List of all relevant changes

1. Added a small correlation study between fohn occurrence and sea ice concentration.

Point by Point Response to Reviewers

RC2-

Thank you for your comments and suggestions. We believe this manuscript has improved significantly with your suggestions and we sincerely appreciate your valuable contributions. We have addressed your comments below marked with [Author Response].

I commend the authors for taking the time to improve the quality of the paper. The tone of the narrative is much more compelling and now fixes many of the previous issues of claiming novelty. The manuscript is nearly ready for publication and will surely be a great addition to the literature on ice-shelf instability, but I have one major comment and then some minor comments I would like to see addressed before publication.

Major comment

Abstract and Line 325-326: I was glad to see the authors include discussion on the Massom et al., 2018 paper which discussed how a lack of sea-ice cover allows swells to apply a strain to the ice-shelf fronts which happened during the collapses of the Larsen A and B. Although, I am not sure if the authors can claim the offshore foehn wind direction pushed sea ice away from the calving front without making a small analysis of this. While there are papers that discuss sea ice being influenced by large scale circulation patterns that create northwesterly flow across the Antarctic Peninsula (see Turner et al., 2002, Massom et al., 2006, Massom et al., 2008), to my knowledge there have been no studies that specifically connected foehn winds to sea ice conditions east of the Antarctic Peninsula. Likely the northwesterly direction of the foehn wind is the same as the northwesterly flow mentioned in previous studies, but perhaps it is only during these foehn periods when the sea ice is being blown off the ice shelf front. It could be a very interesting and insightful result if we see that the sea ice primarily responds to foehn winds and then this would really complete the argument for the importance of foehn winds on ice-shelf instability. Therefore, I recommend that if the authors want to connect the occurrence of foehn winds to sea-ice displacement, then they should analyze if there is a sea-ice response to periods of foehn winds or at least if there is a correlation between seasonal foehn wind occurrence and sea ice fraction. The sea ice fraction variable is readily available as an output in the ERA5 reanalysis.
[Author Response] **This is a very insightful suggestion. We have added a small fohn occurrence and sea ice concentration study summarized in a new figure (Figure 5). It is clear that there is a negative correlation between fohn occurrence and sea ice concentration, both in the long term but also on short time scales (hours to days) (Lines 211-217)**

Minor comments

Line 53: Fix parenthesis in citation.

[Author Response] **We have corrected the manuscript.**

Line 56: Figure 1b shows January 10, 1993, which was not during the collapse of the Larsen A or B. Please fix figure reference in text.
[Author Response] **Thank you for identifying this oversight. We have altered the manuscript to exclude Figure 1b.**

Line 105: Please elaborate more on the RACMO temperature biases? It is a little concerning to see the model has trouble simulating high temperature extremes when being used to study melting causes by high temperature extremes.
[Author Response] **We did a little deeper research and found that RACMO has a slight warm bias likely due to model resolution, as well as under/over-estimates shortwave/longwave radiation because of how clouds and moisture are simulated. This may also be why temperature is slightly warmer. We have added these comments and references into the manuscript.**

Line 118 – 121: I still don't understand how the authors verify that the FöhnDA is the most accurate detection method. Looking at Supplementary Table 1, it appears the false negative and false positive foehn detections are simply foehn detections that did not agree with the FöhnDA. These false positives and negatives should be classified as something like detected agreement or disagreement. One way to test if a foehn event is "real" is to test with a different method such as an isentropic test (pressure levels showing a difference).
[Author Response] **Thank you for this comment. It was not clear that we compared two other fohn identification studies to AWS observed and our ML algorithm. I have made this more clear in the table notes.**

Line 151: Can the authors elaborate on how the variations in fohn jet location and wind strength explain why the SCAR inlet and Larsen C ice shelf are still intact? It seems like the difference in annual surface temperature is the main reason why these further south ice shelves are still intact as regions along the Larsen C receive frequent foehn winds. (Datta et al., 2019; Turton et al., 2018)
[Author Response] **Yes, temperature gradient does drive fohn melt strength, however the fact that there is no fohn jet on SCAR inlet may be why it is still intact. We see how the wording in the manuscript is not clear and have altered it for clarification.**

