with using the depth retrieval algorithm applied to optical images, in our revised manuscript, we will also add the following sentence about the assumptions the depth algorithm makes: "*This approach makes a number of assumptions, including that the lake bottom has a homogenous albedo, there is little to no particulate matter in the water column to alter its optical properties, and that there is minimal wind-induced surface roughness (Sneed and Hamilton, 2007)."*

I think it could also be highlighted more clearly in the manuscript that this record surface melt in 2019/2020 was unrelated to foehn-driven melting (see specific comments).

We agree with this comment and will add extra sentences to clarify this in the Abstract, Results and Conclusion sections.

Once the authors address these issues and my specific comments below, I can therefore recommend that this manuscript is suitable for publication in The Cryosphere.

Specific comments:

Line 25: consider quantifying 'low-speed' winds here, i.e. ≤ 7.5 ms^{-1}

We will do as suggested.

Line 63: Consider adding either an additional sentence here or an additional panel in Figure 1 showing which other ice shelves experienced increased meltwater ponding in 2019/2020 to provide further context. Although it is mentioned in the Figure caption that Larsen C, Wilkins and Bach also experienced ponding, this is not immediately clear from Panel A.

We thank the reviewer for these suggestions. We would prefer not to add an additional panel to figure 1 as that would mean that panel (b) would need to become smaller, meaning lakes on GVIIS would be less visible. Instead we plan to add the following additional sentence about ponding on the Wilkins, Bach and Larsen C to the main paper: "*However, as Fig. 1a shows, in 2019/2020, surface meltwater ponding was also prevalent on the northwestern Larsen C, the northern Wilkins (also visible in the bottom left corner of Fig 1b), and the northern and northwestern Bach ice shelves*."

Line 66: I suggest either enlarging the latitude-longitude labels on Panel A, or adding them to Panel B. In addition, I think it would be helpful to add an arrow labelling ice flow direction.

We will enlarge the latitude-longitude labels on panel a) and also add an arrow to indicate ice flow on panel b).

Line 87: Quantify surface summer melt rates (e.g. up to ~400 mm w.e. yr, Trusel et al., 2013).

Thank you for reminding us of this useful reference, which we will refer too when stating the melt rates of the northern GVIIS.

Line 90: Reynolds (1981) discusses observations of moulins on George VI, so consider modifying this sentence.

Thank you for pointing this out. We will modify this sentence to state that moulins have been observed in the pressure ridges near to the margins of GVIIS by Reynolds (1981).

Line 91: Quantify 'high' basal melt rates and thinning rates.

Basal melt rates are < 6 m yr^{-1} in the southern GVIIS (Adusumilli et al., 2020), and thinning rates have been < 2 m y^{-1} in this area (Pritchard et al., 2012). We will state these statistics (and references) in our revised manuscript.

Line 93: Consider adding one line in this paragraph quantifying ice flow speeds on northern GVIIS, since ponding preferentially occurs on slower-moving ice.

This is a good suggestion and we will add the following sentence to this section: "*The ice shelf decelerates as it flows westwards across the sound, with ice velocities on the northern GVIIS varying from ~ 400 m yr^{-1} near the grounding line to ~ 30 m yr^{-1} near Alexander Island (Bishop and Walton 1981).*"

Line 95: I suggest also citing Lucchitta and Rosanova (1998) here as well.

Thank you, we will add this reference.

Line 99: I suggest making it clearer in this paragraph that these three types of surface lakes form every austral summer to make it clearer you are not just referring to 2019/2020. In addition, consider adding sub-panels to Figure 1 to show examples of these three types, or a separate Supplementary Figure.

This is a good point; we will re-word this sentence to read "*On the northern GVIIS, three types of surface lake patterns tend to form each summer*". However, we currently do not plan to add an additional figure to our paper to highlight these three lake types. This is because although we observe these three lake types in 2019/2020, our study does not advance scientific knowledge about these lakes, which have previously been described in detail in the literature. For example, the first two lake types are described in detail in Reynolds (1981) alongside photos and diagrams (their figs 3a and 5), and the third lake type is the focus of the study by LaBarbera and MacAyeal (2011). These references are already noted in our paper.

Line 100: I suggest you also cite Hambrey and Dowdeswell (1994) here, and perhaps add to the end of this sentence that ice flowlines are surface manifestation of longitudinal foliation.

