Response to Editor, minor corrections

Dear Editor,

Thank you for your feedback on the manuscript and the invitation to submit our revised manuscript following minor amendments below. We provide point-by-point responses here with author comments in red and italics used for specific manuscript sections, as well as a marked-up manuscript with changes highlighted in red. We appreciate the detailed reviews and editing of this paper during a difficult time.

Dr Jenny Turton, on behalf of all co-authors.

L31: perhaps include a reference to a hydrofracturing paper – _eg. Bell, Kingslake or Banwell.
Thank you, we have now included Robel and Banwell (2019). This section now reads as: '*The collapse of Larsen A and B was facilitated by a process known as hydrofracture, whereby ice is weakened due to drainage of surface melt water into crevasses and increased pressure from ponds of standing meltwater forming on the ice shelf surface (Scambos, 2002; Robel and Banwell 2019). Recently, Robel and Banwell (2019) confirmed that the rapid rate of collapse of Larsen B (only a few weeks) was caused by an anomalously large, sudden and widespread surface melt event, which triggered successive or simultaneous hydrofracturing.'*

L74: errant comma after '(fluxes directed towards the surface are defined positive), ' _
Removed now.

L88: 'aim to extend the scope' _is not really sufficient justification for the study. Please tell us plainly how this study is novel. Both reviewers raise this issue, and I noted it in my initial assessment. It is clear from the paper that the study is indeed new, but that it necessarily builds on existing work by yourself and other authors. I think this just requires better 'selling' _in your introduction, and also in your conclusions.
Thank you for this suggestion. Based upon yours and the reviewer's comments, the introduction and conclusion has been changed considerably. Please see the marked manuscript for specific sentences in the conclusion. Particular focus has been paid to the introduction and these sections have now been included towards the end of the introduction:
'Most recently, Datta et al. (2019) analysed how the frequency of föhn winds influences snow melt, density and depth of water percolation over Larsen C using a regional climate model and remote sensing data. Studies investigating how föhn winds specifically influence the Surface Energy Budget (SEB) components, which are then responsible for melting, are less common and therefore explored here.' …

'However, these studies have only focused on a number of case studies, during particular seasons with a large number of föhn winds, or for a particular location on Larsen C. The interannual and seasonal influence of föhn winds on the SEB and melt characteristics are currently lacking, and therefore all seasons are investigated in this study. Our current understanding is largely from analysing extreme melting episodes related to föhn winds (e.g November 2010; King et al. 2017, Kuipers Munneke et al. 2012). Whether föhn winds are responsible for melting under more typical conditions, and if so which of the SEB components are influenced, are not as well understood, and are therefore explored in this study. Due to the break-up of Larsen B, observations on this ice shelf are limited, and previous studies investigating the role of föhn winds have focused on Larsen C. Here, we use a SEB model along with observations on the remnants of Larsen B (Scar Inlet) to understand the potential impact of föhn winds in this more northerly setting, for the first time.' …

'In this study, we aim to extend the scope of previous studies by analysing the composite effects of föhn against non-föhn periods on the SEB and melt production for both the Larsen C and Larsen B (remnants) ice shelves, inter- and intra-annually. By doing so, we investigate the impact of föhn winds on each season with the hypothesis that the impact is highest in spring, when föhn winds are more frequent. Furthermore, we analyse observationally-derived model output from a previously unpublished dataset (on Scar Inlet) in combination with high-resolution AMPS output from 2009 to 2012, to provide a wider spatial analysis than many previous studies.'

Table 3: Following Jan's suggestion, since you have included percentages in brackets for some rows, why not include for all? That gives us a quick idea of the % of melt days, foehn days as well as the foehn days with or without melt. Also, the final 'foehn' _in the caption is capitalised – _is this intentional?

The table has been adjusted slightly for ease of understanding and the percentages have now been added in. The capital was a typo which has now been changed. The table caption has been changed to reflect the updates. It now reads as: *'Table 3: The representation of surface melt from observation-derived data at AWS2, alongside AMPS model output interpolated to the same location. The total number of days with observations for 2009 to 2012 is 1439, which is used for the calculation of percentages for rows 2-4. The average E_{melt} values are daily averages over the same period. The total number of föhn and non-föhn days are the same for both AWS and AMPS as a result of the föhn identification (Sect. 2.4), föhn conditions must be identified in both to be classified as föhn. The number of föhn and non-föhn days are the same in AWS and AMPS as this was a criterion for the detection of föhn winds in Turton et al. (2018).'*

L530: the first sentence of the conclusion is rather long. Could you cut it in half?

This has now been changed to shorten but also to answer the comments on novelty. It reads as: *'The discrimination between föhn and non-föhn conditions provides a robust understanding of the impact of föhn on components of the SEB and ultimately, surface melt. Furthermore, by assessing the more general response to föhn, as opposed to individual events, we now know the impact of particularly frequent föhn periods on the surface melt.'*

L541: can you tell us how the results of this study differ from those published previously? At the moment, this rather reads as if it's just another paper in a long line of similar studies. Given the concerns about novelty (see reviews and my comments previously, and above), I would appreciate a strong defence of why this particular paper is important.

Thank you for raising this. We have greatly improved the manuscript in this regard throughout, but particularly in the introduction, discussion and conclusions. As reviewer 2 also points out, the novelty is in both the length of the observations being used (as opposed to case studies or specific seasons) and in the analysis of all föhn conditions to understand the relationship between föhn and melt (as opposed to specific 'strong' föhn events or anonymously frequent periods). The results are therefore important, as we can now say for certain that spring-time föhn events have the largest impact on the melt by extending the melt season and increasing the energy available for melt. Previously, this was hypothesised based on a case study analysis of spring 2010 föhn events.
We also now know that spring 2010 was an anonymous springtime compared to other years due to the high number of föhn events. Interannual analysis shows us that föhn impact on the annual melt production is significantly larger in years with a high frequency of föhn events.

Furthermore, with the analysis of the AWS1 SEB, we have now observed that föhn winds directly impact the SEB on Larsen B also- which was previously only hypothesised from model output due to the collapse of the shelf prior to observations. 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 detail about the SEB components responsible for the melt, as we do here.

Previous studies such as Bevan et al. 2018 and Weienekker et al. 2018 looked over a longer time period than our study, but only investigated spatial melt patterns and the number of föhn events per year, but did not look into the seasonal distribution of these föhn events, and therefore pinpoint specifically the föhn-induced melting. For example, if most of the föhn events in the early 2000s were during winter, these would not have had such a large impact as in another year with fewer föhn events but clustered in spring and summer.

Finally, another aspect of novelty is the detailed investigation of the success of AMPS at representing the total amount of surface melt, melt associated with föhn events and melt in different seasons (also associated with föhn events). Previous studies have shown the success and weaknesses of AMPS at identifying föhn winds from the upper atmospheric features and near-surface meteorological conditions, but not the surface melt (King et al. 2017, Turton et al. 2018, Kirchgaessner et al. 2019). This particular aspect was missing from the conclusions, so has been included now.