Line 163: The addition of figure 2b and the inclusion of the total melt hours percentage are very helpful and show that the foehn wind produces melt efficiently. It still would be good to know how frequent the foehn winds are as a percentage of all hours during the summer instead of just when melt is present.
[Author Response] **We have altered Figure 2b to include the percent of all hours that fohn winds occur. Thank you for the suggestion.**

Line 176: Replace with "large-scale"
[Author Response] **Thank you, the manuscript has been altered.**

Line 179: Previous studies have shown this to not be entirely true. Elvidge et al., 2016

explains the physical mechanisms for linear foehn winds which cause melt at the Larsen C ice shelf terminus and nonlinear foehn events which only cause melting by the base of the mountains. Still melting does occur more often at the base of the Larsen C mountains than at the ice shelf terminus. Please correct this sentence to account for the limited foehn melting that does occur at the ice shelf terminus.
[Author Response] **This is a good point. We have altered the manuscript to say "However, the vast size of the LCIS limits the amount of föhn-induced melt at the terminus." and added reference.**

Line 180: Good analysis, but the authors should cite Turton et al., 2018 which shows that foehn is detected more frequently at the base of the Larsen C mountains (see Cole Peninsula AWS vs AWS2).
[Author Response] **Another good point. We have added the citation to the manuscript.**

Line 194: Should also cite Rott et al., 1996 here as that study first discusses the northwesterly flow that pushed the sea ice from the ice-shelf front
[Author Response] **The reference has been added to the manuscript.**

Line 226-228: The authors should cite Massom et al., 2018 and Turner et al., 2002 here as they discuss the lack of sea-ice cover and the atmospheric circulation patterns that caused the offcoast wind. Also going back to the major comment, this could be a good place to show if foehn winds progressively pushed the sea ice away from the ice-shelf front during the austral spring and summer.
[Author Response] **We have added this reference and added reference to figure 5.**

Line 257-258: It could be helpful to reference Figure 6b at the end of the sentence regarding the LSR without foehn-related melt.
[Author Response] **We have added the reference to Figure 6b.**

Line 275 – 277: I still do not understand why the temperature gradient alone does not explain the surface melt difference between ice shelves. The temperature gradient should also explain why foehn winds on the intact ice shelves do not produce large amounts of surface melt right? Unless the authors can explain that the foehn winds cause a different temperature change response between the collapsed and intact ice shelves. In fact from looking at Figure 5e and 5f, the temperature difference between foehn and non-foehn is slightly larger on the Larsen C than the Larsen A and B.
[Author Response] **Yes, this is a good point. Temperature is a driver of the strength of fohn winds and their ability to melt. Areas that are colder will not experience as much melt. We have added a sentence that discusses this idea in the discussion section ("Temperature gradient however, could explain why fohn wind events cause less melt on more southern ice shelves and may cause super melt events on collapsed ice shelves because temperature is already elevated on more northern ice shelves prior to the effect fohn has on temperature.")**

Line 341-342: Again regarding the sea ice, this is something not really proven in the manuscript. An analysis of the foehn wind/sea ice extent relationship would be helpful and make the manuscript even more compelling.
[Author Response] **We have added a sea ice concentration study as you suggested. Thank you!**

Works Referenced

Datta, R. T., Tedesco, M., Fettweis, X., Agosta, C., Lhermitte, S., Lenaerts, J. T. M., and Wever, N.: The Effect of Foehn-Induced Surface Melt on Firn Evolution Over the Northeast Antarctic Peninsula, Geophysical Research Letters, 46, 3822–3831, https://doi.org/10.1029/2018GL080845, 2019.

Elvidge, A. D., Renfrew, I. A., King, J. C., Orr, A., and Lachlan-Cope, T. A.: Foehn warming distributions in nonlinear and linear flow regimes: a focus on the Antarctic Peninsula: Foehn Warming Distributions in Nonlinear and Linear Flow Regimes, Q.J.R. Meteorol. Soc., 142, 618–631, https://doi.org/10.1002/qj.2489, 2016.

