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Response to referee #2

We thank referee #2 for its careful reading and for its positive comments on our work. We have addressed all points raised by the referee as detailed below.

> The measurements show an interesting relation between boulder size and height of the supporting ice column. I suppose that the spread in the data is at least partly due to the fact that the height H will grow with time, and its maximum represents the height when the boulder topples; if that is the case, it is worth pointing out, presumably in the caption to figure 3.

The data of fig. 3 shows the height of 80 structures at one point in time when the rocks did not appear to have already fallen from their ice foot, even for the highest structures. This was made clearer in the caption of the figure. To our interpretation, the spread of the data is likely due to the influence of the aspect ratio of the rock on the formation dynamic.

> Obviously a one-dimensional theory misses the lateral melting of the ice column and thus the cause of rock collapse, and it might be interesting in future work to add a lateral melt component (which can be done in the same fashion as here)

Indeed we concentrate our study on the vertical melt of the ice under the rock and we did not consider the lateral melt of the ice foot which ultimately controls the maximum height reached by the glacier table. This was made clearer in the model description and a qualitative discussion on the effect of the lateral melt of the ice column was added at the end of the revised version of the manuscrit.

> Although the model is validated with reference to the four principal rocks, the statement at 199 that there is 'excellent agreement' of the theory with the measurements for the sample of 80 tables is disingenuous: it is obviously not. What can be said is that the agreement is good for small ice columns, but it is quite inadequate for large ice columns and for holes ($H < 0$). Indeed one of the features of figure 3 is that the data follows a fairly well-defined curve (with spread perhaps due to the comment above (?)) where on the face of it H asymptotes to ~ -0.5 m for small d or h (use h not e), and appears to become infinite (well, large) for $h \sim 1$ m or $d \sim 5$ m. It seems to me that this latter behaviour is associated with a lack of lateral melt of the column (and thus H can become very large as toppling will not occur). For a large boulder, radiative melting will disappear due to complete shading. Using the numbers in the paper, I find the short wave radiation to be ~ 220 W m^{-2} and the effective (LW and turbulent) heat flux to range from 35–126 W m^{-2} , depending on wind speed.
> Under a large boulder, only the turbulent heat transfer will provide much melting, and I suppose the column narrows more slowly, allowing growth to greater heights. Similarly for holes, for example the left most data point in figure 3a, a 1 cm pebble in a 5 cm hole will be effectively shielded both from wind and incident radiation. So in my view the correct statement is to highlight the agreement at small H, but also highlight the disagreement at large or negative H, and then comment accordingly, perhaps as above.

We agree that claiming that the agreement was excellent was too strong of a statement. Indeed the agreement and discrepancy of these data were not commented on in a clear manner. As you mention, our model is not expected to stay valid for holes (H<0) and does not take into account lateral melting which controls the maximum height reached by the ice columns. In the revised version of the manuscript we have commented in the result section (4.3) more clearly the two regimes (H<0 and H>0) and where our model is expected to be valid. We have also mentioned in the conclusion the lateral melting of the ice foot that likely controls the maximum height reached by the structure.

We have taken into account the smaller points mentioned: we have rewritten the second half of the abstract, mentioned tafoni and ice sails in the introduction and used h to denote the rock thickness instead of e.