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Comment on esurf-2021-8

Anonymous Referee #2

Referee comment on "Application of sequential unmanned aerial vehicle surveys to monitor the kinematics and geomorphological changes of a rapid rock glacier" by Sebastián Vivero et al., Earth Surf. Dynam. Discuss., <https://doi.org/10.5194/esurf-2021-8-RC2>, 2021

Review of the manuscript "Validation and application of sequential unmanned aerial vehicle surveys to monitor the kinematics of a rapid rock glacier" from Vivero et al. Submitted to The Cryosphere earth surface Dynamics.

This paper attempt to show the importance of using sequential UAV-surveys and Structure for Motion photogrammetry workflows for monitoring and quantifying changes in rock glaciers. This study is based on the analysis and comparison of very high-resolution photogrammetric products (i.e. DEMs and orthomosaics) acquired between 2016-2019.

The manuscript has as the main goal to propose the benefits of using UAV surveys through a "rigorous" methodological approach to estimate surface velocities and volume changes over the Tsarmine rock glacier.

Finally, the authors analyze the UAV monitoring strategies as well as the kinematical behavior and surface changes of this rock glacier.

Due to the general arguments and some detailed comments unfortunately I think that there are some major drawbacks in this manuscript that from my point of view could question the acceptance of the manuscript. In addition, even though I am not native English speaker, I think the text requires some corrections. I strongly recommend having the manuscript reviewed by a English native speaker.

General and specific comments are described as follows:

General comments:

SfM methodology

- First, the main contribution of the study is focused on the establishment of a "rigorous methodology" for monitoring rock glaciers using UAV. However, this aspect is crucial because in this study I could not find significant differences (in the methodology) concerning other studies.

- No discussion is presented about the “implementation of this rigorous protocol” methodology. At this point, I wonder, what is the main difference between the traditional UAV surveys and the new one you propose? More details are needed about specific steps on the protocol, e.g. orientation parameters, the quality of the point cloud, and the differences between GCPs and external data.
- Second, you mentioned in the text the implementation of the “co-registration” stage as the main difference between other studies. However, I think there is a misinterpretation of the term “co-registration”. Initially, you speak of multi-temporal co-registration (Figure 2) to the fact of always using the same 4 control points for the aerotriangulation stage. However, co-registration is more related to comparing one product with another, (i.e. master DEM to another, co-registration on SAR images that follows the same principle). The fact that you are always using the 4 points for the aerotriangulation stage is part of the "rigorous protocol", nevertheless, this is not necessarily a co-registration stage. In your case, the co-registration step should be implemented after ortho-images and DEMs generation and with independent references (points, DEMs, etc) that were not included in photogrammetric analysis.
- Then you talk about that the co-registration based on the Helmert methodology you do not find rotations or changes of scale. This issue is obvious since you always used the same 4 points for aerotriangulation. Then, you mention shifts between orthomosaics (computed again using Helmert methodology) but these values are not reported anywhere. Is this important? Or not?
- Later, only the ortho-images were "co-registered" and not the DEMs. In the case of x-y shifts in the ortho-images were important, this also implies strong variations in the elevation changes calculated using the non-coregistered DEMs.
- Finally, you mention that you use small areas to perform this co-registration, however, these areas represent a very small portion of the overall DEMs (which is not presented indeed). The fact of selecting small areas can lead to misinterpretations in the uncertainties.

Surface velocities

- Another methodological question arises on the presentation of rock glacier surface velocities. More details are needed to discuss the validation of the feature tracking products. Also, the current manuscript does not allow the correct interpretation about technical details like grid size, correlation windows size which is fundamental. For example, given the current displacements of Tsarmine rock glacier in the frontal part, adaptable window correlation size are maybe needed. Independent comparisons between products (correlation results) should be mentioned and results should be provided as supplementary material.
- Given the recent importance of these in geosciences, I consider that it is necessary to expand a little more literature on the methodology used in other studies and to be able to propose a new one.
- As showed in Table 1, your latest survey was made early in the morning. This aspect is not even mentioned, we don't have any idea if the presence of shadows in the morning or light conditions could harm the feature tracking process.
- The fact that you are using very high-resolution data leads to fine changes in image texture and quality. There is no discussion about the influence of using a very high-resolution image to compute surface velocities fields.