Massom, R. A., Stammerjohn, S. E., Smith, R. C., Pook, M. J., Iannuzzi, R. A., Adams, N., Martinson, D. G., Vernet, M., Fraser, W. R., Quetin, L. B., Ross, R. M., Massom, Y., and Krouse, H. R.: Extreme Anomalous Atmospheric Circulation in the West Antarctic Peninsula Region in Austral Spring and Summer 2001/02, and Its Profound Impact on Sea Ice and Biota*, 19, 3544–3571, https://doi.org/10.1175/JCLI3805.1, 2006.

Massom, R. A., Stammerjohn, S. E., Lefebvre, W., Harangozo, S. A., Adams, N., Scambos, T. A., Pook, M. J., and Fowler, C.: West Antarctic Peninsula sea ice in 2005: Extreme ice compaction and ice edge retreat due to strong anomaly with respect to climate, J. Geophys. Res., 113, C02S20, https://doi.org/10.1029/2007JC004239, 2008. Rott, H., Skvarca, P., and Nagler, T.: Rapid Collapse of Northern Larsen Ice Shelf, Antarctica, Science, 271, 788–792, https://doi.org/10.1126/science.271.5250.788, 1996.

Turner, J., Harangozo, S. A., Marshall, G. J., King, J. C., and Colwell, S. R.: Anomalous atmospheric circulation over the Weddell Sea, Antarctica during the Austral summer of 2001/02 resulting in extreme sea ice conditions: ANOMALOUS ATMOSPHERIC CIRCULATION OVER THE WEDDELL SEA, Geophys. Res. Lett., 29, 13-1-13–4, https://doi.org/10.1029/2002GL015565, 2002.

Turton, J. V., Kirchgaessner, A., Ross, A. N., and King, J. C.: The spatial distribution and temporal variability of föhn winds over the Larsen C ice shelf, Antarctica, Q.J.R. Meteorol. Soc., 144, 1169–1178, https://doi.org/10.1002/qj.3284, 2018.

RC1-
Thank you for your comments and suggestions. We believe this manuscript will improve significantly with your suggestions and we sincerely appreciate your valuable contributions. We have addressed your comments below marked with **[Author Response]**.

Review for "The role of föhn winds in Antarctic Peninsula rapid ice shelf collapse" by Laffin et al.

General comments

I am pleased to see that the authors have taken on board the majority of my previous comments, which I thank them for. The authors now also have a much more complete reference list, and in general, the paper is written more clearly, though there are still some vague sentences that I comment on in my line by line comments below. Before those general comments, I have summarized some additional general comments below.

On many occasions of the paper a 'critical stability depth' of lakes is now referred too, e.g. in the Abstract (line 16) and in the introduction (line 46). The authors also often state that critical stability lake depth is 1 m (though on one occasion they state that this depth is 3.5 m). The papers by Glasser and Scambos (2008) and Banwell et al. (2013, 2014) are often referred too after these statements, but none of those papers actually talk about a critical stability depth for hydrofracture. What Banwell et al (2014) do state is that the average depth of lakes on Larsen B in the Landsat image prior to ice shelf break up is 1 m, however they do not suggest that is a depth threshold for break up. Glasser and Scambos (2008) do not mention any specific lake depth threshold for breakup, and nor do Banwell et al (2013; in fact, in that study, they model lakes under the assumption they are all 5 m deep). As it is the volume of water in a lake that determines the 'load' actually on the ice shelf surface, not the water depth, I do not think that the authors this paper under review should talk about critical depth threshold, especially as the 1 m 'threshold' has not been suggested to be a threshold in the literature previously. Some sort of lake volume threshold may exist, perhaps combined with a lake density threshold, and those may help to determine when an ice shelf is primed for rapid break up via chain reaction lake drainage, but I am not aware of a paper that has specifically studied this.
[Author Response] **We agree and never want to miss represent other authors' findings and research. We have altered the manuscript in the abstract, introduction and conclusion to better clarify and represent previous research. We no longer state that 1m or 5m lake depths are critical lake depths, only that they are observed or modeled lake depths.**

In general, I think it is too speculative to suggest that the "extant ice shelves are less likely to experience rapid collapse due to föhn-driven melt so long as surface temperatures and föhn occurrence remain within historical bounds" (from the final sentence of the abstract). Such an idea could be discussed as part of the Discussion, but personally. I do not think there is a need to include this idea in the abstract. It seems fair that this suggestion may be true on the basis that föhn wind occurrence is less common on these ice shelves (I think Larsen C and the Scar Inlet, but please see my comment below) as the authors discuss, but on occasions the authors state that lakes do not form on these ice shelves, whereas in fact lakes often do form on Larsen C, where huge impermeable ice lenses have also been found (see Hubbard et al 2016).
[Author Response] **We agree that our comments may be to speculative, however we still feel that this**

a reasonable result that should be stated in the abstract. We did however change the sentence to; "extant ice shelves <u>may be less</u> likely to experience rapid collapse due to föhn-driven melt so long as surface temperatures and föhn occurrence remain within historical bounds."

