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Interactive comment on "Debris-covered energy balance model for Imja-Lhotse Shar Glacier in the Everest region of Nepal" *by* D. R. Rounce et al.

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This manuscript is a well-written contribution to the community's understanding of debris-covered glaciers, presenting a summary of debris-properties at a variety of sites. The sub-debris energy-balance sensitivity to critical debris properties is a particularly useful analysis, as energy-balance approaches are increasingly used for debris-covered glaciers.

The microtopographic analysis for debris surface roughness and aerodynamic roughness estimation is also a valuable and novel contribution to the field as photogrammetric methods are increasingly used in glaciology. Although the authors find that z0 values within the bounds of literature values have the smallest effect on their subdebris melt estimates, I have some concerns about the methodology as presented, which I'm sure that the authors can clarify. It is important to note that these changes are not likely to change the principal results or weaken the authors' conclusions relating to the sensitivity of modelled subdebris ablation to critical debris parameters.

1. Error assessment:

1a. From Sections 3.1 and 4.2, it is unclear how and when the 'DEM Error' presented in Table 2 is calculated. Specifically, are these values the mean GCP errors in the SfM-derived DEM, or some other estimate? Agisoft calculates an error estimate, but this methodology is not transparent. Does this analysis occur before or after the x-y plane-fitting? The error assessment is critical as it determines the choice of final DEM resolution, and therefore the scale of analysis.

1b. The authors note that the error is dominated by human error, largely due to the choice of a type of cone (visible in Figure 2) which has no point. This certainly makes the total station survey and photogrammetric georeferencing very difficult, which is unfortunate as the dense point clouds from 40+ photos are probably very self-consistent (low internal error), although without a meaningful unit distance. What is the maximum resolution that could be achieved with the photo survey?

2. Justfication for the novel z0 method and equation:

2a. As presented, the authors use an established (Lettau-Munro) method to calculate topographic roughness, but find that it doesn't agree with their expectations based on literature. They then opt to devise a new method with an arbitrary threshold, which then produces values in-line with their expectations based on literature. Unfortunately, the field instrumentation did not include meteorological equipment to determine the effective aerodynamic roughness for any of their plots, which could have established their method rigorously (this is a problem that has faced prior authors, including, e.g., Rees and Arnold, 2006, and my own analyses). Instead, the modified values (which are used to constrain the optimization of z0) may as well have been selected from reported values. On the other hand, the lower-than-expected roughness values are worth re-

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porting, and should at least demonstrate the same pattern of inter-site variability seen by the modified method. It could have been very useful for the authors to also consider the many microtopographic methods in literature to estimate z0 (several are considered in Smith, 2014, also see Nield, 2013a). Alternatively, simply choosing surface roughness estimates from the literature will not change the bounds of the optimization or sensitivity test, and would provide a more consistent methodological approach, as both albedo and debris thermal conductivity are allowed to vary within literature bounds rather than according to in-situ observations.

2b. The new z0 method is not clearly motivated in the manuscript. With a few basic assumptions it is numerically equivalent to the original Lettau (1969) method, except that the authors here choose to use a different definition of an obstacle, by 1) initially looking only at profile changes greater than 0.01m to determine an average obstacle height, and 2) then only considering obstacles larger than this average obstacle height. In other words, the authors are implicitly suggesting that aerodynamic roughness is best predicted by the largest obstacles, where Nield and others (2013b) found a non-linear increase in z0 for an increase in obstacle size. The idea that a subset of obstacles (whether large or small) dominates the roughness effect is an interesting suggestion that has not been considered much in the literature, which I hope the authors include in their discussion and methodological justification for the z0 derivation.

2c. One difficulty with this implicit suggestion is the scale-dependency of the method relative to the plot size. Filtering the candidate obstacles as in the modified method removes the small obstacles from influencing z0, yet the ∼2m by 2m plots are also unable to encompass very large boulders present at similar study sites (the authors note a boulder ∼1m in diameter, but larger boulders of 3-5m diameter have been commonly observed on similar debris-covered glaciers (e.g. Hambrey and others, 2008)). Thus, only middle-sized obstacles (over 1cm based on the obstacle thresholding, and presumably under 50cm based on the plot size) are considered in the analysis. It would be particularly useful to see if the authors' z0 estimates change linearly with a different obstacle threshold or DEM resolution.

3. 'Topographic' vs 'aerodynamic' roughness:

3a. What is clear from the literature (Nield, 2013a) is that topographic surface roughness estimates do not always match the effective aerodynamic roughness obtained by meteorological instrumentation. This is a difficult problem to solve, because the length-scale of analysis is important for determining aerodynamic processes (and therefore turbulent energy transfer; e.g. Smith, 2014) but this presents a direct conflict between microtopographic methods (which can only be performed for a few-square-meter area while free of instrumentation) and meteorological methods (which reflect the aerodynamic roughness over an unknown area and require instrumentation in place). The microtopographic methods developed to estimate aerodynamic roughness have been primarily developed for surfaces with low permeability relative to the surface of debris-covered glaciers, where airflow into the debris matrix could influence actual aerodynamic roughness, without being accounted for in transect approaches. Consequently there is room for new microtopography-based approaches such as the method proposed by the authors; such approaches need to be shown to reproduce in-situ observations, however.

While this short comment may seem very critical of the z0 estimation presented in the manuscript, in truth the authors present one of the first such assessments for debris-covered glaciers and the study opens very important lines of inquiry to understand the role of debris surface properties. With some additional justification and discussion, the z0 section of the analysis will be much stronger and could lead to very meaningful further investigations.

With regards to the title, the current phrasing is a bit awkward (taken literally, a 'debris-covered model'?). A clearer alternative is 'Energy-balance model for the debris-covered . . .', although some emphasis on the optimization and sensitivity analyses would point readers directly to the key content.

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Select additional literature to consider for the z0 determination:

Nield, J. M., et al. (2013a), Estimating aerodynamic roughness over complex surface terrain, J. Geophys. Res. Atmos., 118, 12,948–12,961, doi:10.1002/2013JD020632.

Nield, J. M., Chiverrell, R. C., Darby, S. E., Leyland, J., Vircavs, L. H. and Jacobs, B. (2013b), Complex spatial feedbacks of tephra redistribution, ice melt and surface roughness modulate ablation on tephra covered glaciers. Earth Surf. Process. Landforms, 38: 95–102. doi: 10.1002/esp.3352

Mark W. Smith, Roughness in the Earth Sciences, Earth-Science Reviews, Volume 136, September 2014, Pages 202-225, ISSN 0012-8252, http://dx.doi.org/10.1016/j.earscirev.2014.05.016.

Interactive comment on The Cryosphere Discuss., 9, 3503, 2015.