List of all relevant changes

1. Changed the title of the manuscript.
2. Created a new narrative of the manuscript to put fohn winds in a supporting role and not the trigger of collapse.
3. New Introduction to provide new narrative and more in depth reference review of fohn winds in the region. (also includes shifting a review of fohn winds from the results section to intro)
4. Moved the liquid-to-solid ratio (LSR) equation from results to the methods section.
5. Added a previously done fohn identification sensitivity study in the methods from Laffin et al., (2021).
6. Altered Figure 2b to include fohn melt frequency.
7. Added additional discussion about sea ice during collapse events.
8. Altered the conclusions to not suggest fohn winds trigger collapse, but played a role in it.
9. Added other clarifying remarks throughout.
10. Added 17 new references not previously mentioned in past manuscript versions.

Point by Point Response to Reviewers

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 "*Antarctic Peninsula ice shelf collapse triggered by fohn wind -induced melt*" by Laffin et al.

General comments

This paper has the potential to be a very interesting study about the possible influence of föhn winds on the large-scale collapse past events of the Larsen A and B ice shelves, and on potential future break-up events of Antarctic Peninsula ice shelves. However, in its current state, it is poorly written and badly structured in many places, e.g. why are föhn winds only defined and described in the Results and not in the Introduction? I give more examples in my line-by-line comments below.

[Author Response] - **We agree that the structure of the manuscript can be improved, especially with an overview of föhn winds in the introduction section. This manuscript was originally submitted to a short form journal which is why it was structured differently then a typical Cryosphere article. We overlooked this fact when we re-wrote the manuscript for The Cryosphere and have made changes to the manuscript that are more in line with The Cryosphere structure. Please see the below comments that show and explain the changes to this manuscript in more detail.**

Additionally, the current paper includes extremely limited references to relevant work that has already been done (particularly regarding föhn winds, but also regarding surface melt processes in general). A good example of this is the sentence in the abstract (line 13/14) which reads: "However, no studies examine the timing, magnitude, and location of surface melt processes immediately preceding these disintegrations." This statement about the Larsen A and B ice shelves is entirely incorrect as there have been many studies that have examined surface melt processes on these ice shelves, e.g. Scambos et al (2000, 2003, 2004) Glasser and Scambos (2008), Leeson et al (2017, 2020), Banwell et al (2013; 2014), Kuipers Munneke et al (2014), Lenaerts et al (2017) and Robel and Banwell (2019), to name just a few. I suggest that this sentence (and similar sentences in the Introduction) are reworded to specifically focus on the research to-date regarding effects of foehn winds on surface melt on ice shelves. Currently this paper references only a few such föhn wind studies; the following key studies about föhn wind induced ice-shelf melt are missing: Datta et al (2019), Wiesenekker et al (2018), Bozkurt et al (2018), Kirchgaessner et al (2021), and I suspect a good few others. Kirchgaessner et al (2021) is particularly relevant to the current study as it also focuses on AP ice shelves. As I am not 100% up to date with the ice-shelf melt-related föhn wind literature myself, it has been hard for me to give this paper thorough review given that the authors have not placed their study in the context of existing knowledge from other literature.
[Author Response] - **We agree this manuscript is limited in it's references and in particular articles about föhn winds and föhn-induced surface melt. See lines 60-84 of the new manuscript which includes a more detailed explanation of fohn winds. We have also added an overview of the most relevant research on fohn winds in the region.**
Also, in regard to your comment about Line 13/14:, *"However, no studies examine the timing, magnitude, and location of surface melt processes immediately preceding these disintegrations."*, and others like it, the passages were meant to show that little research was done on time scales shorter than annual or seasonal, however, we see that the way these comments are written make it seem like there is no research on föhn winds and surface melt. We have changed all passages in the manuscript to better frame this study among the rich array of studies on föhn winds in the region.

Finally, unlike the LAIS, I think I agree with the statement that the 'LBIS collapse was not directly related to the impact of föhn-induced melt', e.g. as the authors state on line 190 and in the Conclusion. However as the initial LBIS collapse on Feb 9 2002 coincided with a föhn wind event, I wonder if the authors have considered the idea that that föhn wind event may have helped produce sufficient surface meltwater such that the drainage of multiple surface lakes via hydrofracture cascades may have been triggered (i.e. 'chain reaction' lake drainage), thereby resulting in LBIS's near complete collapse a couple of weeks later (see Banwell et al 2013, Robel and Banwell, 2019). So in that sense, I am wondering what the authors think about the idea of föhn winds having been an indirect cause of LBIS's break-up?
[Author Response] - **This is a very interesting question that inspired us to change the manuscript. After reading Massom et al., 2018, which theorized a useful conceptual framework for rapid ice shelf collapse and identifies large period ocean swells as the trigger mechanism for the collapse of the Larsen A and B ice shelves, we decided to alter our interpretation of our findings. Fohn winds were present at the time of collapse for both ice shelves which produced enhanced surface melt rates that caused extensive melt ponds over each ice shelf. Additionally, the direction of fohn winds (from the**

west/northwest direction) pushed/melted sea ice and fast ice away from the calving front of both ice shelves which allowed large ocean waves to trigger collapse, which was also discussed in Banwell et al (2017). We have changed the manuscript to show that without the extensive melt ponds and lakes enhanced by fohn winds, and the wind direction that pushed protective sea ice away from the calving front, large-scale hydrofracture cascaded and subsequent collapse would not have taken place. See the new abstract/introduction/conclusion that re-frames our results with this narrative. We have also changed the title of the manuscript to not suggest fohn winds triggered collapse, but instead played a supporting role in the rapid collapse of LA and LB ice shelves.