Related to the above comment, the authors often mention 'extant ice shelves' (lines 19 and again on line 20 in the abstract), and 'remaining ice shelves' (e.g. line 354) on multiple occasions through the revised manuscript. Such statements are vague, and could be referring to all remaining Antarctic ice shelves, or just those remaining on the AP, or just those in Eastern AP. In fact, in such statements, I believe the authors are just referring to the Larsen C and Scar Inlet. So they should either state these two ice shelf names each time they are discussed, or at least, state 'East AP ice shelves' on these occasions. Related to this point, I also wonder if the paper title should include 'east' or 'eastern', given it is only the eastern AP ice shelves that are studied in this paper.
[Author Response] **We agree clarifying which ice shelves we mean by "extant" is important. We have clarified our meaning throughout the manuscript by using "SCAR inlet and the LCIS" or "extant ice shelf on the eastern AP."**

Finally, I don't know of any evidence (observational nor modeling) that suggests Larsen A experienced rapid chain reaction style break up, i.e. like Larsen B did (Banwell et al 2013). So the authors need to remove all references to this process having happened on Larsen A.
[Author Response] **We agree we do not want to speculate how Larsen A collapsed, so we have changed all language that suggests it collapsed through hydrofracture chain reaction break up.**

<u>Line by line comments</u>

8 – 11: Unlike Larsen B,s I do not know of any evidence that suggests that Larsen A also experience cascading hydrofracture events. Yes there may have been, but there is no evidence, so this sentence needs rewording.
[Author Response] **We agree we do not want to speculate how Larsen A collapsed, so we have changed all language that suggests it collapsed through hydrofracture chain reaction break up.**

10: I believe this should be 'long' period ocean swell, rather than 'large' period. E.g. see Massom et al (2018). Check throughout the paper.
[Author Response] **We have altered the manuscript to say long period ocean swells. Thank you for identifying this oversight.**

11: "During collapse, surface observations indicate föhn winds were present on both ice shelves" – this is very vague. What kind of surface observations are you referring too? Also, for Larsen B at least, observations in the form of optical satellite imagery were very sporadic (maybe just 3 Landsat images in a 2 month period?).
[Author Response] **We have clarified our meaning of observations with field observations and satellite observations.**

17 – 19: Be specific with what 'extant ice shelves' you are referring too.
[Author Response] **We have clarified our meaning throughout the manuscript by using "SCAR inlet and the LCIS" or "extant ice shelf on the eastern AP."**

19 – 21: As I state above, I think this sentence may be too speculative to include in the abstract.
[Author Response] **We agree that our comments may be to speculative, however we still feel that this a reasonable result that should be stated in the abstract. We did however change the sentence to; "extant ice shelves <u>may be less</u> likely to experience rapid collapse due to föhn-driven melt so long as surface temperatures and föhn occurrence remain within historical bounds."**

23: Be more specific; which 'ice shelves' disintegrated? Also, it seems odd to start the paper with this conclusive statement (lines 23 – 24) saying that disintegrations were caused by regional warming trends, if you then go on to argue in this paper that föhn winds also played a role! Perhaps this sentence should be removed from this location and stated/discussed elsewhere?
[Author Response] **We agree that we do not want to contradict our findings with this sentence so we have taken this out of the manuscript.**

28 – 32: Again, I do not think there was evidence that chain reaction lake drainage was observed (or modeled) prior to Larsen A's collapse, so this sentence needs rewording. I also suggest breaking up the long list of references after the word 'hydrofracture', as not all of these references are related to hydrofracture. Suggest moving some of these references to earlier in the sentence after 'melt pond flooding'. Glasser and Scambos (2008) should also be mentioned after 'melt pond flooding'. And Banwell et al (2013) should be added to the list after 'hydrofracture'.
[Author Response] **We altered the sentence to not suggest the LAIS was observed to collapse through chain reaction lake drainage. We also altered the location of references and added those you suggested. Thank you.**