We will additionally cite Hambrey and Dowdeswell (1994) here, and add the following text to the end the current sentence *"….similar to the dominant pattern of lakes observed on the Amery Ice Shelf (Hambrey and Dowdeswell, 1994)*." However, we are not sure we fully understand the reviewers suggestion about stating that the "ice flowlines are the surface manifestation of longitudinal foliation", as to us, ‘ice flowlines” are simply the directions along which ice flows across the ice shelf, not only at the surface, but throughout the depth of ice.

Line 104: Please consider also citing Langley et al. (2016) and Arthur et al. (2020b) here, which record observations of down-ice lake advection on Langhovde Glacier and Shackleton Ice Shelf, East Antarctica.

We will add these references.

Line 111: I suggest continuing the final sentence with: ‘as described above, enabling it to support a large surface area of surface meltwater (Alley et al., 2018).

Thank for this good suggestion. We will add this extra text and reference.

Line 117: Be consistent with the use of northern/north GVIIS; I think northern is used most frequently throughout.

Previously we had used ‘on north GVIIS’ when trying to be brief (e.g. in sub headings, and figure captions), whereas we had used ‘on the northern GVIIS’ in the main paper text. However, we are very happy to use ‘northern’ (not ‘north’) throughout our revised paper.

Line 125: What is the uncertainty associated with this microwave brightness temperature product and the ASCAT product in Section 3.2? There could be more discussion of this.

Regarding uncertainties with the microwave radiometer and scatterometer products, please see our response to this reviewer’s general comment on page 2 of this response letter.

Line 129: I suggest adding one sentence after this along the lines of: the sensor measures the emitted energy from the surface and sub-surface, proportional to the brightness temperature (which increases with the presence of liquid water and which increases absorption and emissivity).

We thank the reviewer for this helpful suggestion, though we will modify their suggested sentence slightly to avoid overlap with the previous sentence, and to insert the following two sentences at the beginning of the paragraph: “*Microwave radiometer observations of melt, expressed as brightness temperatures, depend primarily on the snow temperature profile and emissivity (Zwally, 1977). When liquid water exists in the snow, there is a significant increase in the absorption, and therefore an increase in the microwave emissivity, resulting in higher brightness temperature*.”

Line 142: Briefly state why grid cells > 1700 m a.s.l. were masked out, presumably to only show data corresponding to ice shelf areas.

The reviewer is correct with their assumption. Cells > 1700 m a.s.l. were masked out such that only melt on ice shelves was predominantly analyzed. We will reword this sentence to read as follows: “*Grid cells with surface ice elevations > 1700 m a.s.l. were masked out so that melt over the ice shelves was predominantly analysed and to avoid large topographic features in the radiometers field-of-view (i.e., mountain peaks).”*

Line 148: Consider adding a citation here?

We will add a reference to Zwally (1977) here.

Line 161: I suggest adding a sentence explaining where this threshold comes from i.e. was based on empirical comparisons with QuikSCAT-derived melt (Ashcraft and Long, 2006).

We will add the following text to the end of this sentence: "*…..ASCAT signal is lower than the winter mean signal minus 3 dB, as proposed by Ashcraft and Long (2006) using a melt model and QuikSCAT Ku-band (13.4 GHz) observations*".

Line 166: Add 'scatterometer-derived' before 'cumulative melt days'.

We will add this.

Line 177: Perhaps briefly outline here the threshold-based algorithms other than NDWI that this method uses, i.e. NDSI and others, or else list which bands are used.

Adding details of all the bands and band combinations that we used to the manuscript would be extremely lengthy (at a minimum this would be two additional paragraphs, i.e. one paragraph per sensor, with 5 – 6 sentences in each paragraph), and given all this detail is in Moussavi et al (2020), which we follow exactly, we not think repeating this information in our paper is of value. We will, however, add a little more detail to the current text, and will re-word the section to read: "*Moussavi et al's (2020) method, developed separately for Landsat 8 and Sentinel-2, combines separate threshold-based algorithms to detect (1) lakes, (2) rocks, and (3) clouds. Optimal thresholds for each band and band combination (e.g. Normalized Difference Water Index (NDWI), Normalized Difference Snow Index (NDSI) and others) were determined by creating a training dataset based on selected Landsat 8 and Sentinel-2 images, which represented spectral properties of several classes (e.g. lakes, slush, snow, clouds, rocks, cloud shadows). Most notably, to classify liquid water-covered pixels, the NDWI is used (Pope et al., 2016; Bell et al., 2017), with NDWI thresholds of > 0.19 and > 0.18 for Landsat 8 and Sentinel-2, respectively".*

Line 182: Consider also citing Bell et al. (2017) and Arthur et al. (2020b) here.

