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

General comments

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

On many occasions of the paper a ‘critical stability depth’ of lakes is now referred too, e.g. in the Abstract (line 16) and in the introduction (line 46). The authors also often state that critical stability lake depth is 1 m (though on one occasion they state that this depth is 3.5 m). The papers by Glasser and Scambos (2008) and Banwell et al. (2013, 2014) are often referred too after these statements, but none of those papers actually talk about a critical stability depth for hydrofracture. What Banwell et al (2014) *do state* is that the average depth of lakes on Larsen B in the Landsat image prior to ice shelf break up is 1 m, however they do not suggest that is a depth threshold for break up. Glasser and Scambos (2008) do not mention any specific lake depth threshold for breakup, and nor do Banwell et al (2013; in fact, in that study, they model lakes under the assumption they are all 5 m deep). As it is the volume of water in a lake that determines the ‘load’ actually on the ice shelf surface, not the water depth, I do not think that the authors this paper under review should talk about critical depth threshold, especially as the 1 m ‘threshold’ has not been suggested to be a threshold in the literature previously. Some sort of lake volume threshold may exist, perhaps combined with a lake density threshold, and those may help to determine when an ice shelf is primed for rapid break up via chain reaction lake drainage, but I am not aware of a paper that has specifically studied this.

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

Related to the above comment, the authors often mention ‘extant ice shelves’ (lines 19 and again on line 20 in the abstract), and ‘remaining ice shelves’ (e.g. line 354) on multiple occasions through the revised manuscript. Such statements are vague, and could be referring to *all* remaining Antarctic ice shelves, or just those remaining on the AP, or just those in Eastern AP. In fact, in such statements, I believe the authors are just referring to the Larsen C and Scar Inlet. So they should either state these two ice shelf names each time they are discussed, or at least, state ‘East AP ice shelves’ on these occasions. Related to this point, I also wonder if the paper title should include ‘east’ or ‘eastern’, given it is only the eastern AP ice shelves that are studied in this paper.

Finally, I don't know of any evidence (observational nor modelling) that suggests Larsen A experienced rapid chain reaction style break up, i.e. like Larsen B did (Banwell et al 2013). So the authors need to remove all references to this process having happened on Larsen A.

Line by line comments

8 – 11: Unlike Larsen B,s I do not know of any evidence that suggests that Larsen A also experience cascading hydrofracture events. Yes there may have been, but there is no evidence, so this sentence needs rewording.

10: I believe this should be 'long' period ocean swell, rather than 'large' period. E.g. see Massom et al (2018). Check throughout paper.

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

17 – 19: Be specific with what 'extant ice shelves' you are referring too.

19 – 21: As I state above, I think this sentence may be too speculative to include in the abstract.

23: Be more specific; which 'ice shelves' disintegrated? Also, it seems odd to start the paper with this conclusive statement (lines 23 – 24) saying that disintegrations were caused by regional warming trends, if you then go on to argue in this paper that föhn winds also played a role! Perhaps this sentence should be removed from this location and stated/discussed elsewhere?

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

41 – 42: 'In addition Massom et al., (2018) concluded that a lack of summer sea ice allowed large period ocean swells to reach the ice shelf calving front.' - It's unclear why this sentence is mentioned separately as surely it is part of (2) in the previous sentence. But in any case, this whole section from lines 38 – 44 seems very repetitive given basically the same detail in is included in the previous paragraph (lines 30 – 37). So I suggest delete much of this section.

46/47: Again, as mentioned above, I think the authors should reconsider their suggestion that 1 m is a critical depth threshold for rapid ice shelf break up.

48/48: This definition of hydrofracture is useful, but ideally it should come when 'hydrofracture' is first mentioned, which is currently on line 32 (I think).

51: Suggest replacing ''at critical water depths' with 'that rapidly drain by hydrofracture'.

67: I am unclear why just ‘late season fohn melt reduces firn pore space’. Surely this process has the same effect on the fir at any time during the melt season?

78: Replace ‘does’ with ‘did’.

83: Further to my general comment above, I Suggest replacing ‘each ice shelf’ with ‘each eastern AP ice shelf’. (Assuming you are referring to all eastern AP shelves here, and not just LAIS and LBIS? Maybe state the specific shelves in brackets?)

86: Again, please clarify what ‘extant ice shelves’ are being referred too.

126/127: In the second part of the sentence: ‘Therefore, we consider RACMO2 simulated estimates of surface melt caused by föhn winds to be conservative and likely higher in regions where föhn winds are funneled and concentrated’, it sounds as though it is being suggested that modelled fohn winds will be higher than in reality. But based on the fact you also say modeled estimates are conservative, I think you may mean to say the opposite?

144: Is the ML algorithm being referred to in this sentence fohnDA? State that if so.

175 – 188: Another reason for Scar inlet’s stability may be that it has had lots of sea ice buttressed up again it until very recently (when it broke up); you could also mention this.

194 - 196: I suggest putting ‘e.g.’ in front of Massom et al (2018) seeing as this study did not focus on the LAIS. And as I state above, I do not think any study has suggested that chain reaction lake drainage (aka hydrofracture cascades) contributed to Larsen A’s collapse.

218: As I also mentioned in my last review, Banwell et al (2014) should be referenced after the following sentence: ‘We find mean melt lake depth to be between 1.38-6.86 meters depending on lake location and föhn influence, which exceeds the average lake depth of the LBIS lakes prior to collapse (1 meter)’.

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

238: State what ice shelves you are referring too by ‘all major ice shelves’.

252/253: Again, very vague; clarify which ice shelves are being talked about.

253 – 255: I suggest moving the description of the firn densification process to the introduction.

252 – 261: Somewhere it would be useful to mention that there is some evidence of firn densification, surface ponding, and expansive ice layers on Larsen C. See Hubbard et al (2016).

290/291: Similar to my earlier comment, I am unsure why just late season melt is being talked about here.

303: Which ice shelf is being referred to here?

301: Comment on why this additional snow is relevant.

314: I think you mean 'basal <u>melting</u>'

317: Again, I don't know why the authors talk about a 'critical melt lake depth of stability' (it is not in the Banwell et al 2013 reference given here).

329: Which 'extant ice shelves' are being referred too? (FYI: Some southwest AP ice shelves, specifically George VI and Wilkins have had lots of melt and surface ponding in recent years. E.g. see Banwell et al 2021).

341: Again, remove mention of critical 1 m lake depth.

358: Clarify that AP ice shelves are beginning referred to here.

360: what 'region'? And surely fohn winds should be mentioned in this sentence too? What about future fohn winds?

<u>Figures</u>

Figure 1: Clarify that the LAIS and LBIS in the figure are no longer present, and perhaps give their collapse dates in the first sentence of the caption.

Figure 3: In the caption, I think 'graph' would be a more appropriate word than 'curve'.

Figure 6: What time period are the data for?

<u>Additional references</u>

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

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