41 – 42: 'In addition Massom et al., (2018) concluded that a lack of summer sea ice allowed large period ocean swells to reach the ice shelf calving front.' - It's unclear why this sentence is mentioned separately as surely it is part of (2) in the previous sentence. But in any case, this whole section from lines 38 – 44 seems very repetitive given basically the same detail in is included in the previous paragraph (lines 30 – 37). So I suggest delete much of this section.
[Author Response] **We have taken out line 41-42 as suggested, because it was repetitive. We thank you for your suggestion about deleting likes 38-44 because of their repetitive nature, but we feel that those lines, other than those already deleted, provide a good background of the "4 essential prerequisites for rapid collapse" theorized by Massom et al., that are not covered in lines 30-37.**

46/47: Again, as mentioned above, I think the authors should reconsider their suggestion that 1 m is a critical depth threshold for rapid ice shelf break up. 48/48: This definition of hydrofracture is useful, but ideally it should come when 'hydrofracture' is first mentioned, which is currently on line 32 (I think).
[Author Response] **Thank you, we agree, see response above.**

51: Suggest replacing ''at critical water depths' with 'that rapidly drain by hydrofracture'.
[Author Response] **This is an excellent and clarifying alteration. We have incorporated it into the manuscript.**

67: I am unclear why just 'late season fohn melt reduces firn pore space'. Surely this process has the

same effect on the fir at any time during the melt season?
[Author Response] **Our understanding is that late season fohn melt does not stay liquid for long, it will freeze overnight filling in surface pore space, whereas early and mid summer melt can stay liquid for longer, leading to firn percolation, runoff, evaporation, drainage etc.**

78: Replace 'does' with 'did'.
[Author Response] **We have altered the manuscript.**

83: Further to my general comment above, I Suggest replacing 'each ice shelf' with 'each eastern AP ice shelf'. (Assuming you are referring to all eastern AP shelves here, and not just LAIS and LBIS? Maybe state the specific shelves in brackets?) 86: Again, please clarify what 'extant ice shelves' are being referred too.
[Author Response] **We have altered the manuscript, see note above.**

126/127: In the second part of the sentence: 'Therefore, we consider RACMO2 simulated estimates of surface melt caused by föhn winds to be conservative and likely higher in regions where föhn winds are funneled and concentrated', it sounds as though it is being suggested that modeled fohn winds will be higher than in reality. But based on the fact you also say modeled estimates are conservative, I think you may mean to say the opposite?
[Author Response] **We mean to say that fohn winds smaller than the model simulations are likely to exist, but because model resolution is not simulated and therefore melt is not calculated but is likely greater under their influence. We have altered the manuscript to clarify this point.**

144: Is the ML algorithm being referred to in this sentence fohnDA? State that if so.
[Author Response] **We have added reference to the ml algorithm (FöhnDA)**

175 – 188: Another reason for Scar inlet's stability may be that it has had lots of sea ice buttressed up again it until very recently (when it broke up); you could also mention this.
[Author Response] **We have added reference to decreased buttress force from sea ice.**

194 - 196: I suggest putting 'e.g.' in front of Massom et al (2018) seeing as this study did not focus on the LAIS. And as I state above, I do not think any study has suggested that chain reaction lake drainage (aka hydrofracture cascades) contributed to Larsen A's collapse.
[Author Response] **We have altered the manuscript to better represent previous research and understanding of both ice shelf collapse events, that does not include linking LAIS collapse with chain reaction lake drainage.**

218: As I also mentioned in my last review, Banwell et al (2014) should be referenced after the following sentence: 'We find mean melt lake depth to be between 1.38-6.86 meters depending on lake location and föhn influence, which exceeds the average lake depth of the LBIS lakes prior to collapse (1 meter)'.
[Author Response] **We have added reference to this paper and apologize for this oversight from the previous review.**

