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 plan to remedy the lack of background by changing the manuscript, specifically the introduction section to provide a clear overview of the current research to date on föhn winds and föhn-induced melt in the region.

Additionally, with your valuable comments and suggestions and after re-reading many previous studies we have decided to alter the story that we are telling. After 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 will alter 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 will also change 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, tt 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 powerpoint 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 will be altering 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 will change 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.**

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 plan to change the abstract to better reflect the new direction of the paper and will alter this sentence to be clearer.**

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 will be altered to address your comments.**

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 plan to completely alter the introduction to better fit the new direction of the manuscript so this will likely be changed.**

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 and will be altered with the addition**

of adding reference to previous work.

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 will be 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 agree and will alter this sentence.**

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 plan for it to say something like: " 2) What role did fohn winds and associated melt play for the trigger for the collapse of the LAIS and LBIS?"**

Line 87: What height is the air temperature measured at?

[Author Response] - **10 Meters, which we will add 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, Turton et al., 2018). We will make sure to discuss this study in this manuscript as well as provide the summary statistics from that study 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 will clarify which variables were used in our t-test.**

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] - **Yes, We agree and will include a section in the introduction that discusses how fohn winds form as well as previous research. Again this was tied to the original short for journal.**

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 and will alter the figure numbers.**

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 will add in melt frequency stats.

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 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 will clarify these sentences with less conflicting words.**

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 will make sure to 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 so we will**

alter this sentence to better explain how our research fits in the context of other research.

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 will also 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.**

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, and will clarify this in the manuscript**

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 but would be a great future direction of study. We will make sure this point is discussed in the discussion section and highlighted in the conclusions.**

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 will add melt frequency statistics.**

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 will add more references with our research. As stated above we will be adjusting the direction of the paper.**

Line 254: One thing missing about this discussion on the timing of the ice shelf collapses is if ice shelves have existed for thousands of years and foehn winds are a quasi-permeant feature on the Larsen ice shelves, why did foehn winds only trigger the Larsen A collapse relatively recently?
[Author Response] - **Great point! We will add this to the discussion section.**

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 are altering the direction of the paper indicating a supporting role of fohn winds for ice shelf collapse, and not trigger.**

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 are altering the direction of the paper 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 will look at satellite observations for these events to see if we can pinpoint the lime of collapse to assess if winds helped with these events as well.**

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 will look more into these conclusions to better assess possible future research directions, but will likely take out this assertion.**

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.