Thickness changes

- This study leads with very high-resolution data, therefore the validation of these must be rigorous and independent. Although the manuscript presents a validation step (independent using GPS data), robust statistical analysis is necessary to support these results.
- Additionally, as you are computing differences between many DEMs, uncertainties between those should be presented at least as supplementary material.

Specific comments:

P1L10-11: as details about UAV surveys were not detailed, the "rigorous procedure" is ambiguous and it is no possible to understand why it is rigorous.

P1L19: On the phrase "we almost archived the same accuracy as the GNSS-derived velocities", numerical comparations should be provided for this comparison.

P1L26-27; The phrase "Rock glacier analogues [...] (Cliford et al.,2013)" does not contribute to the discussion of the document and is out of context.

P2L33: the word "top" is too ambiguous. I suggest changing this by "surface".

P2L37-39: The phrase "The most extending [...] Schneider, 2001)" should be revised because it is no clear.

P2L44: Annual surveys derived from what?

P2L62-63: on the sentence "[...] more recently using differential GPS (Berthling et al., 1998)" the reference is too old.

P3L64-68: In this paragraph, you mentioned remote sensing techniques to measure rock glacier displacements and all the references are at the end. However, we do not know which corresponds to which one. I suggest changing this paragraph putting the correct references after each technique.

P3L70: There is no only Kaab that has been working on photogrammetry and rock glacier. For example, you forgot Kaufman publications and one of your Swiss colleagues Michelletti et al 2015, Australian and French colleagues.

P3L80-81: Ok, we are gree that UAVs are changing and improving, but it will affect the way to make photogrammetry? I think this sentence is too ambiguous.

P3L85: What is necessary?

P3L87: the benefits of UAV have been already proved for rock glaciers? Why are you interested in proving these again?

P4L119: Mounty peak instead that "peak monthly".

P5L127: 58 points is a specific number, it cannot be "around".

P5L128: [...] six fixed points [...]. In this sentence, you write six? However, on the L127 you mentioned "58 points..." with a number. Please harmonize.

P5L141: Table 2 is mentioned before Table 1.

P5L151: Add focal length units.

P5L152: Add focal length units.

P5L155- L152: The sentence "However [...]" is too ambiguous. Poor information is provided about the independent test.

P6L164-165: The sentence "Moreover [...] ground [...] is repetitive. You already mentioned this on L146. Please check the redundancy.

P7L208: are you sure that the way to present "average density of points" is the correct way? Please verify this.

P8L223: I am confused. On the L210 you said that you generate ortho-mosaics at 0.1 m resolution. But here on the L223, you calculate surface displacements using orthomosaics at "one-tenth of the original pixel size (i.e. 0.01m)" which is not coherent. Please verify.

P8L229: Do you check the influence of shadows on very high-resolution ortho-mosaics? This aspect is not even mentioned.

P8L234-237: Disagree. Please see my general comment. No detailed information is presented about this issue (i.e. differences in stable areas, x-y shifts, etc.).

P8L240: Again, 69 is a specific number, it cannot be "around". Also, these 69 "rock surfaces" are not displayed anywhere.

P8L241: x-y-shift translation vectors are not mentioned anywhere. Please provide a detailed description of the specific values obtained from the Helmert similarity.

P9L255: What means ca? Please check this. Moreover, no information on this comparison was presented. Please provide Z differences of these 35 points.

P9L261-262: Uncertainties of DEMs, as well as uncertainties propagated, through DoD calculation should be shown.

P9L265: how much is this area examination? Please presents values on square meters.

P9L272: Did you measure the displacement of these 35 points? Additionally, these are the same used in section 3.4 (DEM assessment)?

P11L335-339: Disagree. I think this paragraph is too ambiguous because it depends on your goals. Recent developments of satellite photogrammetry (i.e. world-view 2 and 3, Pleiades, etc) showed the high quality of their products even on rock glaciers.