We will add these references here.

Line 184: This depth-reflectance algorithm makes a number of assumptions which I think are worth briefly outlining in an additional sentence, including the assumption of homogenous lake bottom albedos, minimal wind-driven light scattering, etc (Sneed and Hamilton, 2007).

Please see our earlier response to a similar comment made by this reviewer on page 4 of this letter.

Line 185: There is no discussion currently in this section of false positives. How did you deal with these (if there were any), and were they manually removed?

The major issue with any image classification method, including ours, is false positives, which results in overestimation of lake areas. The only way to reduce these errors would be to post-process products, i.e. manual inspection of results, which is very labor intensive and is rather subjective. Therefore, we did not manually post process the images used in our study.

Line 218: I think 'warm' here more accurately describes foehn winds than 'hot'. Also suggest adding at the end of this sentence 'and commonly occur on the AP', and citing Luckman et al. (2014).

We agree re. foehn winds being 'warm' (not hot) and will change this. We will also add these additional words and reference.

Line 219: I suggest adding an indication here of how steep the topography is, e.g. maximum slope.

Having shown that foehn winds are not a dominant contributor to surface melt during this 2019/20 season, we think that the further examination of drivers for foehn winds (including the slope of Alexander Island on the NW margin of the GVIIS) to be beyond the scope of our current study.

Line 339: This is an interesting finding. I wonder whether it would be worth adding an additional sentence (either here or at the end of this section) explicitly summarising these observations demonstrate you can still get record melt when conditions are generally warmer, with no involvement of foehn wind events.

This is a useful point, and we will add the following sentence to the Conclusions to explain this: "*It is therefore notable that although the high melt event over the northern GVIIS is 2019/2020 was caused by warmer than average air temperatures, such local weather conditions were not foehn-driven*". Also, we will add additional sentences to the Abstract and Results to emphasize that the high melt in 2019/2020 was not foehn driven.

Line 393: Are there any measurements of firn air content/thickness on George VI?

There are a couple of studies that have modelled firn air content (FAC) over GVIIS (e.g Ligtenburg et al., 2014), but the spatial resolution of those studies is insufficient for us to be able to state differences in FAC between the north and south GVIIS.

Line 410: I don't think you can necessarily suggest that surface melt volumes were highest in 2019/2020 out of the prior 31 seasons without having explicitly derived volume estimates from imagery pre-2013.

We thank the reviewer for their comment about this, and having thought about this paragraph some more, we agree that it is best deleted from our revised paper.

Line 419: Change 'zero re-freeze' to 'no refreezing occurred'.

We will change this.

Line 434: Explain 'shoulder seasons' – do you mean colder seasons (autumn/winter?).

By "shoulder seasons" we meant all seasons excluding summer, but mainly winter. However, we now realize that our terminology is not clear, and we apologize for that. We will reword the sentence to read: "*This is predictable foehn flow behaviour, e.g., over the Larsen C, foehn winds are strongest in winter, when wind speeds are generally higher (Datta et al., 2019; Wiesenneker et al., 2018).*"

Line 446: Change 'back to 2013' to 'from 2013 to 2020' and suggest adding to the last part of the sentence: 'was also exceptional in areal extent and volume [..]'.

We will make these changes.

Line 454: Add AOI area here in brackets to remind readers of its size in comparison with Larsen B in the next sentence. Also, what was the maximum lake depth?

This is a good suggestion and we will add the area of the AOI (7850 km^2) in brackets. However we are not convinced that stating the maximum depth here in the Conclusions is suitable given this value appears to be rather an outlier. Instead, we will state this value in the Results.

Line 458: I suggest also citing Alley et al. (2018) here.

We will add this reference.

Line 460: Consider showing in a Supplementary Figure the lakes mapped in this study overlaid onto the areas classed as vulnerable to hydrofracture by Lai et al. (2020).