Specific comments

Line 11: 'Add 'grounded' before 'glaciers'.
[Author Response] - **This was changed to clarify grounded glaciers. See line: 9**

12/13: In addition to surface melting, a mention of lake drainage via hydrofracture, and/or cascades (or a chain reaction) of lake drainage events could be mentioned here.
[Author Response] - **We have altered the abstract to include hydrofracture cascades. Line: 12**

13/14: See 'general comment' above.
[Author Response] - **We have altered the abstract to include hydrofracture cascades. Line: 12. It was already discussed in the manuscript but we have also added additional clarification in the introduction. .**

16: Mention the paper's specific focus on Antarctic Peninsula shelves.
[Author Response] - **We clarified the study region. Line: 14**

18: 'less' vulnerable compared to what?
[Author Response] - **We agree and have altered the abstract significantly and have taken this language out for clarification. See line: 18-21**

22: 'Forensic' is the wrong word as there is no link with crime.
[Author Response] - **We meant to say that examination of past events is useful. We altered "forensic" with re-evaluation. Line: 27**

26 – 28: Similar to the comment I made about line 13/14 in the abstract, this sentence is entirely incorrect and does not reference prior key studies regarding both surface melt processes on ice shelves and föhn winds specifically. I suggest you add at least the references I mention above, but I will have missed some.
[Author Response] - **We have completely re-written the introduction to include valuable background and references as well as frame our findings in the context of other studies. See the new introduction.f**

29 – 30: Be clear that you are using a ML method you developed in a previous study (at least that

is what I am guessing), i.e. Laffin et al (2021), and reference that. Currently this sentence is vague.
[Author Response] - **Yes, this method was developed in Laffin et al., 2021. We have clarified this fact in Line: 109**

30- 32: You state that your method is the 'most accurate', but you do not state what other methods/studies you are comparing it too, and nor do you state how you came to such a conclusion? Did you do some sort of intercomparison study? If so, that should be briefly explained.
[Author Response] - **We did complete an intercomparison sensitivity study detailed in Laffin et al., 2021, comparing other identification methods. We have added the sensitivity study statistic table from Laffine et al., 2021 into the supplement. See supplement and Line: 119**

33 – 41: This is interesting, as it totally contradicts the statements made in the Abstract and Introduction about there being no studies that have looked at such ice shelf melt/collapse processes! Additionally, by 'warm water intrusion', I assume you are referring to enhanced basal melting? And another good example of a study that demonstrated how sea swell caused ice shelf frontal break up is Banwell et al (2017).
[Author Response] - **We do see the contradictions in this statement and those made in the abstract and throughout the manuscript. We added additional background for ice shelf stability and fohn winds in the region. We also clarify our mention of "warm water intrusion" to basil melt as well as reference Banwell et al., (2017) in regard to ocean swell stress on the calving front.**

43: For the 1 meter lake depth reference for LBIS, the two references given are incorrect. They should be Glasser and Scambos (2008) and Banwell et al (2014).
[Author Response] - **We have fixed this embarrassing oversight.**

47: Regarding 'ice shelves into sections with aspect ratios that support unstable rollover', Burton et al (2013) would be a very appropriate reference to add.
[Author Response] - **We have added this reference. Line: 52**

48: Robel et al (2019) is incorrect. It should be Robel and Banwell (2019).
[Author Response] - **Fixed. Line: 52**

49 - 51: The first part of the following sentence requires references, and the second part is incorrect (for the reasons I give above in General Comments): '*Previous research acknowledges enhanced surface melt during years of collapse and the presence of föhn wind events in the region, however, no attempt to produce a timeline of total melt quantity or melt caused by föhn before and during ice shelf breakup has been undertaken*'
[Author Response] - **With the alteration of the narrative we have completely taken this sentence out of the manuscript.**

52/53: Poor English. Reword.
[Author Response] - **We have changed the manuscript and taken this sentence out, and replaced it with Line: 57.**

55 – 58: These questions are good; clear and precise.

