Interactive comment on "Brief communication: 4 Mm3 collapse of a cirque glacier in the Central Andes of Argentina" by Daniel Falaschi et al.
Anonymous Referee #1

General comments: This study deals with the collapse of an unnamed glacier in the central Andes of Argentina in March 2007 (between 5 March and 14 March 2007). This glacier, named by the authors, ' Lenas glacier ', is located in the very remote area. The collapse and the avalanche have not been observed directly and it seems to remain unnoticed during several years. Very few data are available about this collapse. Most of data come from satellite images. This study aims at reconstructing the conditions of the collapse (Volume, slope, meteorological conditions, sismic events) in order to understand the possible causes of this breaking off. The authors claim that this event, very rare, can be compared with the very large collapses of Kolka in 2002 and Aru glaciers in 2016, given that the volume size is huge and the slope of the glacier is low.
Unfortunately, the analysis and the conclusions remain qualitative and speculative due to the lack of data. This study contains vagueness, large assumptions, and lack of rigour for the following reasons:

We thank the referee for the in-depth review. In regards to his/her main comments, we agree that the data available is limited and probable causes of the event are difficult to assess In our view, the chosen format of a Brief Communication, is perfectly suited to present and report this Leñas update and to make the community aware of the event. We cannot expect to get an answer for every open question from image time-series analysis and geomorphometric interpretation of surface characteristics (in the field and using DEMs / satellite data) for an event discovered with many years delay. The kind of event described in our study is rare enough that knowledge about every single event is important.
Also, brief communications do not normally require lengthy accounts of theoretical background and methodology principles, and thus we had not extensively elaborated on i.e. DEM accuracies and limitations of the DEM difference method, which can be nevertheless found in the literature.

1) First, the uncertainty relative to the collapse volume can be questioned. The volume changes have been assessed from satellite images (Spot 5 12 February 2007, Landsat 5 March 2007, Spot 5 14 March 2007, Quickbird 19 April 2007). The failed glacier area and thickness have been estimated from images 12 February 2007 and 19 April 2007. The accuracy of each DEM is not mentioned. The authors wrote that ' the average thickness at the scarp was roughly 35 m as estimated from scarp shadows and solar angles at the time of acquisitions . Thus assuming a linear decrease of the glacier thickness from the scarp to the former glacier front . . .. . .a rough estimate of 4.5 106 m3 ' (l. 108-111) . No detailed information is given about the method and the uncertainty of thickness. In the following lines (l.112), the authors mentioned a ' conservative 15% error from uncertainty in detached area and thickness estimate' without providing any details about this uncertainty. Another ' independent ' estimate has been done from the difference between SRTM DEM (February 2000) and ALOS PRISM DEM AW3D obtained between 2007 and 2011. Again, the uncertainty of these DEM is not given. The uncertainty related to SRTM penetration is not mentioned. The authors mentioned only that the assumption of no radar penetration is confirmed by the comparison between C Band and X Band SRTM which show no significant difference.

The authors did not provide any detail or reference. In addition, the authors used the ' average DEMS' of ALOS PRISM DEM (2007-2011) with an ' average year' of 2009. They assume that there is no change between March 2007 and 2009 (l. 125-126). It is very confusing. The uncertainties relative to this assumption are not explained. Nothing is said about the elevation differences between these 2007-2011 DEMs. The uncertainty of 2.3 m (l. 128) on elevation difference seems to be very optimistic. Moreover, from Figure 2a, one can see surprising elevation changes of 25- 50 m in several areas of the upper part of the glacier between 2000 and 2009 far from the detached zone. These values are similar to the elevation changes of the collapse area. However, no explanation is given about that. Due to the lack of information, it seems very difficult to assess the uncertainty on the volume of the collapse.

We have now partially recalculated our volume estimates, and largely rewritten, extended and rearranged the related description. We use now SRTM, ALOS PRISM, and (new) the TanDEM-X DEMs as main source of our volume estimate, and use the scarp height only as rough check. Note that we don't need to estimate the DEM accuracies but only the accuracies of elevation differences to arrive at an accuracy estimate for the volume. A detailed assessment of the DEMs used is out of scope for our brief communication. The gross uncertainties seen in the figure are situated on the steep headwall of the glacier, whereas the detachment happened from the flatter part for which the elevation differences to the left and right of the detachment are more representative. Most importantly, we believe the exact number of the volume is not crucial as we are only interested in the ballpark of the volume, i.e. around 4 Mm3, which we hope to demonstrate sufficiently now.

2) The discussion about the mean slope' is confusing. The authors make a difference between hanging glaciers with steep slopes (>30◦) and glaciers with low slope (lines 26-27). They wrote that ' the detachment of large portions of low-angle glaciers is much less frequent ' (l. 34-35). The manuscript is confusing because the authors mentioned both the low angle of reach (5◦) (lines 22 and 106), the average slope of glacier (24.6◦) (line 97) and the slope of the detachment part to discuss the stability/instability of the glacier. These slopes are mentioned in different sections of the manuscript which creates confusion.

