This manuscript is a case study of a two-part failure of a portion of a hanging alpine glacier. Surface displacement measurements at several points on the glacier were recorded up to the time of failure. The displacement time series at two stakes was fit to two different power law relations, one with a superimposed log-periodic signal. The regression parameters from fitting these data are used to forecast the time of failure. The retrospective analysis indicates that the break off event could be forecast about 10 days in advance.

The manuscript reads as more of an engineering case study using existing methods rather than original scientific research. While it is undoubtedly useful and promising that the prediction of such a break off can be made well enough in advance to support an early warning system, I fail to see what research question or scientific hypothesis was addressed in the paper. The methods applied were all developed and published previously (indeed, the manuscript relies heavily on previous publications, to the point of lacking key details in some places). The manuscript could be bolstered by a more rigorous and quantitative analysis of the applied methods, especially when it comes to reporting uncertainties in the time forecast or a sensitivity analysis around some of the subjective choices made in the framework. Furthermore, some discussion or analysis of the physical implications of this framework–especially regarding the claims of "universal" behaviour–might add sufficient originality to warrant publication in The Cryosphere.

The reviewer is right when stating the methods applied in this study were all developed, but for the first time, we could gather data up to the break-off. Thanks to this dataset we could definitely confirm the presence of logperiodic behavior prior to break-off, enabling to forecast 10 days in advance the occurrence of the catastrophic event. Moreover, this dataset gave us the opportunity to validate previous conjectures on the physical processes during the onset of the instability.

General comments

1. The crevasse in the hanging glacier opened in autumn 2008, after which a monitoring system was set up. The break-off events occurred in autumn 2014. Can your analysis shed any insight on this 6-year time lag between the crevasse opening and the break off? What time lags have been observed for other events? Certainly the crevasse could be seen as a requisite precursor event for this type of break off, so there may be some important physical insight to be gleaned by thinking about this timescale as well, or the conditions that lead to this type of crevasse opening in the first place.

There are only few observations and monitoring of cold glacier break-off event. Pralong and Funk 2006 have observed a time lag of 2.5 years between the crevasse opening and the break-off event on the same glacier, with approximately similar geometry (see line 86). They also stated that the formation of the upper crevasse is related to the increase of the bedrock slope and not to the destabilization process. Such a crevasse is therefore not necessarily observed on other cold ramp glaciers, which cling to a homogeneous face.

2. More detail on methods is needed in a number of places. In fact, for a “methods” paper, there should probably be a Methods section to organize this material for the reader. For example, on p. 4931, what does “interpolated on a regular time step” mean? What is the time step? How sensitive are your regression results (and thus your time forecast for the break off) to your choice of data smoothing? A sensitivity analysis involving some of these choices could help to quantify the uncertainty in parameter values and thus forecast time, and would add some originality to the paper.

We modified the structure of the paper and added a new section (now section 3: “Previous findings on cold glacier break-off”). See also comments made to Reviewer 1.

3. Some more discussion and detail on the physics of damage accumulation would be welcome, especially if this case study supports the idea of damage accumulation at/near the base of the glacier as being the culprit in these break off events. For example, on p. 4927, line 22, this statement needs clarification or a reference: is it always the case that the failure occurs within a few meters above the bedrock? What evidence do you have for this? Are you discussing previous observations or model results (in which case a reference is needed) or your own original observations from this study? You mention this again on p. 4932, lines 1-2. Other than visually seeing ice left over after the break off rather than bedrock, how do you know the scale is a “few” meters? How does this align with other observations? If this is indeed related to the fundamental physics of damage accumulation, then this is very important! It would be helpful to discuss this in a bit more detail.

All the observations (Weisshorn 1973, Weisshorn 2005, Grandes Jorasses 1998 and Grandes Jorasses 2014) suggest that the final failure is not located at the bedrock but “few” meters above. Pralong and Funk 2006 (with the help of the model developed in Pralong et al 2006) noted that the position of the fracture over the bed is lower in the model than in nature, and associated this difference with bedrock irregularities (which are not considered in the model). We have added a discussion on damage evolution process lines 146-157.

4. You claim that the log-periodic relation (Eq. 2) fits the data better than the simpler power law relation (Eq. 1), but of course this is no surprise given that there are more free parameters in the log-periodic relation. Therefore comparing these two regression fits is not sufficient to justify that the log-periodic relation is more appropriate. You need to invoke some other physical evidence or line of reasoning here, especially if you want to conclude at the end of the manuscript that there is some “universality” for log-periodic behaviour (see related comment below).

Universality might not be the right term as it refers to constant critical exponents. However, we showed that such oscillating pattern is also confirmed in this break-off (with large amplitudes close to failure) and is to be common to all the observed break-off events. The complete understanding of the onset of the instability was already assess with surface displacement and seismic measurements (Faillettaz et al. 2011). We summarize the results lines 146-157.