[Author Response] - **Thanks!**

59/60: 'spatial distribution' of what? Poor English.
[Author Response] - **We meant to say the distribution of foehn-induced surface melt. We have clarified this sentence to read, "To address these questions we consider three metrics: Section 3.1 explores the total annual surface melt quantity induced by föhn winds and how melt is spatially distributed across each ice shelf…".**

85: It needs to be much clearer that the current study uses a föhn detection algorithm developed in a prior study (Laffin et al. 2021), and NOT in this study (at least that is my understanding from the current paper).
[Author Response] - **Yes, this method was developed in Laffin et al., 2021. We have clarified this fact in Line: 109**

86 – 97: It would be interesting for the authors to compare how their algorithm compares to that used by Datta et al 2019 ('Foehn Index'; also used in Banwell et al. 2021) and perhaps other existing algorithms too. E.g., on what basis/using what evidence can the authors state that there '*method is the most accurate compared to previous work*' (without even giving reference to that previous work).
[Author Response] - **We did complete an intercomparison sensitivity study detailed in Laffin et al., 2021, comparing Datta et al 2019 and other identification methods. We have added the sensitivity study statistic table from Laffine et al., 2021 into the supplement. See supplement and Line: 119**

105: I think these should more accurately be described as ice shelf "areas" given that Larsen C is split into two areas. Also, I suggest listing those ice shelves/areas in this sentence.
[Author Response] - **We agree that this is not clear and have changed the sentence to say ice shelf areas, as well as name those areas in reference to Figure 1. Line: 130**

113: you have already defined AWS elsewhere.
[Author Response] - **We adjusted the manuscript.**

116 – 120: This useful definition/description about föhn winds needs to be moved into the Introduction; it does not belong here.
[Author Response] - **This is a great point. We have taken this out of the results and added a deeper look into fohn winds in the region into the introduction.**

121: '*AP winds from the west and northwest (*föhn *influence)'* is not clear. Are you suggesting that all winds from the W and NW on the AP are föhn? (If so, that isn't clear, and I assume not all winds from that direction are föhn?
[Author Response] - **In this region, because of the location of the Antarctic Peninsula range, most winds from the W/NW will have some fohn influence. We have added more references and discussion of fohn winds in the introduction. Lines: 57-78**

121/122: I assume this is a result from the current study, but that needs to be made clear if so.
[Author Response] - **Yes, this is a result from this study. We included this information in Figure 2, however, we have made our results more clear with specific percentages from our findings to**

compliment the figure. Line: 163

129: ‘*The degree to which föhn winds impact surface melt on each ice shelf varies…*’ state what timescale(s) are being considered here.
[Author Response] - **We have altered this sentence to clarify the timescales. Line: 161**

131: Figure 5 is mentioned before figures 3 and 4 have been mentioned.
[Author Response] - **We have re-worked the manuscript and ensured all figures are identified chronologically.**

140/141: I simply do not know what the authors are trying to state by the following sentence: ‘*However no single factor, including föhn-induced melt rate, lessens the influence of all the other factors that contributed to these collapses.*’
[Author Response] - **We agree this sentence is confusing and have removed it from the manuscript.**

153/54: For the first part of this sentence, please acknowledge (and reference) other studies that have also established this fact.
[Author Response] - **Yes, there are other studies who have established this fact which we have referenced. Line: 183**

168: Banwell et al (2013) did not study Larsen A.
[Author Response] - **We have corrected this oversight.**

190: Please see the final paragraph in my ‘general comments’ above.
[Author Response] - **See our response above.**

211-225: It seems like some of this material (inc. equation1) should be in the Methods, not Results?
[Author Response] - **Yes, since the manuscript was originally submitted to a short form journal it was best placed in this section. We agree it is now better suited in the methods section. See lines 138-142**

229/230: Again, discuss this statement in the context of the findings of other studies.
[Author Response] - **We have altered the narrative to include out study with many other notable studies about ice shelf stability and fohn winds.**

251: Glasser et al 2018 should be ‘Glasser and Scambos (2008)’, and Glasser et al (2021) is not in the reference list.
[Author Response] - **We have fixed this embarrassing oversight everywhere in the manuscript.**

274: Satellite-derived depths of lakes are in Banwell et al (2014).
[Author Response] - **We have fixed this embarrassing oversight.**

278 – 281: The authors state the following two sentences, which I disagree with: ‘*The large melt volume in a relatively short amount of time spatially expanded and increased melt lake formation*

and depth, filled crevasses, increased water pressure on the crevasse tip and walls and triggered large-scale hydrofracture cascades that led to catastrophic disintegration of the LAIS (Scambos et al., 2000; Banwell et al., 2013). The same cannot be said about the LBIS'. The processes described in the first part of the sentence are what various studies have proposed caused the ultimate collapse of the LBIS, but I am not aware of any study have proposed the same mechanism for LAIS (Scambos et al 2000 or Banwell et al 2013 certainly did not).
[Author Response] - **Thank you for this comment. This is one of the reasons we have decided to shift the focus of the manuscript story to put fohn winds and associated melt in a support role for collapse and not the trigger. This change is discussed in more detail above.**

290: George VI is not a good example to use here as it has very constrained, compressed ice flow.
[Author Response] - **We agree and have altered this sentence. Line: 328**

293: '*more stable'* than what? This is vague.

