

Interactive comment on “Monitoring Tropical Debris Covered Glacier Dynamics from High Resolution Unmanned Aerial Vehicle Photogrammetry, Cordillera Blanca, Peru” by Oliver Wigmore and Bryan Mark

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The authors would like to thank Christian Huggel who took the time to provide a detailed review of this manuscript. Huggel’s constructive comments and suggestions will greatly improve the quality and impact of the manuscript. Our responses can be found below a reiteration of the comments.

General comments: This is a paper documenting a pioneering UAV based study in the Cordillera Blanca in Peru. While UAV studies have become widespread over the

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past few years still very few applications exist on high elevation glaciers, and only a couple of them to date in the tropical Andes. This is among other related to the challenges of operating a UAV at elevations higher than 4000 m or even 5000+ m. The technical (UAV related) part of this paper is basically ok in my opinion. The question is how much insight the UAV data provides in terms of understanding of the glaciological processes. In general I think the paper is stronger on the technical side than on the process side. This is also due to the limited temporal and spatial coverage of the UAV data, i.e. one repeat flight, 1 year after the other, and focusing on the terminus area of the glacier. This limitations is totally understandable considering the challenges of operating a UAV in these conditions, on top of further requirements for high-precision geodetic measurements. And I would reiterate the merit of this study to provide high-resolution information on a debris covered glacier terminus in the tropical Andes where such studies are very rare or missing. However, both limited spatial and temporal coverage implies some constraints: 1 year is little to analyze characteristic patterns on a glacier, and the lack of information for the upper glacier areas limits the process understanding as it needs to ignore the dynamics of the entire glacier. The paper should more explicitly acknowledge these limitations.

Concerning the research questions (RQ) I think they are ok but RQ 3 and 4 are not fully addressed. The process of glacier change should be identified more clearly. E.g. can the effect of ice cliff related mass loss be better quantified? Some more evidence may be provided concerning the proposal that the ice cliffs are the primary control on mass loss. A comparison to other mass loss information on glaciers in the Cordillera Blanca could be useful. I'm furthermore unclear whether the large share of positive elevation change is just related to blocks and cliffs and other objects moving through the scenes? RQ4 is not addressed in much detail in the paper. E.g. much of the connectivity issue remains vague. I suggest to clarify which connectivity is exactly in place here: hydrological connectivity? Can this be addressed using UAV data? The text on future lake changes is not informed by UAV data. Does it tell us more than we know from other debris covered glaciers? E.g. the time frame?

I suggest some major revisions and re-organization of sections 1 and 2. The current organization is rather odd, or at least unusual, not supporting clarity. The research gaps and objectives should be better worked out. An option is that section 1.1 and 1.2 go in a separate background section after the Introduction. Section 2 could then be integrated in the Introduction.

Overall, I think the paper is mostly fine on the technical side but needs to be enhanced on the glacier process side, i.e. interpretation of the UAV data to the extent possible, comparison with available data from other glaciers in the region or possibly outside, and further dig into literature supported relevant processes. It may be interesting to be more explicit on the potential and limitations of UAV methods for fostering understanding of glacier change. Certainly, UAV's role for glacier monitoring is strong and increasing. But the authors claim that this data is providing unprecedented insights into glacier processes and this needs to be further elaborated and demonstrated. I'm confident that the authors can deliver a valuable and impact-full paper after some major revisions.

Response: We thank you for your feedback and agree that the limitations of these data should be more explicitly addressed with respect to what a limited spatial extent (only the tongue) and temporal range (two dates) can tell us about glacier processes.

Your general comments can be summarized as 1) refocusing the research questions/objectives, 2) reorganization and consolidation of the introductory sections, 3) enhancing the glacier process side.

To address these concerns we plan to significantly reorganize the introductory section, and reduce its length to improve clarity and provide less general background and focus more on the specific study questions. We will also revisit the stated objectives to ensure that they are fully addressed within the text. We will separate the results and discussion sections. Separating the discussion section will allow us to expand the glacier process side and tie our results into the existing literature more explicitly, this should improve the impact of the manuscript. In making these changes will will address the general

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comments you have outlined above, additionally we will address the detailed comments as outlined below.

Detailed comments: Page 2, line 1: rise in average glacier terminus elevation?

Response: We will reword for clarity.

2,7: Huggel et al should be 2003

Response: The tagline on the top of the paper says: Proceedings of EARSeL-LISSIG-Workshop Observing our Cryosphere from Space, Bern, March 11 – 13, 2002. Can you please confirm if the date should be 2003, or are you referring to a different paper?

2, 19 ff.: I don't think too many people would agree on the distinction of four primary methods to quantify glacier change. A more classical distinction foresees a distinction of glaciological, geodetic and hydrological methods to determine glacier mass change. I think the four methods identified here are somewhat odd, or arbitrarily identified (e.g. I would not see energy balance modeling as a method to determine glacier mass change). This also relates to page 2, 29 which is an incorrect statement in my opinion.

Response: We will significantly edit the introductory sections consolidating much of this material. We will address your concerns within this edited version.

3,4: you may add that energy balance studies are typically limited to point measurements of a weather station.

Response: Agreed.

3, 24: 10-100 m rather than decimeter (and vertical accuracy may also be indicated with a range)

Response: This is an error, should be decameter not decimeter. However, we will edit accordingly and include a range in the interest of improved clarity.

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5, 2: there are other recent studies using UAV in the tropical Andes but they are not yet published, at least to my knowledge.

Response: We are aware of groups attempting UAV work in the tropical Andes, and in the Cordillera Blanca, but have not yet seen any results from this work. We will edit to address this.

7, 3: spell out SfM the first time used

Response: Agreed, we will correct this. We spelled it out in 4.4 but missed the brief first mention of SfM in section 4.2.

7, 22: do you mean outdated for use. . . ?

Response: Yes, we will edit to improve clarity.

10, 10: I suggest to compare measured elevation changes to other available measurements in the Cord. Blanca.

Response: We will include this in the expanded and separated discussion section.

11, 25 ff: Section 5.3 is very thin. How compare these velocities to other debris covered glaciers? What are the implications?

Response: We will include this in the expanded and separated discussion section.

13, 14: did the UAV fly over 5000m? In the method section it is mentioned that flight height was 100 m above ground which would translate into a max of ca. 4800 m asl. In case you have experiences flying over 5000 m it may be interesting to know and report (but be specific).

Response: We have flown at elevations over 5000m, including at the glacier above Cuchillacochoa (Quilcayhuanca valley), and more recently at Huaytapallana in Huancaayo. However, for Llaca we did not need to fly above 5000m. We will clarify this point, expand as necessary and report on our experiences elsewhere in the expanded

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discussion.

13, 15: the insights into the processes that control glacier melt could be enhanced in the main body of the text (see above comment).

Response: Agreed. We will expand and improve the glacier process side of this paper in the revised manuscript.

List of references: please check. Some references are duplicated.

Response: Agreed.

Figure 7: how was the glacier boundary determined? This is often difficult on debris covered glaciers.

Response: The glacier boundary was determined through visual inspection of the ortho imagery and the DEM surface in planimetric and 3D views. We followed terrain breaks and textural differences. There is some error in doing this, however it is much easier to identify the boundary with the high resolution imagery and DEM than when using coarser satellite derived data.

It would be helpful if the location of figures 10, 11, 12 could be indicated

Response: We will include extent rectangles for these figures.

in Fig. 7. The color scale should be improved: especially the 0 value level could be made more clearly visible.

Response: We will improve the colour scale and consider using a categorical scale bar as suggested by the anonymous reviewer.

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