219: Where did a critical depth of 3.5 m come from? Earlier the authors said this was 1 m. But in any case, as I explain in my General Comments, I don't think that talking about a critical lake depth

is useful anyway.
[Author Response] **Thank you for identifying this oversight, we have altered the sentence and reference to include a simulated 5m lake depth.**

238: State what ice shelves you are referring too by 'all major ice shelves'.
[Author Response] **We have included a reference to all easter AP ice shelves in parentheses.**

252/253: Again, very vague; clarify which ice shelves are being talked about.
[Author Response] **We agree and have added…"Our analysis of firn density or available firn pore space identifies significant differences in ice shelves that have collapsed (LAIS, LBIS) and those that remain intact (SCAR inlet, LCIS)"**

253 – 255: I suggest moving the description of the firn densification process to the introduction.
[Author Response] **We have altered the manuscript.**

252 – 261: Somewhere it would be useful to mention that there is some evidence of firn densification, surface ponding, and expansive ice layers on Larsen C. See Hubbard et al (2016).
[Author Response] **We have added a sentence to discuss this point. "It is important to note that there is evidence that the LCIS experiences regions of firn densification through melt processes, however these regions are mostly focused close to the AP mountains, likely formed from the location of fohn jets."**

290/291: Similar to my earlier comment, I am unsure why just late season melt is being talked about here.
[Author Response] **Our understanding is that late season fohn melt does not stay liquid for long, it will freeze overnight filling in surface pore space, whereas early and mid summer melt can stay liquid for longer, leading to firn percolation, runoff, evaporation, drainage etc.**

303: Which ice shelf is being referred to here?
[Author Response] **We have clarified which ice shelves we mean.**

301: Comment on why this additional snow is relevant.
[Author Response] **The additional snow mass leads to a thicker firn layer that can store more water before melt lakes form. Additionally this added additional stress to the ic shelf.**

314: I think you mean 'basal melting'
[Author Response] **We did mean basal melting and have made that change. Thank you.**

317: Again, I don't know why the authors talk about a 'critical melt lake depth of stability' (it is not in the Banwell et al 2013 reference given here).
[Author Response] **We have altered this language to better represent previous work and no longer use "critical melt lake depth"**

329: Which 'extant ice shelves' are being referred too? (FYI: Some southwest AP ice shelves, specifically George VI and Wilkins have had lots of melt and surface ponding in recent years. E.g.

see Banwell et al 2021).
[Author Response] **We have change the manuscript to say "extant eastern AP ice shelves"**

341: Again, remove mention of critical 1 m lake depth. 358: Clarify that AP ice shelves are beginning referred to here.
[Author Response] **We have altered the manuscript to no longer mention a critical lake depth. "long period ocean swells to trigger large-scale hydrofracture cascades on the LBIS and possibly LAIS…" We have also clarified which ice shelves we mean.**

360: what 'region'? And surely fohn winds should be mentioned in this sentence too? What about future fohn winds?
[Author Response] **We have altered the sentence to clarify our meaning and mention fohn winds. "Nevertheless, this research highlights a new understanding behind föhn melt mechanisms for ice shelf collapse and suggests that SCAR inlet and the LCIS may remain stable so long as surface liquid water from melt and precipitation remains within historical bounds"**.

Figures

Figure 1: Clarify that the LAIS and LBIS in the figure are no longer present, and perhaps give their collapse dates in the first sentence of the caption.
[Author Response] **We have added clarification of which ice shelves collapsed as well as when collapse occurred.**

Figure 3: In the caption, I think 'graph' would be a more appropriate word than 'curve'.
[Author Response] **We have changed the manuscript.**

Figure 6: What time period are the data for?
[Author Response] **We have added the data time period**

Additional references

Banwell, A. F., Datta, R. T., Dell, R. L., Moussavi, M., Brucker, L., Picard, G., Shuman, C. A., and Stevens, L. A. The 32-year record-high surface melt in 2019/2020 on the northern George VI Ice Shelf, Antarctic Peninsula, The Cryosphere, 15, 909–925, https://doi.org/10.5194/tc-15-909-2021, 2021.

Hubbard, B. et al. Massive subsurface ice formed by refreezing of ice-shelf melt ponds. Nat. Commun. 7:11897 doi: 10.1038/ncomms11897 (2016).