294: 'than previously thought' – by who? Give references.
[Author Response] - **We agree this is vague. We have altered this sentence, Line: 329 "We conclude that föhn winds and the associated surface melt played a significant role in the collapses of the LAIS and LBIS, while extant AP ice shelves are not likely to collapse from föhn-induced melt and hydrofracture in today's current climate."**

Figures

Figure 2: I assume the data shown in panels b) and c) are from RACMO2, but that should be clarified.
[Author Response] - **Yes, the data shown is from RACMO2, we have clarified this in the manuscript caption.**

Figure 3: Again, where is the data shown in this figure derived from?
[Author Response] - **Yes, the data shown is from RACMO2, we have clarified this in the manuscript caption.**

Figure 4: Again, please state the source of the data.
[Author Response] - **Yes, the data shown is from RACMO2, we have clarified this in the manuscript caption.**

Figure 5: Again, state the source of the data in the caption, and specify what kind of data it is. 'data' is vague.
[Author Response] - **Yes, the data shown is from RACMO2, we have clarified this in the manuscript caption.**

Figure 6: Again, state data source. And for a), should this be 'total melt'?
[Author Response] - **Yes, the data shown is from RACMO2, we have clarified this in the manuscript caption.**

References (those in **bold** are not referenced in the current paper)

Bozkurt, D., Rondanelli, R., Marin, J. C., & Garreaud, R. Foehn event triggered by an atmospheric river underlies record-setting temperature along continental Antarctica. Journal of Geophysical Research: Atmospheres, 123, 3871–3892. https://doi.org/10.1002/ 2017JD027796, 2018.

Banwell, A. F., Cabellero, M., Arnold, N., Glasser, N., Cath- les, L. M., and MacAyeal, D.: Supraglacial lakes on the Larsen B Ice Shelf, Antarctica, and Paakitsoq Region, Greenland: a comparative study, Ann. Glaciol., 55, 1–8, https://doi.org/10.3189/2014AoG66A049, 2014.

Banwell, A.F., Willis, I.C., Goodsell, B., Macdonald, G.J., Mayer, D., Powell, A. and MacAyeal, D.R. Calving and Rifting on McMurdo Ice Shelf, Antarctica. Annals of Glaciology. doi: 10.1017/aog.2017.12, 2017.

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.

Burton, J., L. Mac Cathles, W. Grant Wilder, The role of cooperative iceberg capsize in ice-shelf disintegration. Ann. Glaciol. 54, 84–90, 2013.

Cape, M. R., Vernet, M., Skvarca, P., Marinsek, S., Scambos, T., & Domack, E. Foehn winds link climate-driven warming to ice shelf evolution in Antarctica. Journal of Geophysical Research Atmospheres, 120(21), 11,037–11,057. https://doi.org/10.1002/2015JD023465, 2015.

Datta, R. T., Tedesco, M., Fettweis, X., Agosta, C., Lher- mitte, S., Lenaerts, J. T. M., and Wever, N.: The effect of Foehn-induced surface melt on firn evolution over the north- east Antarctic peninsula, Geophys. Res. Lett., 46, 3822–3831, https://doi.org/10.1029/2018GL080845, 2019.

Glasser, N. F., & Scambos, T. A. A structural glaciological analysis of the 2002 Larsen B ice-shelf collapse. Journal of Glaciology, 54(184), 3–16, 2008.

Kirchgaessner, A., King, J. C., & Anderson, P. S. The impact of Föhn conditions across the Antarctic Peninsula on local meteorology based on AWS measurements. Journal of Geophysical Research: Atmospheres, 126, e2020JD033748. https://doi. org/10.1029/2020JD033748, 2021.

Kuipers Munneke, P., Ligtenberg, S. R. M., Van Den Broeke, M. R., and Vaughan, D. G.: Firn air depletion as a precur- sor of Antarctic ice-shelf collapse, J. Glaciol., 60, 205–214, https://doi.org/10.3189/2014JoG13J183, 2014.

Leeson, A. A., Van Wessem, J. M., Ligtenberg, S. R. M., Shepherd, A., Van Den Broeke, M. R., Killick, R., et al. Regional climate of the Larsen B embayment 1980–2014. Journal of Glaciology, 63(240), 683–690. https://doi.org/10.1017/jog.2017.39, 2017.

Leeson, A. A., Forster, E., Rice, A., Gourmelen, N., & van Wessem, J. M. (2020). Evolution of supraglacial lakes on the Larsen B ice shelf in the decades before it collapsed. Geophysical Research Letters, 47, e2019GL085591. https://doi.org/10.1029/2019GL085591

Lenaerts, J. T. M., Lhermitte, S., Drews, R., Ligtenberg, S. R. M., Berger, S., Helm, V., Smeets, C. J. P. P., Broeke, M. R. van den, van de Berg, W. J., van Meijgaard, E., Eijkelboom, M., Eisen, O., and Pattyn, F.: Meltwater produced by wind–albedo interaction stored in an East Antarctic ice shelf, Nat. Clim. Chang. 2017 71, 7, 58–62, https://doi.org/10.1038/nclimate3180, 2016.

Robel, A. A. and Banwell, A. F.: A speed limit on ice shelf collapse through hydrofracture, Geophys. Res. Lett., 46, 12092–12100, https://doi.org/10.1029/2019gl084397, 2019.

Scambos, T., Hulbe, C., and Fahnestock, M.: Climate-induced ice shelf disintegration in the antarctic peninsula, Antarctic Penin- sula climate variability: Historical and paleoenvironmental per- spectives, Vol. 79, 79–92, American Geophysical Union. Antarct Res. Ser., Washington, DC, 2003.

