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Interactive comment on "Automated detection of ice cliffs within supraglacial debris cover" *by* Sam Herreid and Francesca Pellicciotti

Anonymous Referee #2

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This manuscript tries to facilitate a more robust and operator-independent mapping of ice cliff extent on debris covered glaciers. The method is based on an investigation and classification of local surface slopes, using raster elevation maps (e.g. satellite derived DEMs). In order to assess the usefulness of such an approach, it might be worthwhile to discuss the reasons, why ice cliffs should be mapped at all. Ice cliffs are steep, smooth sections of glacier surface with no, or only a minor coverage of supra-glacial debris, embedded in glacier area with a considerably thicker debris cover. Due to this difference in debris thickness and the fact that the thin debris cover on the ice cliffs usually is below the critical thickness of the Östrem curve, these areas tend to show a strongly enhanced melt rate compared to the surrounding glacier surface. Also, the aspect and slope of ice cliffs can be favorable for melt. The crucial parameter, which

relates potential ice melt to atmospheric parameters, in this context, is the surface temperature. In fact, it is not really necessary to call something an ice cliff, if high melt rates can be derived or parameterized from other information. Unfortunately, the map view area of ice cliffs is usually much less than the available pixel resolution of thermal remote sensing products. The availability of higher resolution DEM information might therefore be a good reason to try and identify such areas of high ice loss from geometric constraints. This manuscript demonstrates a novel approach to investigate the slope distribution across debris covered glacier surfaces and relates these results to the probability of ice cliff existence. In my view, this is a considerably advance towards automated ice cliff mapping, based on the availability of remote sensing products.

Even though the authors discuss the problems connected with ice cliff definition, the main problem I see is the missing link between the classification tool and the physical conditions for ice cliff generation. The basis of the method is a threshold for surface slope, above which a slope can be considered an ice cliff. Furthermore, the probability of ice cliff occurrence is computed for a series of thresholds, producing a Gaussian distribution function of ice cliff fraction versus slope threshold. The optimum choice of slope threshold is then found as the intersection of the maximum orthogonal distance between a hypothetical line P1-P2 and the distribution function. I cannot see any physical reason why this should be a "preferred" angle in the distribution function. On page 8, line 22 it is clearly stated that this is hypothesized, but there is no attempt further in the manuscript relate that to any physical characteristic. Maybe I missed the point, but I would encourage the authors to improve this relation between the distribution function and the conditions defined as requirement for ice cliff existence. In this context, it might also be worth to discuss the choice of a Gaussian distribution function, which relates to the reasons for surface undulations on the glacier surface. Basically, it can be assumed that strain, differential melt and the existence of surface melt water are responsible for the creation of surface undulations. If these effects are randomly distributed, a Gaussian distribution function is probably a good representation. This, however, can be questioned with regards to ice cliffs, because these features are usually connected with a discontinuity in surface slope. The reason for this is that the ice cliff surfaces are not able to maintain the original debris cover and melt rates change abruptly at the cliff boundaries, leading to characteristic cliff slope angles.

Besides this lacking linkage between the statistical model and the physical world, this manuscript is a valuable contribution to the important issue of including ice cliffs in the mass balance estimates of debris covered glaciers. I add minor comments, according to their appearance in the manuscript: P.2,L.7: What do you mean with "conventional input data". This should be a bit more elaborate.

P. 2,L.25: I do not see that this reduction of D is correct. The ice cliff transect s oriented parallel to x, but D is the ice cliff width from bottom to top, which is in an angle (perpendicular) to x. In this case an integral 0-D by dx makes no sense.

P.3,L.8/9: Could you clarify what ambiguities you mean? Your method only aims on slope, while radiometric sensors aim on roughness, brightness and temperature. There might be a range of possible ambiguities.

P.3,L.6-20: This paragraph is rather unclear to me. Is there any relation to published observations? Based on my observations (of course depending on the definition of an ice cliff), cliff have no ability to accumulate any debris larger than small rock flakes. The temporal evolution of cliffs shows that coarse debris can accumulate at the bottom and slowly covers the cliff, but then it should not be considered a cliff anymore. As I mentioned in the introduction, in my opinion the definition of an ice cliff makes only sense if it is connected to the considerable difference in thermal fluxes. Therefore the geometry aspect is only a supporting approach.

P.3,L.24: Cliff slopes mainly differ due to aspect and thus solar radiation. There are several publications connected to this topic. It is likely that there is some overlap, but it is probably rather small, because what could be the physical reason that coarse debris sticks to one part of the slope, but it slides from the other part with the same angle? The only situation I can see is a small and short slope, where the talus is large enough

to prevent additional mass movement from the steeper part.

P.3,L.29: The spatial resolution below 1m seems a random choice without any example from reality. You could provide some real observations about typical ice cliff height, slope and map view expression, to demonstrate which resolution is required to clearly capture ice cliffs.

