Author response: Reviewer 1, Jan Lenaerts

Dear Dr Lenaerts,

Thank you for your review and for your positive outlook on our manuscript. We appreciate your apology and understand that the current pandemic has caused additional strains on scientists. We provide a point-by-point response to your suggestions below, and we will mark any changes in red on the updated manuscript so that these can by reviewed by you again with ease. Once we have permission from our editor to upload the new manuscript, we will do so. The manuscript has now improved in clarity and the motivation is now stronger with your suggestions.

Many thanks,
Jenny Turton, on behalf of all authors.

L46: The study by Datta et al. (2019) is missing.
Yes, this is indeed an oversight. The original study was conducted in 2017, prior to the Datta paper, however it slipped by us when we were updating the manuscript ready for submission. We have now read it and added it to the manuscript where necessary. Upon uploading the final comments, we will highlight exactly where these changes are.

More generally, I find it challenging to pinpoint the originality of this paper within the realm of recent papers on foehn-driven melt. How does this study build on from the SEB study by Kuipers Munneke et al. 2012 and from Turton et al. 2018? What are some of its unique features e.g. the methods, the data, the results? How is this study different from the Kuipers Munneke et al. 2018 study?
Editor comment: initial notes from the editor also highlighted that we did not make the contribution to science clear enough. Hopefully these changes also satisfy this comment.
There are an increasing number of foehn-driven melt studies in the literature now. However, we are confident that our study provides some points of novelty and development in our understanding. We will ensure that this is clearer in the introduction and discussion of the updated manuscript. Furthermore, we provide additional information here in response to some of the specific studies you have highlighted.
Firstly, this study builds on that from Turton et al. 2018 by investigating the *impact* of the föhn events on the SEB of the Larsen C. In Turton et al. 2018, föhn were identified and their spatial and temporal distribution were investigated, however there was no presentation of the melting and SEB change due to the föhn events.
The Kuipers Munneke et al. (2018) study focuses on winter föhn melting specifically, and a much shorter period of time (in observations) than the current study. Whereas in the current study, we analyse all seasons and have 4 years of observations. We compare our winter föhn-melt results with those of Kuipers Munneke and do not see evidence of winter melting. Largely this is due to 1) the location of the AWSs are further away from the inlets and valleys where winter melt was observed 2) winter melt in Kuipers Munneke et al. (2018) may be specific to the particular season which was observed. Therefore, a combination of the Kuipers Munneke et al. (2018) study and the current study suggests that more work investigating the role and presence of winter-föhn melting should be conducted in the future.

The Kuipers Munneke et al. (2012) study focuses on the SEB over Larsen C in a more general or climatological sense. Föhn winds are only introduced as a case study of a particular event. Here, we specifically investigate the föhn impact and compare to non-föhn SEB periods. Our study builds on that of Kuipers Munneke et al. (2012) by looking at the case study in context of all föhn events over a four-year period. The current study uses the SEB model and AWS data from Kuipers Munneke et al. (2012). Furthermore, additional AWS data is used to run the SEB on the remnants of Larsen B ice shelf. This observationally-derived SEB model output (as opposed to reanalysis or atmospheric model output) has not previously been published and is here analysed to look at the impact of föhn on a location which is often overlooked in other studies (i.e King et al. 2015, Kuipers Munneke et al. 2018, only look at föhn on Larsen C, but not on Larsen B). Therefore, results SEB model data at AWS1 is novel.

The current study uses a longer observational record for analysing föhn impact on the Larsen C (four years) than in previous studies. The majority of previous studies have analysed a single föhn event or a single season with a few föhn events (e.g King et al. 2017, Kuipers Munneke et al 2012). We also analyse SEB data from a new location, north of Larsen C, which is also prone to föhn events, and has previously only been looked at in atmospheric model output. Many föhn-melt studies (e.g. Datta et al 2019, Kuipers Munneke et al. 2018) complement modelling with satellite images of melt extent or duration, but do not go into much or any detail on the SEB components responsible for the melt, as we do here. For these reasons, we believe that this work is original and provides additional information on the impact of föhn winds on the Larsen C ice shelf. We will ensure that the novelty and motivation for this study are highlighted in the updated manuscript, so that our developments are clearer to the reader.

Figure 1: It would be more logical to zoom in from subpanels a to c, instead of zoom out.
We have now re-ordered the figure to zoom in.

Table 2: it is unclear (1) if this is a new result of simply a repetition of Turton et al. 2018; and 2) whether the data gaps are considered when calculating the foehn frequency. I assume the percentages represent ratios between foehn episodes and total duration of non-missing data- this this is important to clarify; 3) what's the agreement/overlap between AWS and AMPS is in general, i.e what is the frequency of events in AWS only, AMPS only, and both- and why is that?
Thank you for your questions, we hope to clarify your individual sections below:

1) This is a repetition of Turton et al. (2018), but presented as frequency percentages rather than absolute numbers as in the original 2018 study. We wanted to include this data so that the discussion surrounding the importance of frequency of föhn winds on the SEB is clear. We have changed the table caption to make this clearer.
2) Data gaps are considered when calculating the foehn frequency yes. We have included this in the caption now to make it clear.
3) This information was presented and discussed in Turton et al. (2018). As it is relevant here too, we have now included some of this information in the manuscript.

Table 3: it would be worthwhile representing the numbers in the first columns as percentages as well. What does the 31.4% and 33.7% represent exactly?

We are not 100% sure what Dr Lenaerts means by this comment. We think that he is referring to the first two rows (as opposed to columns). These are not written as percentages, as the values are used to calculate the percentages in other rows and are needed to put these percentages into context. For instance, the 31.4% is the percentage of föhn days on which melt is also observed. Out of a total of 86 föhn days, 27 of them experience melting, which is 31.4% of the time (from AWS2). Similarly, the 33.7% is the same calculation but for AMPS data. We are happy to review this table again should this still not be clear.

Figure 3: this figure needs to be improved considerably. The dots are difficult to see, black and red is difficult to separate, and units need to be added. Use subscripts as necessary.
Thank you for your suggestions. We have now increased the size of the dots and changed the marker shape for non-föhn events to aid clarity, included units and subscripted the labels.

Table 4: this might be nitpicky but try to use subscripts and superscripts in this table. Would it be an idea to combine Table 4 and Figure 5, to avoid table redundancy/overload?
The sub/superscripts were also raised by reviewer 2. However, Table 4 has now been removed, and additional panels added to Figure 5 to combine the two and remove the excess table information. Please note that the original Figure 4 and 5 have switched numbers to better reflect their position in the manuscript now that Table 4 is removed. Thank you for this suggestion, which has streamlined the manuscript.

Figure 4: color scale is not useful for this purpose (this would be a color scale for 0 in the middle, ranging from negative to positive numbers). Consider changing the color scale.
We have now altered this to better reflect the data we are showing.

Figure 5: use consistent terminology throughout.
Updated in the new figure, thank you.

Figure 6: same here- this color scheme is not useful for this purpose.
We have now re-plotted the figure with a new colour scheme that is more useful for the data we are showing.

Figure 7: mind the units in brackets.
Units now in brackets and sub/superscript used.