Scambos, T. A., Hulbe, C., Fahnestock, M., and Bohlander, J.: The link between climate warming and break-up of ice shelves in the Antarctic Peninsula, J. Glaciol., 46, 516–530, 2000.

Scambos, T. A., Bohlander, J. A., Shuman, C. U., and Skvarca, P.: Glacier acceleration and thinning after ice shelf collapse in the Larsen B embayment, Antarctica, Geophys. Res. Lett., 31, L18402, https://doi.org/10.1029/2004GL020670, 2004.

Wiesenekker, J., Kuipers Munneke, P., van den Broeke, M., & Smeets, C. A multidecadal analysis of Föhn winds over Larsen C ice shelf from a combination of observations and modeling. Atmosphere, 9(5), 172. https://doi.org/10.3390/atmos9050172, 2018

RC2-

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].

General comments

The authors here use a foehn wind detection algorithm to quantify surface melt magnitude and timing to claim that a foehn wind event pushed the Larsen A ice shelf past a critical stability threshold ultimately leading to its collapse in 1995. Meanwhile, since the Larsen B ice shelf experienced weaker foehn-related melt prior to its collapse in 2002, foehn winds likely preconditioned the ice shelf for collapse. While the foehn detection algorithm provides new, detailed insights into foehn jet positions and foehn wind related melt magnitude, the conclusions regarding ice shelf stability and collapse are underdeveloped and unsupported by the results. I give line-by-line results later, but globally I believe this manuscript suffers from two key elements.

The first is the lack of references to already published work that describe ice-shelf stability processes. Other times, relevant papers are cited, but their conclusions are misrepresented or not mentioned in the text. I give more detailed examples below, but one glaring example is the exemption of discussion from Massom et al., 2018 which discusses of ice shelf collapse triggered by sea ice loss and ocean swells. This paper is cited in the manuscript, but the results about how sea-ice loss and exposure to ocean swells triggered the collapse of the Larsen A and B are never discussed in this manuscript. The authors should consider these processes before claiming foehn winds triggered the collapse of the Larsen A.

[Author Response] - **Thank you for your comments and we agree. We agree this manuscript is limited in it's references and in particular articles about föhn winds, föhn-induced surface melt, and ice shelf stability. This manuscript was originally submitted to a short form journal, which limited the number of references. We felt, at the time we submitted this manuscript to The Cryosphere, the amount of references and background regarding föhn winds and ice shelf collapse processes was sufficient. However, after your valuable comments we have altered the introduction section to provide a clear overview of the current research to date on föhn winds and föhn-induced melt in the region along with the most relevant studies that aim to identify ice shelf collapse mechanisms. .**

Additionally, with your valuable comments and suggestions and after re-reading many previous studies we have decided to alter the narrative of the manuscript. After re-reading Massom et al., 2018, which produced a useful conceptual framework for rapid ice shelf collapse and identifies large period ocean swells as the trigger mechanism for the collapse of the Larsen A and B ice shelves, we decided to alter our interpretation of our findings. Fohn winds were present at the time of collapse for both ice shelves which produced enhanced surface melt rates that caused extensive melt ponds over each ice shelf. Additionally, the direction of fohn winds (from the west/northwest direction) pushed/melted sea ice and fast ice away from the calving front of both ice shelves which allowed large ocean waves to trigger collapse, which was also discussed in Banwell et al (2017). We have altered the story of the manuscript to show that without the extensive melt ponds and lakes enhanced by fohn winds, and the wind direction that pushed protective sea ice away from the calving front, large-scale hydrofracture cascaded and subsequent collapse would not have taken

place. We have also changed the title of the manuscript to not suggest fohn winds triggered collapse, but instead played a supporting role in the rapid collapse of LA and LB ice shelves.

The authors also cite Scambos et al., 2000, but appear to miss some important observations from that study. The authors in that study cite a storm as the trigger for the final disintegration of the Larsen A, but this fact does not appear in this paper's discussion of the Larsen A collapse. Is the foehn wind event here related to that storm mentioned in Scambos et al., 2000? Also, Scambos et al., 2000 mentions the Larsen A suffered major retreats in 1987 and 1989 which did not appear to be major foehn event years according to this study but did precondition the ice shelf for collapse which contradicts one of the authors' conclusions.

[Author Response] - **After extensive research to learn more about the storm mentioned in Scambos et al., 2000, and reviewing surface observations and model simulations, we determined that "the storm" mentioned was the powerful fohn wind events discussed in this study as there were no other major storm systems in the region.**

In response to your comment about Scambos et al., 2000 and the Larsen A retreats of 1987 and 1989 and the lack of fohn winds in those years. Just because there was a low fohn melt year does not mean fohn winds could not have played a role in collapse events. Also, it is hard to distinguish what caused these events because of the lack of observation and timing. We know that the events occurred in 1987 and 1989, but we do not have a clear research, first hand observations, or satellite observations of when the events actually occurred, so it is difficult to attribute a cause for these events. I have extensively researched satellite observations to try and triangulate the timing of these events, but I was unable to pinpoint the exact time of collapse. I also tried to identify other collapse event timing, such as the collapse of Larsen inlet, north of Larsen A in 1987, as well as the minor collapse events of Larsen B in 1998, 1999, and 2000, but again lack satellite observations.

