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.

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.

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.

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.

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).

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

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.

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

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.

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

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.

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

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.

Line 108: Perhaps explain which variables you used to make the two-tailed t-test statistic. "Mean of both ice shelves" is vague

Line 115-119: This seems like background information on the physics of foehn winds that would be better suited in the introduction section.

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.

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.

Line 137: If the SCAR inlet is not impacted by a foehn jet, where is the foehn wind influence coming from?

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.

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?

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.

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.

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.

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.

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.

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.

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?

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.

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.

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.

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).

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