

Review of revised version of “Time forecast of a break-off event from a hanging glacier” by Faillettaz et al.

The organization and readability of the manuscript have been improved. The authors have addressed some of the concerns and suggestions posed by each of the three reviewers. Many of the suggestions provided in the initial stage of review would have represented major changes to the manuscript. However, I would characterize the changes made by the authors as relatively minor. I view this as a missed opportunity for making a more substantial contribution to the literature on geohazard prediction, however I believe that with some additional minor changes the manuscript might be acceptable for publication in the Cryosphere.

1. The physical insight regarding the source of the log-periodic oscillations, specifically the initiation, interaction, and evolution of microcracks near the base of the glacier, is based partly on model results and partly (perhaps) on observations (e.g. Pralong and Funk, 2006). I think the damage accumulation model is quite plausible physically. However, the description in the manuscript is somewhat misleading in several places, as it sounds as if the location of failure (“typically a few meters above the bedrock”, line 43; point 5 on lines 121-122) is observed. I made a similar point in my initial comments, and I’m not sure it was really addressed: did you gather any actual observations or have any other proof that this scale is “several meters” or otherwise not on the bedrock? What observations, and from where, indicate this length scale (if any)? Is this based entirely on the damage model of Pralong and Funk (2006)? (the authors of this reference clearly state that observations of the fracture process at the base of the glacier are not possible). The extra description of the physical failure mechanism that the authors added (lines 114-125) is welcome, although this again seems to mix observations of the Whymper and Weisshorn events with model insights. I do not think that the authors are being intentionally misleading, but it should be made more clear which processes are based on observed (or observable) phenomena and which are based on insights gleaned from physical models.
2. The 0.4 m/day velocity threshold suggested as a conservative estimate still seems arbitrary, since 0.5 m/day and 1.0 m/day thresholds were used in the analysis (and 0.5 m/day seemed to be quite good for predicting the break off). Why suggest a threshold for future forecasts if there is no insight in the manuscript that can be gleaned from the predictive skill of this threshold?
3. The term “significantly” still needs to be removed in several places (line 193, line 198, line 320). Using this term to describe differences or improvements implies a statistical analysis that quantitatively demonstrated a statistically significant change. Since you did not perform such analyses, the use of this terminology is misleading.
4. The difference in fits between the power-law and log-periodic relations is quite negligible (Figs 5 and 6), in terms of the residuals. The differences are hardly greater than the measurement accuracy, and it is doubtful if a statistical analysis would show a statistically significant difference. I think you can make a plausible argument, on the basis of the damage accumulation modeling, that the log-periodic relation is more appropriate physically. However, I don’t think that plotting the residuals makes much of a case here.
5. Why did all of the results in Table 1 change? What have you done differently here? It’s not clear from reading your responses to the reviews. Most of the changes are relatively small, but some of the parameter values changed quite a lot.
6. It’s not clear from Figure 7 that the large low-frequency peaks in Lomb power for Stakes 13 and 2 ($f_{\text{Lomb}} = 0.5$) are after the first break off. These are clearly the strongest peaks, and which draw the reader’s attention. In the text (lines 231-233) it reads as if you have focused on the smaller peaks at $f_{\text{Lomb}} = 2$ d according to what you already expected (since these are the peaks you describe first, which seems odd if the reader looks at the figure and first sees the obvious and much bigger peaks for

f_Lomb = 0.5). If you can indicate dates somehow on the figure, or describe the bigger peaks first, then it might seem less like you are searching for an answer that you already know.

7. Figure 8 (former Figure 7) is still quite confusing. There must be a better way of presenting these results, as I think this is a very important part of describing the real-time descriptive skill of the different methods and choices for the velocity threshold. There's a lot of good information in the figure, but I'm still struggling to understand it. The casual reader will invest much less time in figuring it out than I have.