The second issue is claiming one particular process could trigger an ice shelf collapse is a very high bar to pass given the multitude of other processes known to cause ice shelf instability. This manuscript would be much easier to accept as a reader if the authors move their focus away from the supposed novelty of their research and towards the value this research brings to an already rich field of research relating foehn-wind and ice shelf stability. In fact, there are moments when the authors claim to demonstrate a result for the first time when this result was already discussed in previous literature (see comment on line 51). The manuscript would be much easier to digest if the authors moved away from the claim that foehn winds triggered ice shelf collapse and instead focused on highlighting foehn winds as one of many processes that lead to ice shelf instability and the timing of the foehn winds may have played a supporting role in the collapse of the Larsen A.

[Author Response] - **We agree. As we mentioned in our first response, we have altered the title and the direction of the manuscript to show that fohn winds played a role in collapse, but did not trigger collapse. See above for a more detailed explanation.**

Line 13: Saying that there are no studies examining surface melt prior to disintegrations is incorrect. You should revisit the Van Den Brooke, 2005 GRL paper that you cited that explicitly studies surface melt on the Larsen B prior to its collapse.

[Author Response] - **These passages and others like it were meant to identify that little research fohn melt was done on time scales shorter than annual or seasonal, however, we see that the way**

these comments are written make it seem like there is no research on föhn winds and surface melt. We have changed all passages in the manuscript to better frame this study among the rich array of studies on föhn winds and surface melt in the region. See the new introduction.

Line 17-19: This claim is based on a premise that foehn wind and surface temperatures remain within historical bounds. The Antarctic Peninsula already experiences large temperature variability and is projected to become warmer which would actually make the extant ice shelves more vulnerable to foehn winds in the future (Siegert et al., 2019; Chyhareva et al., 2019).
[Author Response] - **We agree but wanted to make it clear that when we say extant ice shelves are less vulnerable to collapse than collapsed ice shelves because of large scale surface melt, that it is with the caveat that climate change is not considered here. We have changed the abstract (Line: 20-22) and the rest of the manuscript for clarification of our meaning.**

Line 25-27: The claim of novelty seems unwarranted here. Plenty of studies already cited in this manuscript plus some others discuss fohn-related melt mechanisms on the Larsen B ice shelf (see Datta et al. 2019). Plus, Van den Brooke et al., 2005 claims surface melt accelerated the rate of ice shelf retreat, but did not claim it was a leading contributor to the final collapse
[Author Response] - **We did not mean to suggest that there have been no studies that explore surface melt processes in the region, we only meant to identify small gaps in the research such as a lack of short time scale melt rates from fohn winds on Larsen A and B. We see that the current claim does not reflect this sentiment and have added a much deeper review of fohn win research in the AP region. See lines 39-82**

Line 33-41: I don't understand why the manuscripts claims surface melt as the lead cause of the ice shelf final collapse in the previous paragraph and then point out all the other well documented processes that also affect ice shelf final collapse.
[Author Response] - **There have been a few studies that point to large scale hydrofracture cascades caused by extensive melt ponds as a major factor that led to rapid collapse (Massom et al., 2018, Banwell et al., 2017), however, we understand your point and believe the sentence can be written better. We have completely changed the narrative of the manuscript and how our research fits within the plethora of previous research.**

Line 30: This is a strange claim to make in the introduction. If this claim is valid, then it should first be proven in the results and then mentioned in the conclusion. (Changed this sentence to reflect work in previous study)
[Author Response] - **Yes, we agree this statement feels strange. We changed the manuscript omitting this statement while also adding additional references.**

Line 51: This is repeating a claim from the first paragraph that incorrectly states no previous research has been done on foehn-related melt around ice shelf collapses. This study may certainly give further detail on the intensity and spatial distribution of the foehn wind, but certainly is not the first.
[Author Response] - **We agree as stated above, these comments and others like it have been changed.**

Line 54: The temperature trends on the Antarctic Peninsula are a bit more complicated than this. Bozkurt et al., 2020, Carrasco et al., 2021, and Turner et al., 2016 paint a different picture where temperature trends are periodic and dependent on the location along the AP.
[Author Response] - **We have completely changed this statement in the new introduction and narrative reframing.**

Line 57: Questions 1 and 3 are very important and reasonable questions to address in this manuscript. Question 2 is much harder to answer with certainty without considering all the other processes (atmospheric and non-atmospheric) that could affect ice-shelf stability.
[Author Response] - **Thank you, we plan to alter Question 2 to fit more in line with the new direction of the manuscript. We have altered question 2 to say: " 2) Did föhn winds and associated melt play a role in triggering the collapses of the LAIS and LBIS?"**

Line 87: What height is the air temperature measured at?
[Author Response] - **10 Meters, which we have added to the manuscript.**