We thank the reviewer for this suggestion, which we will consider. Our main reservation for doing what they suggest is that we think such a figure would be too 'busy' to be easily interpretable, and therefore it would not serve it's purpose well. We could instead insert just a map of the vulnerable areas of the GVIIS from Lai et al. (2020), but in which case, it makes more sense to simply refer the readers directly to the figure in that study.

Figure S5: I notice there are two particularly deep lakes – why, out of interest, do you suggest this is?

We think that these areas may remain as topographic depressions from season to season due to the flow regime in this area. Additionally, their basin depths may have been enhanced due to enhanced lake bottom ablation in these areas. In other words, the greater the number of days that meltwater ponds in a specific area, the greater the effect of enhanced melting at the lake bottom due to the lower albedo of the lake water compared to the surrounding water. However, as our study focusses on the total volumes of surface meltwater each season, rather than behaviours of specific lakes, we suggest it is beyond the scope of the current study to discuss the evolution of these deep lakes in the paper.

Line 31: minor point, but check here and throughout that citations are listed chronologically.

We will do this.

Line 49: Add comma after 'However'.

We will do this.

Line 100: 'most extensive' rather than 'the largest'?

We will change this.

Line 106: Change en-echelon to 'en-echélon' and consider adding a brief description for those unfamiliar with this term, e.g. closely spaced, sub-parallel.

Thank you, we will make both of these changes.

Line 120: Change '(from 2013)' to 'from 2013-2020' and the same with '(from 2017)'.

We will change this.

Line 125: Insert 'passive' before 'microwave radiometer'.

As a radiometer is, by definition, a passive sensor, we have instead reworded this part of the sentence to read "… *was derived from microwave radiometer (i.e. passive) observations…".*

Line 132: Hyphenate 25 km.

We will do this.

Line 157: Hyphenate 4.45 km and consider briefly outlining the SIR algorithm.

We will reword this sentence to read as follows: "*The 4.45-km enhanced product is obtained by applying the Scatterometer Image Reconstruction (SIR) algorithm with filtering (Lindsley and Long, 2016), which is used to improve the spatial resolution of irregularly and oversampled data (Early and Long, 2001).*"

Line 159: Hyphenate 'SMMR-based'.

We will do this.

Line 173: Rephrase to 'to selected multispectral imagery (see paragraph below)'.

We will do this.

Line 197: Are italics needed in this paragraph?

We are happy to remove these italics.

Line 216: Remove 'periods'.

We will do this.

Line 220: citation should be Wiesenneker et al., 2018 (full reference is correct).

We will correct this, however we note that the following spelling is correct: "Wiesenekker" (not Wiesenneker).

Line 291: remove commas after 2020 and AOI.

We will do this.

Line 327: Change 'refreeze' to 'refreezing'

We will change this.

Line 390: Re-word sentence to ‘meltwater ponding is not observed in the optical imagery until mid-December’.

We will change this.

Line 495: Please add volume, issue and page numbers: 44(6), 837-869.

We will do this.

Additional references that we will add

Alley KE, Scambos TA, Anderson RS, et al. Quantifying vulnerability of Antarctic ice shelves to hydrofracture using microwave scattering properties. Remote Sensing of Environment 210: 297–306, https://doi.org/10.1016/j.rse.2018.03.025, 2018.

Arthur, J. F., Stokes, C. R., Jamieson, S. S. R., Carr, J. R., and Leeson, A. A.: Distribution and seasonal evolution of supraglacial lakes on Shackleton Ice Shelf, East Antarctica, The Cryosphere Discuss., https://doi.org/10.5194/tc-2020-101, 2020b.

Langley ES, Leeson A. A., Stokes. C. R., et al. Seasonal evolution of supraglacial lakes on an East Antarctic outlet glacier. Geophysical Research Letters 43(16): 8563–8571, 2016.

Lucchitta, B. K. and Rosanova, C. E. Retreat of northern margins of George VI and Wilkins Ice Shelves, Antarctic Peninsula. Annals of Glaciology 27: 41–46, 1998.

Luckman A, Elvidge A, Jansen D, et al. Surface melt and ponding on Larsen C Ice Shelf and the impact of fohn winds. Antarctic Science 26(6): 625–635, 2014.

Trusel, L. D., K. E. Frey, S. B. Das, P. Kuipers Munneke, and M. R. van den Broeke, Satellite-based estimates of Antarctic surface meltwater fluxes, Geophys. Res. Lett., 40, 6148–6153, doi:10.1002/2013GL058138, 2013.