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 will make changes to the manuscript that are more in line with The Cryosphere structure.

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. As with the comment above 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 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. 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 will change 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 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 change 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.**

Specific comments

Line 11: 'Add 'grounded' before 'glaciers'.
[Author Response] - **This will be changed to clarify grounded glaciers.**

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 will add in lake drainage via hydrofracture to the abstract. It was already discussed in the manuscript but will be helpful for clarification to add it into the abstract.**

13/14: See 'general comment' above.
[Author Response] - **We will add in lake drainage via hydrofracture to the abstract. It was already**

discussed in the manuscript but will be helpful for clarification to add it into the abstract.

16: Mention the paper's specific focus on Antarctic Peninsula shelves.
[Author Response] - **We will clarify the region of study.**

18: 'less' vulnerable compared to what?
[Author Response] - **We agree this is not a useful comparison so we will clarify our remarks and compare collapsed ice shelves and extant ice shelves.**

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 so we will take out this word and replace it for clarification.**

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 will completely re-write the introduction to include valuable background and references as well as frame our findings in the context of other studies.**

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 will make this more clear in the updated manuscript.**

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 will make sure to discuss this study in this manuscript as well as provide the summary statistics from that study in the supplement.**

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 will do a more thorough background summary in the introduction and fix these contradictions. We will 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 will make sure to fix 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 will add this reference and appreciate the suggestion.**

48: Robel et al (2019) is incorrect. It should be Robel and Banwell (2019).
[Author Response] - **We will fix this reference error.**

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] - **We will add references to the beginning of this sentence as well as clarify and change the second part of the sentence. The change in the introduction to include more background on foehn winds and ice shelf dynamics will likely make this sentence change completely.**

52/53: Poor English. Reword.
[Author Response] - **We will clarify this sentence.**

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. This sentence will be changed to reflect this change and clarify our meaning.**

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] - **We will change this sentence to make it clearer that this identification method was developed previously in Laffin et al., 2021.**

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 will make sure to discuss this study in this manuscript as well as provide the summary statistics from that study in the supplement.**

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 will change the sentence to say ice shelf areas, as well as name those areas in reference to Figure 1.**

113: you have already defined AWS elsewhere.
[Author Response] - **Noted, we will adjust 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 will provide an in depth definition and thorough reference background in 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 will have some fohn influence. We will expand more on this in the manuscript and include other research articles as well.**

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 will make it more clear with specific percentages from our findings to compliment the figure.**

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] - **For this sentence we meant to convey the difference in fohn melt from ice shelf to ice shelf and under the influence of fohn jets on single ice shelves. We will clarify this sentence to reflect our sentiment.**

131: Figure 5 is mentioned before figures 3 and 4 have been mentioned.
[Author Response] - **We will be re-working the manuscript and will alter the mention of Figure 5 to after the other figures.**

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 will be removed 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 will reference.**

168: Banwell et al (2013) did not study Larsen A.
[Author Response] - **We will correct 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 and will be adjusted.**

229/230: Again, discuss this statement in the context of the findings of other studies.
[Author Response] - **As we mentioned above, we plan to alter the story of the manuscript to better include this work among previous research.**

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 will make sure to fix this embarrassing oversight.**

274: Satellite-derived depths of lakes are in Banwell et al (2014).
[Author Response] - **We will make sure to fix 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 will alter this sentence.**

293: '*more stable'* than what? This is vague.
[Author Response] - **We agree this is vague. We meant to compare ice shelves that have collapsed and extant ice shelves. We will clarify this in the manuscript.**

294: 'than previously thought' – by who? Give references.
[Author Response] - **We agree this is vague. We meant to compare ice shelves that have collapsed and extant ice shelves. We will clarify this in the manuscript.**

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 will make this more clear in the figure captions.**

Figure 3: Again, where is the data shown in this figure derived from?
[Author Response] - **Yes, the data shown is from RACMO2, we will make this more clear in the figure captions.**

Figure 4: Again, please state the source of the data.
[Author Response] - **Yes, the data shown is from RACMO2, we will make this more clear in the figure captions.**

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 will make this more clear in the figure captions.**

Figure 6: Again, state data source. And for a), should this be 'total melt'?
[Author Response] - **Yes, the data shown is from RACMO2, we will make this more clear in the figure captions.**

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