P11L242: Disagree. Again, too ambiguous. The UAV survey can early take more than 30 minutes because it is weather-dependent. In this case. In case the conditions are not optimal, the acquisition time can easily exceed 30 minutes. I suggest changing this part.

P12L344: Disagree. The limits of detection depend on the size of the window used for correlation and it should be tested on fixed areas. Some information about these values is partially reported in Table 3, however, more detailed information needs to be provided.

P12L350-356: This paragraph does not contribute anything to the study. It is maybe a general discussion about "optimal conditions" but this topic is already known.

P12L258-269: Disagree. This paragraph is redundant and a repetition of what the others

already do. However, no technical comparisons are given about your protocol against the others (i.e. planning, processing, etc). I suggest revising or excluding this paragraph.

P12L367-369: Ok, you are using stable terrain to validate your products but detailed information about these areas is not presented anywhere. Also, these areas are very small compared to the total extension of your DEMs. Please see my remark on P8L234-237.

P12L375. The fact that you perform all working steps does not guarantee high-quality results. It should be proved by comparing independent methods. Additionally, a detailed report is needed to support this argument. I suggest revising or exclude this paragraph.

P13L404: "[...] net frontal retreats (Fig. 6)" reference is missing.

P14L411: Surface velocities are showed instead of kinematic points. Please change this and also on Figure 9.

P14L428-430. Agree, but what is the contribution of this paragraph to your work? This paragraph takes place at the introduction instead than in the discussion section. Please revise this or exclude this paragraph.

P14L430-432: I could not find an uncertainty analysis. You are relating this to GNSS comparison? The fact that you are using very high-resolution products needs a robust statistical analysis concerning the uncertainties between photogrammetric products notable on stable areas (for DEMs) and surface velocities.

P14L434: What are the differences between your UAV protocol against others? More details are needed to be related to "customizable data acquisition".

P14L436-437: Agree. With your data, you could explain more extensively this process regarding even into sub-periods.

P15L449-450: Even this works shows a detailed description of UAV surveys on rock glaciers, I difficult to me what's is the main difference against a normal UAV survey. The current status of the manuscript does not show in detail the relevant differences of SfM photogrammetric studies.

P18L564: the list of references needs revision. The reference Fugazza [...] is not cited in the text.

Figures and tables

Figure 1. Panel b: add a north arrow to make clear the image.

Figure 2 Panel b. Disagree with multitemporal co-registration. Please see my remark in general comments.

Figure 4: I think that the community has much more interest to know which are the values of these limits of detection (LoD) instead than only the text. Please show specific values of LoD.

Figure 5. This figure is nice. However, there is not always clear to relate a 3D view to a plane view as shown in Figure 4. I suggest changing the 3D image by a more explicit figure that shows the localization of the longitudinal profile.

Figure 6. It is difficult to see front evolutions because the lines overlay. You said the mean "front line displacements" did you calculate this displacement over the entire line? If yes, precise which method did you use?

Figure 7. These results could be showed better. Is too difficult to interpret this figure because even if the intervals are regular, there is not obvious to see ± 0.8 . The intervals should be divided by entire numbers (-1.0 - -2.0 as an example) instead of 0.8. Also, on the color scale, you say that you found values greater (less) than +6.8m (-6.8m), I suggest that you present the min and max values as well as the north arrow. Finally, please use a color scale adapted for colorblind people.

Figure 8. On panel a), please indicate the photo was acquired.

Table 1. Please provide the "GSD" acronym.

Table 1. if it is a rigorous protocol, all flights were made between 11h-13h. Why was your last flight in the morning? I found no justification in the text. Please specify this. Did this early flight have any consequences on the velocity calculation? (important presence of shadows).

Table 2. Change "errors" by uncertainties.

Table 2. Why are you expressing uncertainties in pixels? It should be better expressed in meters.

Table 3. Additional statistical analysis is required.

I hope you will find my comments appropriate,

Best regards