P.3,L.31: Again the definition of conventional is missing.

P.3,L.32: 5m might be moderate for visible imagery, but for large coverage elevation data this is still high resolution.

P.4,L.23: High vertical accuracy is not critical for cliff localization, but for correct slope calculation, the relative accuracy is decisive.

P.5,L.2: Can you shortly specify the data used for calibration already here?

P.5,L.7: What do you mean with different surfaces? Types?

P.5,L.13: can you provide some specifications about the data collection? Chip dimensions, mean flight elevation, ground resolution, spatial overlap?

P.5,L.16: Again, can you please provide the spatial ground resolution?

P.5,L.30-32: This sentence is difficult to understand. Do you mean the differences in the ice cliff slope distribution indicates that a unique value cannot be used for larger regions?

P.6,L.10: Is this an additional GeoEye scene (23rd December), compared to the one used before (29th December)? Please clarify.

P.6,L.23: what is an "area threshold"?

P.6,L.27: As in many other cases, the manuscript would be more easily readable if expressions could be simplified: "elevation difference" instead of "rate of change in elevation". Please check also other cumbersome expressions.

P.7,L.21: what does “piecewise” mean in this context? This description is misleading. The iterative process is based on n iterations of varying beta. But the probability model is not “piecewise”.

P.8,L.6: The parameter of the probability map is p(x) not beta. Maybe it could be written: ice cliff probability maps p_i(x) in dependence of beta_i.

P.8,L.7: see comment above about complicating the readability: “vector ice cliff cape area” basically means “the resulting ice cliff area”

P.8,L.11/12: This sentence does not explain the characteristics of the function in relation to the parameters: Why are the y(beta) are unrealistically high for low beta? y'(beta) approaches 0 for larger betas due to the nature of the exponential function. This is true for the existence of ice cliffs, but also without. It is rather a distinction that high betas do not occur, if there are no cliffs, because debris cannot be maintained on steep slopes.

P.8,L.16: The formulation most accurate final A_cliff and coupled p(x) is not necessarily true. It is probably the optimum combination of A_cliff and p(x).

P.8,L.22: please refer to Fig. 6 already here, so that the reader can relate this difficult description to the graphical expression.

P.8, eq.4: what mathematical form should “distance (P1,P2,(beta, y(beta))) represent”? This formulation seems strange. Can you give some indication how you came to the resulting right hand side of eq. 4?

P.9;l.5: It should already be mentioned here that gama has small thresholds.

P.9,L.6: I do not see any problem with also calculating realistic values for P2 at beta=90°. Maybe it would be a better approach to use the point of inflexion of the gamma function as optimum. Because this is defined, based on the approximated parameters a, b and c only.

P9,L.10: This is not an additional iteration, but a final application of the model with the found beta_opt.

P9.,L.11/12: Does this indicate that you just use a manual beta_i if the methods fails? What would be the potential reasons for the method failing?

P.9,L. 20: The fractional debris covered area represents the original classification of debris covered glacier, where embedded clean ice is included in the debris cover?

P.10,L.6: The truth dataset x is probably different from the parameter x in p(x). Can you explain?

P.10,L.15: What is the secant of a slope pixel? Here it seems that you calculate a volume (length by area) instead of an area (length by length). Please reformulate.

P.10.L.19/20: Again please simplify: “tool output vector shape defined by A_cliff” means probably the same as “A_cliff vector shapes”.

P.11,L.12: delete “for error distribution <1”.

P.12,L.2: Can you comment here on the color coded distribution of “true positive” results in fig. 7? It seems there is a clear distinction in dependence of beta. What is the meaning of that?

P.12,L.9: magnitude (typo)

P.12,L.19: What would be the difference in this example of Fig. 6, using P_1 (beta_i) for 60° and 90°?

P.12,L.20/21: this needs more emphasis, that the truly artificial determination of beta_opt from the Gaussian distribution matches with the error optimum based on the calibration.

P.13,L.14/15: I do not understand this sentence. Can you please elaborate on the limitations with respect to slope angle?

P.13,L.14: Low slope means that the spatial dimension of the cliff is so narrow that the elevation difference between pixels is dominated by the more flat debris covered parts. The cliff portion itself is still "steep". Beyond the ice cliff means, beyond the spatial classification result.

P.13,L.19: What means “surface slope may be saturated”?

P.15,L.11: The example of Figure 8 should be accompanied by the characteristics of the ice cliff results from Canwell Glacier, because the performance depends clearly on the typical ice cliff width and slope distribution.

P.15,L.17: There still exists a result for beta_opt: about 19° and y_opt: about 4%. The only indication that the method fails is the non-assymptotic shape of the fuction towards the x-axis? What is the reasoning behind this?

P.15,L.29: What is meant with “mass wasting”? Is it removal of supra-glacial debris? Mass wasting usually is used for ice loss.

Fig. 5: Panel (e) is not mentioned on the caption.

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