Line 95: It is stated again that is foehn detection method is the most accurate compared to previous work without explaining what this previous work is or why it is the most accurate. I also believe this is not the first foehn detection algorithm to incorporate station observations and model output (see Turton et al., 2018). The authors should include some information comparing the foehn detection of their algorithm against other foehn detection algorithms even if that data is presented in Laffin et al., 2021.
[Author Response] - **We did complete an intercomparison sensitivity study detailed in Laffin et al., 2021, comparing other identification methods (Datta et al., 2019, Cape et al., 2015). We Discuss this in the methods section (Line: 119), and we have also added the sensitivity study statistics table in the supplement.**

Line 108: Perhaps explain which variables you used to make the two-tailed t-test statistic. "Mean of both ice shelves" is vague
[Author Response] - **We have clarified which variables were used in our t-test. Line: 133**

Line 115-119: This seems like background information on the physics of foehn winds that would be better suited in the introduction section.
[Author Response] - **We agree and we have altered the introduction to include additional explanation and references of fohn winds in the Antarctic Peninsula region. Line: 57-82**

Line 131: This might be a personal preference, but you should change your figure numbers/order if you are referring to figure 5 before figure 3.
[Author Response] - **Yes, we recognize this only provides confusion. We have altered the figure numbers to occur chronologically.**

Line 132: You should present some results on foehn frequency before presenting the foehn related melt percentage. This would help put these melt-percentages in a better context.

[Author Response] - **Yes, we** agree and have altered Figure 2b to include foehn occurrence and fohn melt occurrence discussed in line: 163.

Line 137: If the SCAR inlet is not impacted by a foehn jet, where is the foehn wind influence coming from?
[Author Response] - **SCAR inlet is not directly impacted by a föhn jet, but still experiences clear skies and weak föhn wind influence from the overall descending air that leads to warm winds and more importantly for this shelf, enhances shortwave radiation.**

Line 139 – 142: You are contradicting yourself or at least unclear in these two sentences. First you claim that the disparity in foehn-related melt percentages among the ice shelves implicates the foehn as a contributor to the LAIS and LBIS collapse. This is a very strong assertion. It explains differences in melt rates on the ice shelves but saying this contributes to their collapse is a stretch. Then the next sentence is confusing and muddles your message about whether foehn is important or not to collapse. Probably easier to say that your results indicate foehn is one of many processes that weakened the LAIS and LBIS.
[Author Response] - **We agree our assertion is unfounded and should state that fohn winds are one of the reasons the ice shelves destabilized. We have altered the narrative to place fohn winds in a more supporting role for collapse, rather than a trigger.**

Line 149-152: If extensive foehn wind jets help explain why the LAIS and LBIB collapsed, then why have they not caused the collapse of the LCIS? Is there research showing that having melting at the terminus is essential for an ice shelf collapse? Discussed above in the beginning of paragraph
[Author Response] - **We state in the manuscript *"LCIS on the other hand is impacted by four major jets and regularly experiences föhn-induced melt lakes, particularly in Cabinet inlet. However, the vast size of the LCIS does not allow the föhn-induced melt to reach the terminus. The föhn melt mechanism breaks down by mixing with cold air which reduces the intensity of the föhn jets from their peak at the base of the AP mountains to the calving front (Figure 3b)".* Massom et al., 2018 states that extensive melt ponds are an essential prerequisite for rapid collapse. With the change in direction of the manuscript we have fit our findings in the context of other research about collapse in a more clear manner.**

Line 153-154: Previous literature already shows that foehn winds have a major impact on ice shelf surface melt and the framing of this sentence makes your results sound novel when in fact it would be more accurate to say that your results back up and enhance preexisting knowledge while citing these sources. (Find fohn melt research on LA and LB)
[Author Response] - **We see how this statement makes our research sound novel. We have included a deeper reference pool for fohn winds in the region in the new introduction and references in the results section where we confirm previous work.**

Line 181: It's a bit confusing to see the authors use satellite imagery from the 1992/1993 melt season as an analogue to the 1994/1995 melt season, but then later argue that despite the two seasons had similar amounts of foehn-related melt, the reason the Larsen A collapsed in 1995 and not in 1994/1995 was the timing of the surface melt. This argument needs more analysis of the background state of the Larsen A in 1992/1993 versus 1994/1995 to explain more clearly what was so special in 1994/1995.
[Author Response] - **We do see how this argument may seem contradictory. With the new**

direction of this manuscript, we discuss a lack of sea ice in 1995 that triggered collapse, while in 92/93 sea ice was present during most of the summer and so protected the calving front from collapse. See Lines: 304-314

Line 204-205: The total surface melt results are interesting, but would considering the size of the ice shelves change the perception of importance in regard to ice shelf destabilization? For instance, the Larsen C is much larger than the SCAR inlet ice shelf so total melt amounts would be difficult to compare. Melt per area would be a better metric.
[Author Response] - **We already calculated for the mean melt over the entire ice shelf, but have clarified this in the manuscript. Line: 244**

