Interactive comment on "Brief communication: 4 Mm3 collapse of a cirque glacier in the Central Andes of Argentina" by Daniel Falaschi et al.

This submission documents the catastrophic collapse of a glacier tongue in the Central Andes. The volume can be considered small compared to the well known 2016 twin collapse of Tibetan glaciers (around an order of magnitude smaller) but large compared to ice avalanches typically encountered in Alpine terrain. The described event dates back to 2007 and occurred in a remote area, consequently little evidence is available and the analysis evolves around satellite-derived digital elevation models and images.
The study cannot provide much physical insights into the processes leading up to the collapse, but this should not be expected given the relatively sparse catalog of observations. On the other hand, the 2016 glacier collapses in Tibet vividly illustrated that catastrophic runaway surges of low-angle glacier tongues can occur and may be related to climate, a matter that previously had been largely overlooked by the glaciological community. So I agree with the authors that this short note contribution is of interest to the glaciological community and well suited the Cryosphere journal.
To my mind the manuscript requires a considerable amount of modifications, however. In particular, several figures are poorly presented, annotated and referred to in the text, which obscures some important information on the collapse event. Some of my points of criticism may be misunderstandings on my side but I nevertheless urge the authors to consider and clarify them and make the necessary adjustments to convey their message in this short note. Below I detail these points and provide further minor questions and comments. Fabian Walter.
We thank Fabien Walter for the critical and constructive comments to improve the paper and have modified and corrected the manuscript accordingly. Also the misunderstandings served to clarify concepts in the manuscript (e.g. the glacier and ice avalanche deposit separation) and were useful to prepare better figures. The figures have also been re-arranged and further improved. Because parts of the manuscript have been rewritten, some of the comments are now obsolete (e.g. specific comment in line 229).

MAJOR COMMENTS FIGURES I have to admit I was puzzled when looking at the details of Figures 1-3. It may sound picky, but I was confused because I did not understand the authors' conception of the glacier outlines. Usually I think of avalanche debris as not being part of the glacier, i.e. a calving event (dry or wet calving) causes the glacier to retreat. If I understand the outlines in Figure 1 correctly, then the authors consider the avalanche debris as lying within the glacier extent. Whatever the case is, this should be clarified and my personal suggestion is to define the glacier outlines by the ice that has NOT detached from the glacier in the form of avalanches.
This is actually fully correct but would be unnoticed in a glacier inventory when not checking back the time series of high resolution images. In a 10 to 30 m satellite image (Landsat, ASTER, Sentinel 2) the collapsed (clean ice) part would very likely be mapped as a part of the glacier and the entire feature classified as a valley glacier. As regenerated glacier parts are typically included in glacier inventories and the discussion if this makes sense or not can be endless, the inclusion of the collapsed part is maybe not that wrong. However, as we are presenting a collapse event here, the collapsed part has been marked separately in the revised figures.

Moreover, in Figure 1, there are several grades of red lines, which are difficult to distinguish on the image. It would be better to use different colors or line symbols. The figure would also benefit greatly from two panels, one showing the glacier before and one showing the glacier after the collapse. Within the figure I would also label the avalanche debris as well as the LIA moraine, which is discussed in the text. Once this is clarified, it will be easier to understand Figure 2.

Here I was wondering for a long time why the Leñas Glacier tongue had thickened. Is this a result of surging behavior? Then I noticed that what I thought was the tongue was actually the avalanche debris. It would help to see the extent of the glacier tongue before the break-off in addition to the shown scarp head (note also that some of the text in this figure is likely too small). Similarly, in the different panels of Figure 3 I suggest drawing the glacier outlines. Finally, the photographs in Figure 3 are not self explanatory but do seem to contain important information. I suggest annotating the photographs extensively (e.g. glacier terminus, LIA moraine, avalanche debris, outwash planes, etc.).

We have prepared a new set of figures paying attention to the referee's comments and suggestions. Figure 1 contains three panels, showing the Leñas glacier before and after collapse, and the DEM differencing-elevation change map. 100m contour lines have been included. We have followed the referee's suggestion and clearly separated the glacier extent and the ice avalanche deposit. This is valid not only for the figure but for the area calculations in the main manuscript as well (the avalanched area is only 0.63 km^2 instead of 0.7 km^2 now). The glacier outlines for the years 1955, 1970, 2011 and 2018, as requested, have been removed from figure 1 to avoid confusion and transferred to figure 2, which effectively follows the glacier and avalanche deposit's evolution through time. Also, the LIA moraines can now to be seen more clearly in the figure.

We have eliminated the figure showing Tinguiririca glacier, as we have taken the decision to strictly stick to the event described in the introduction (see response to the Tinguiririca glacier comment below).

Figure 2 includes the 1955, 1970, 2011 and 2018 glacier and avalanche debris outlines, and has been annotated with (LIA) moraines, rock glaciers, glacier forefield, crevasses, avalanche scarp. We note that some of the avalanche features (thermoklarst ponds, hummocks, etc.) are too small to be annotated in this figure and have been marked in figure 3a instead.