The low angle reach is not relevant to study the stability of hanging part of the glacier. More specifically, the ' suprisingly low angle of reach (5◦) ' (Abstract, l. 22) seems to be irrelevant as an indicator of stability of glacier. The slope of ' detached glacier', which seems to be the relevant value to assess the stability, is mentioned in Discussion only in line 186 (15.6◦). We do not have any information about the method used to calculate this slope. Is the surface slope before the collapse ? calculated on which distance ? is the surface slope after the collapse ? Which images have been used to obtain this value ? What is the accuracy of this calculated value ? The analysis of slope change reveals also a lack of rigour. In line 142, the authors wrote : Ân´ the glacier slope decreased from 24 .6 to 20.4◦ from before to after collapse Âz. The ˙ distance on which this slope is calculated is not specified. One can assume that the slope change is mainly due to the length changes of the glacier and the size of the avalanche. In this way, is the slope change a relevant information?

In order to clarify the different angles mentioned in the manuscript and avoid confusion, we have now differentiated only 2 of them.

-The angle of reach is measured from the scarp head to the avalanche terminus. While this is not specifically relevant to the stability of the glacier, it does tell about the fahrboshung mobility index and we have therefore included it.
-The angle of the detached part of the glacier before collapse (15.6°) was measured from the SRTM DEM over the failed glacier area measured from the Quickbird 2007 scene (mentioned in the text). SRTM has been tested in a large number of scientific studies in glacier areas and has been fully accepted to derive glacier topographic parameters (such as slope) with adequate results (e.g. Racoviteanu et al, 2009 in Annals of Glaciology, Frey and Paul, 2011 in International Journal of Applied Earth Observation and Geoinformation). Also, SRTM accuracies can be found in Farr et al. 2007 in reviews of Geophysics.
We agree that the overall glacier slope change is not relevant information and have removed the previous analysis. Incidentally, as opposed to the referee's understanding, there was no hanging part of the glacier before collapse).

3) As mentionned by the authors in Conclusions, due to the limited data, this study is not able to identify the causes of the Lenas event. Many assertions are highly speculative. For example ' the thin glacier front could have been frozen to the bed and a change in this polythermal regime may have caused changes in stability' (l. 202-204) or ' we suggest that the soft glacier bed material could have played an important role in the collapse. . ..' (l. 206-207), or ' we hypothesize a mixed origin for the debris layer observed on the ice avalanche deposit. . .' or ' . . .may indicate that a large glacier collapse has not happened in 2007 for the first time. This speculation relies on. . .. . .. '. The Discussion is a list of assumptions and questions and does not shed light on the causes of this collapse.
We agree that the discussion section includes speculative statements that partly go beyond the empirical evidence. Although we think this is allowed when speculations are clearly marked as such (e.g. to stimulate further research) we have removed most of them and focused on statements with at least some evidence.

In summary, the authors claim that the Lenas collapse deviates from typical ice avalanches from steep glacier and can be compared to the rare low-angle glaciers collapses similar to Aru glaciers and Kolka glaciers avalanches.
We agree that a direct comparison of the event observed here to the collapses at Kolka and Aru should not be made and have revised the text accordingly. However, we think when talking about detachments of glacier sections with comparably low surface angles, and about glacier avalanches travelling large distances over comparably flat surfaces it is appropriate to at least mention the Kolka and Aru collapses.

Given the lack of information given in this study, the uncertainty on collapse volume can be questioned.
We actually think that the derived collapse volume is a comparably robust part of the study and has higher certainty than several other numbers. We however revised the volume estimate parts significantly (see response to above comment).

In addition, the data provided by this study are poor and do not allow to identify the possible causes of the collapse.

We agree that the data available are limited and possible causes of the event are difficult to derive from it. However, we also think that sufficient information is around to make the community aware of the event. This is why we have chosen the format of Brief Communication, which is to our best understanding among others meant for such types of updates.

This study points out the low detachment slope (15.6°) although the determination of the slope has not been explained and the uncertainty on this slope is unknown. 'No significant change in glacier geometry could be identified due to the lack of data' as mentioned in Conclusions (l. 267-268).

We have now added how the slope has been calculated and revised the text sections about the angles involved (see response to above comment).

The authors suggest that soft bed characteristics play a crucial role on the collapse trigger. I do not think that this study provides sufficient quantitative information for understanding complex processes in glacier instabilities and collapses.

We agree that there is very limited evidence for this speculation and briefly mention the idea in the discussion section.

I do not think this study shed new light on the triggers and factors responsible for this event. Given the paucity of data, I am not sure that this event can be compared to Aru glaciers and Kolka glaciers avalanches as claimed by the authors.

We agree that this direct comparison is based on very limited evidence and have rewritten this section (see above).