5. Where do the 50 cm/day and 100 cm/day thresholds come from? How and why did you choose these? You indicate that the time forecast is sensitive to the subjective choice of these parameters, so it seems that some more attention should be paid to why you chose these two values. Furthermore, why

then do you recommend in the Discussion that 40 cm/day be used as a "conservative threshold"? Where did this number come from? Is this a subjective recommendation? It seems rather arbitrary when it is presented in the text. How much more conservative would a prediction be using this threshold? It might be helpful to describe this in more detail, and indicate such a prediction in your figures, especially if you are using the results of this analysis to inform future early warning systems.

As explained in the text, we arbitrary choose such thresholds based on the previous observations. We showed that taking a velocity threshold significantly improve the forecast (Fig 8).

6. At the very end of the manuscript, you claim that there is some "universality" to the log periodic oscillatory behaviour for this type of event. This is a bold claim, but it is unsubstantiated in the manuscript. This would be a very intriguing and important result, but it would take more discussion, evidence, and placing the analyzed event in context with other events to support such a claim.

See point 5. and comments reviewer 3.

7. I am not sure how to interpret the results presented in Table 1. You show regression parameters for the surface displacement data fit to Eq. 2. The results indicate different predictions, and different parameter values, for different stakes. The parameter values also differ for the same stake but using data over different time intervals. Is there anything in these results to support your universality claim? For a predictive forecasting framework, it would be useful to have uncertainties associated with your time forecast. You report t_c to four digits of precision, but certainly your uncertainty is on the order of days, not minutes.

We have added uncertainties in the table (it was in Fig 8 bottom) and leave t_c to a daily precision.

Specific comments

• p. 4929, lines 4–9: this is a speculative ice break-off, as you mention, but it's a bit misleading to list it under the heading of glacier break off events that "have been observed and reported." As such, you really only have two previous confirmed break off events.

• p. 4929, lines 16–19: these sentences are redundant from the Introduction

OK

• All of Section 3.1 is Background material, not Results

We rearranged the structure of the paper, now this section is located before the results section.

• p. 4929, lines 4–9: this is a speculative ice break-off, as you mention, but it's a bit misleading to list it under the heading of glacier break off events that "have been observed and reported." As such, you really only have two previous confirmed break off events.

The reviewer is right but it is worth to mention this event as it reached the valley floor.

• p. 4929, lines 16–19: these sentences are redundant from the Introduction

OK we skip these sentences

• All of Section 3.1 is Background material, not Results

OK, see new structure.

• p. 4933, line 5: this is confusing, are you referring to Fig. 3 in the manuscript? Because Fig. 3 shows smoothed data, which implies post-processing. Or are you referring to Fig. 3 in a different manuscript related to the Whymper break off?

This was confusing. We removed it.

• p. 4933, line 27: how is this 40 cm amplitude apparent from Fig. 5? Fig. 5 shows residuals from a regression, not oscillation amplitudes.

This is an oscillation superimposed on the general power law acceleration (see Eq 2), therefore 40cm refers to the amplitude of the oscillation in this figure.

• Much of Section 4.1 seems out of place as a Discussion section. It contains both background material (lines 5 to about 17) and results, with a bit of discussion. A bit more background material (and maybe Methods description) is needed to frame the results presented here.

See new structure

• p. 4935, lines 16–17: how much "more accurate" is the prediction when made using a threshold velocity rather than infinite displacement? It would be worth stating this in the text to justify this statement.

This was exactly what was Fig. 7 (now Fig. 8) about…

• p. 4935, lines 24–26: which regression is this based on? Eq. 1 or 2? You should probably explicitly state this here.

OK, Eq 2

• p. 4937, line 21: remove the term "significantly" since you did not perform a statistical analysis to show that using a threshold velocity produced a better forecast than an infinite displacement criterion (though such a statistical test could be done in this case)

No, using velocity threshold give the exact timing whereas infinite displacement overestimate the timing by more than 10 days (Fig. 8).

• p. 4937, line 24: similar comment, "highly probable" implies a probability, but you did not calculate probabilities. A subjective term such as "likely" would be more appropriate here.

OK

• Figure 2: I'm confused by the gap in the data in late September. In the text you make it sound like measurements were taken during this period despite a stretch of bad weather. Also, the lines for Stakes 2, 13, and 14 are difficult to distinguish (especially for Stake 14). Can you differentiate the curves with symbols, or different line types?

The surface displacements are similar and therefore not easy to be distinguished. See new Figure 3.

• Figures 4 and 5: These figures are a bit confusing. You're showing residuals to the regression fit, through time, which are plotted as points. A residual makes sense as a discrete measure of the distance, in model space, between an observation and the model. What, then, are the solid lines? I don't see physically what would be represented by a continuous function in residual space. You label these curves as the power law and log periodic "fit" in Figure 4, but these equations define surface displacements, so how are you plotting them in residual space? In Figure 5 you label the red curve as the residuals, so the description is inconsistent as well.

See new figure 5 and 6.