Line 212-214: The statement about the future resilience of the other ice shelves is problematic as it ignores potential future changes in foehn wind patterns. Especially since I believe your foehn wind detection algorithm only detects foehn winds when the temperature is above 0°C. There could be foehn events that currently do not push the temperature above this threshold which are not considered by your algorithm. But theoretically, if air temperatures rises along the Larsen C, then your algorithm would start detecting more foehn wind events. Deleted and discussed later in the conclusions and discussion.
[Author Response] - **You are correct that our algorithm only identifies fohn winds above freezing and with climate change more southern locations will receive more fohn induced melt. Its hard to identify what that impact will be however it would be a great future direction of study. We have taken this statement out of the manuscript.**

Line 227-228: The liquid-to-solid ratio (LSR) analysis here includes foehn-related melt and non foehn related melt. As mentioned earlier, it would be helpful to know the foehn wind frequency according to your detection algorithm in order to judge the significance of this result.
[Author Response] - **Agreed, we added frequency stats see above. .**

Line 244-245: There are likely many other differences between the Larsen A and B and the other ice shelves beyond foehn wind patterns. At the very least, sea-ice coverage and ocean forcings are different (see Massom et al., 2018). As I am not a glaciology expert, I cannot say for certain what the differences are structurally between these ice shelves, but it probably is wise to cite some papers regarding ice dynamics to verify this statement.
[Author Response] - **We agree and have added ice shelf dynamics studies in the introduction. See above comments.**

Line 270: I feel like you cannot conclude foehn-related surface melt triggered the Larsen A collapse without taking into consideration factors like basal melting.
[Author Response] - **We agree which is why we have altered the narrative of the manuscript.**

Line 282-283: How are you certain that a combination of factors also did not trigger the final disintegration of the Larsen A? In fact, in Massom et al., 2018, it was observed that sea-ice loss allowed ocean swells to apply a strain along the ice-shelf front which is cited as a possible trigger of the Larsen A collapse. This needs to be considered and discussed in this manuscript.
[Author Response] - **We agree which is why we have altered the narrative of the manuscript indicating a supporting role of fohn winds for ice shelf collapse, and not trigger.**

Line 289-290: This sentence disregards the gradual retreat of the ice shelves like the major retreats the Larsen A experienced in 1987 and 1989 mentioned in Scambos et al., 2000.
[Author Response] - **We did research to see if we could triangulate other collapse events but could not find corroborating satellite images or in-person observations to clarify possible collapse mechanisms. .**

Line 292-293: You cannot come to this conclusion if your foehn detection algorithm only detects foehn when the temperature is above 0°C which will likely occur more often over the Larsen C according to future climate projections (Siegert et al., 2019) (Chyhareva et al., 2019).
[Author Response] - **We agree so we have altered our conclusions to included changes in the Southern Annular Mode (SAM) and climate change. Lines (351-361)**

References:

Bozkurt, D., Bromwich, D. H., Carrasco, J., Hines, K. M., Maureira, J. C., and Rondanelli, R.: Recent Near-surface Temperature Trends in the Antarctic Peninsula from Observed, Reanalysis and Regional Climate Model Data, Adv. Atmos. Sci., 37, 477–493, https://doi.org/10.1007/s00376-020-9183-x, 2020.

Carrasco, J. F., Bozkurt, D., and Cordero, R. R.: A review of the observed air temperature in the Antarctic Peninsula. Did the warming trend come back after the early 21st hiatus?, 100653, https://doi.org/10.1016/j.polar.2021.100653, 2021.

Chyhareva, A., Krakovska, S., and Pishniak, D.: Climate projections over the Antarctic Peninsula region to the end of the 21st century. Part 1: cold temperature indices, UAJ, 62–74, https://doi.org/10.33275/1727-7485.1(18).2019.131, 2019.

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.

Massom, R. A., Scambos, T. A., Bennetts, L. G., Reid, P., Squire, V. A., and Stammerjohn, S. E.: Antarctic ice shelf disintegration triggered by sea ice loss and ocean swell, Nature, 558, 383– 389, https://doi.org/10.1038/s41586-018-0212-1, 2018.

Turner, J., Lu, H., White, I., King, J. C., Phillips, T., Hosking, J. S., Bracegirdle, T. J., Marshall, G. J., Mulvaney, R., and Deb, P.: Absence of 21st century warming on Antarctic Peninsula consistent with natural variability, Nature, 535, 411–415, https://doi.org/10.1038/nature18645, 2016.

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.

Scambos, T. A., Hulbe, C., Fahnestock, M., and Bohlander, J.: The link between climate warming and break-up of ice shelves in the Antarctic Peninsula, J. Glaciol., 46, 516–530, https://doi.org/10.3189/172756500781833043, 2000.

Siegert, M., Atkinson, A., Banwell, A., Brandon, M., Convey, P., Davies, B., Downie, R., Edwards, T., Hubbard, B., Marshall, G., Rogelj, J., Rumble, J., Stroeve, J., and Vaughan, D.: The Antarctic Peninsula Under a 1.5°C Global Warming Scenario, Front. Environ. Sci., 7, 102, https://doi.org/10.3389/fenvs.2019.00102, 2019.

van den Broeke, M.: Strong surface melting preceded collapse of Antarctic Peninsula ice shelf, Geophys. Res. Lett., 32, L12815, https://doi.org/10.1029/2005GL023247, 2005.