TINGUIRIRICA GLACIER Compared to Leñas glacier, Tinguiririca Glacier receives less attention in the text. It is only illustrated in two panels of Figure 2. The reader needs a map view equivalent to Figure 1 to get a feel for the glacier extent and geometry (for both glaciers it would be helpful to see a few contour lines which helps identifying steep parts and planes) and more explanations, otherwise it seems that Tinguiririca Glacier was half-heartedly added to the study.

We agree that this extra example is difficult to integrate in the research context without providing further details. The available remote sensing data (high resolution images, and DEMs) for Tinguiririca was even scarcer than for the Leñas glacier, which did not allow for a fuller and more comprehensive description. We have taken the decision, based also on another referee's judgment, to discuss and compare the Tinguiririca event only very briefly in *terms of the avalanche volume and runout distance. This means that we have also eliminated the figures showing the Tinguiririca glacier.*

SURGE HISTORY The topic of glacier surging receives little attention in the manuscript. Do the satellite DEM's provide some hints for surge behavior? In any case, it would be good to write 1-2 sentences on this subject to put the collapse into context of the Aru Co and Kolka events. This could be built into the second paragraph of the Discussion section. Currently, there is some mentioning of a thermal regime change, but no specific evidence or context is provided.
We thank the referee for the suggestion. We have added a few sentences about surges in the region. In a recent review of glacier surges in the Central Andes, Falaschi et al (2018, Progress in Physical Geography) found evidences of glacier surges at nearby glaciers, but not for the Leñas glacier specifically. We re-examined the material from Falaschi et al. and our own dataset and concluded that no evidence of a Leñas surge could be identified.
For clarification, the text referred to the overall thermal regime beneath the glacier but not specifically to the possibility of a thermally triggered Svalbard-type surge.

MINOR COMMENTS Figure references: At several parts of the manuscript it is not clear what the autor's assertions are based on. For example, in the first paragraph of Section 3 no references to figures are made, but if I understand correctly the described observations are based on information shown in the figures.
We agree that the figures could be better referenced in the main text and have inserted further links to them.

Line 68: It would help to give a rough estimate of avalanche volumes for the 16 events of the WGMS.
This is a good idea! Now included in the text.

Line 134: "(e.g. due to decrease in glacier slope)" is unclear.
We meant that the new debris cover might have developed in a now flatter glacier. We have clarified this in the text.

Line 140-141: Do detachment scarp and crevasses really disappear or were they simply covered by debris?
The scarp and crevasses were swept away in the avalanche, as they formed its head. No evidence of them being filled with debris was observed in the field.

Near Line 150: How was the absence of bedrock beneath the glacier confirmed? Using boreholes? Could exposed bedrock be concealed by deposited sediments? Please mark/annotate figures accordingly.
The absence of a hard bed beneath the glacier was visually evaluated in situ. No rock outcrops were to be found in the failed area whatsoever. The area is steep and subjected to rockfall, hence boreholes were not considered. From the thick sediment layer in the failed glacier area (visible in incised gullies seen in figures 3a and b) we believe the hard bed lies well beneath the glacier bed. We now mention this characteristic in the caption of Figure 3.

Line 152: Reference to Figure 4a is unclear. Please mark/annotate figure accordingly.

See previous comment. This has now been clarified (annotated).

Line 156-157: The smoother hammocks and thermocarst should be highlighted in the respective figures.
We have marked thermokarst ponds and hummocks in Figure 3a.

Section 4: It would help to show parts of the meteorological analysis in a plot. Also, some specifics on the acceleration criteria would be of interest.
We have chosen not to include a graph showing the meteorological data as this does not reveal a strong link with the collapse event. With 3 multipanel figures, we are already in the maximum length advised for a brief communication in TC journal.

Line 183 (and elsewhere): It would help the non-expert to specify what is mean by "ordinary" ice avalanches.
We agree and have changed this to "from steep fronts and hanging glaciers steeper than 30º.

Discussion: It may be worth considering the possibility that the event happened as a series of small break-offs rather than a single rupture. Such cases are known to exist and it is not clear which conditions favor one scenario over the other. https://www.geopraevent.ch/project/weissmies-glacier-velocities/?lang=en
Indeed, we can not be 100% sure if the event was due to a single rupture or several smaller ones. However, the field evidence and the relatively large blocks of massive ice point in the direction of an avalanche composed of large blocks (see Fig. 3b). We mention the possibility of small events now in the revised manuscript. We assume that numerical modelling could help in identifying this, but this is beyond the scope of the current study.

Line 229 and following paragraph: When discussing the permafrost conditions it seems that the authors present arguments for and against permafrost. It was not clear to me what the actual conditions are believed to be. Also, it is not clear what the implication of the last sentence is (reference to Kolka).
Confirming the presence/absence of permafrost in the plateau (i.e away from the headwall) would need proper, in-situ temperature logging. Alternatively, an approximation of the thermal conditions could be investigated by building a potential incoming solar radiation model. Again, we consider that any of them would be beyond the scope of the current study, and would not help in elucidating a collapse trigger per se. Due to the lack of convincing evidence for permafrost presence/absence in the pleateau where the ice avalanche deposit sits up to this point, we have removed the discussion on the role of permafrost in the preservation of the ice avalanche deposit.

Line 244: Specify "same method".
Checked and corrected. By 'same method' we referred to SRTM and ALOS PRISM differencing.

Line 262: "time difference" → time lag.
Changed to time lag.