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Reply on RC1

Sebastián Vivero et al.

Author comment on "Kinematics and geomorphological changes of a destabilising rock glacier captured from close-range sensing techniques (Tsarmine rock glacier, Western Swiss Alps)" by Sebastián Vivero et al., Earth Surf. Dynam. Discuss., https://doi.org/10.5194/esurf-2021-8-AC1, 2021

We want to thank Gernot Seier for his technical review that will help to improve some essential aspects of the manuscript.

We agree with most of the general and detailed comments made (as explained below) and modified the manuscript accordingly.

The referee comments are in *italic* (red) font, and our responses and revisions are in normal (black) font.

General comments:

Your manuscript entitled 'Validation and application of sequential unmanned aerial vehicle surveys to monitor the kinematics of a rapid rock glacier' intends to show the added value of using UAVs and Structure-from-Motion-based photogrammetry for the quantification of rock glacier dynamics. The study is based on the comparison and analyses of photogrammetrically processed images acquired in the period 2016 to 2019. The manuscript aims at proposing technology-immanent benefits of using UAVs for rock glacier monitoring. As the main outcome, kinematic and volumetric changes and its variations and characteristics are presented.

We learnt from your comments and agree with your central assessment that our aims and arguments concerning the benefits of using UAV monitoring techniques in rock glaciers were not well understood. Our objectives are to propose and explore a monitoring method for rock glaciers using UAV-derived data validated on a destabilized rock glacier. Our study's findings can potentially contribute to established permafrost monitoring networks (like PERMOS). As explained during the first introductory paragraph, rock glacier dynamics cannot be directly measured by remote sensing techniques alone (i.e. UAV-SfM). Therefore, we claim that our paper deals with quantifying rock glacier kinematics (i.e. change in geometry with time), which is a piece of complementary information to understand the dynamical processes associated with the creep of mountain permafrost (see Haeberli et al., 2006).

Due to the general arguments and some detailed comments, I think that the manuscript needs amendment, please see my remarks as follows. In addition, even though I am not a native English speaker, I think the text is basically well-prepared and requires minor proofreading only.

Thanks for these comments. We are also not native English speakers, and we speak different languages, but we enjoy disseminating our work in English and as well as in other languages.

According to the manuscript's title and presentation, the main aim of your contribution is to introduce UAVs in rock glacier monitoring and to propose SfM-photogrammetric UAV surveys as favorable approach compared to established techniques. This assumption is a central issue because even though UAVs are not as long in use as other methods and devices, today, UAVs are already established in geosciences. Therefore, research based on UAVs should either focus on methodological questions, which is not case in your study, or otherwise should in detail discuss and explain geomorphological changes, which rather should be the aim of your manuscript. Therefore, I think it is necessary to revise the title and presentation of the manuscript. In addition, as there are other previous studies, please expand the relating literature discussing UAV-based surveys on rock glaciers. Also, the discussion on relating studies as in the present form needs revisions, please see my detailed comments concerning P12L362-376.

We partially agree with this statement. As indicated in the manuscript introduction, we propose UAV surveys as a complementary method to monitor rock glacier kinematics. For achieving this, we exploited the capabilities of the image correlation software CIAS to derived landform-wide kinematic fields from sequential UAV-SfM-derived orthomosaics. A fundamental step before explaining the geomorphological changes was to validate the results via independent ground information during five consecutive periods. The good agreement with data collected by repeated dGNSS demonstrated the robustness and reliability of the monitoring protocol (Figure 3), which similar studies have not demonstrated. Nevertheless, we decided to change the title of the revised manuscript to "Application of sequential unmanned aerial vehicle surveys to monitor the kinematics and geomorphological changes of a rapid rock glacier". We also included more literature discussing UAV-based surveys on rock glaciers (Halla et al., 2021; Kaufmann et al., 2018), but it has to be noted that published studies having quantified rock glacier kinematics using UAVs remain very few to our knowledge.

In terms of validation of the UAV-based results, geodetic measurements are shown in order to independently assess the quality. However, the way of how the UAV survey-related design and the relating provided information are presented does not allow to completely comprehend the methodological benefits. Several details concerning the survey design, such as the planned and actual flight paths and coverages, the estimated theoretical precision compared to actual achieved precision, the details of processing including, e.g., the quality of tie points on the image plain, as well as a dimensionless theoretical assessment of the achieved quality are just some but relevant information that would help to promote UAV-based surveys, which is not the case in the current form of the manuscript. I think that such a methodological discussion and information is the prerequisite for a technology-featuring presentation of the manuscript. In contrast, the *manuscript appears as a well designed and presented case study of a rock glacier's kinematics but does not introduce new methodological approaches and therefore, should focus on the characteristics and specifics of the landform investigated. In this sense, it appears rather inappropriate to contend that UAVs are a new method for rock glacier monitoring as this is not the case.*

The quality of 3-D point clouds and orthomosaics derived from UAV-SfM workflows depend on several physical, geometric and processing parameters (Benassi et al., 2017): surface texture and contrast, air refraction index, lighting conditions, sensor quality, global or rolling shutter, image sharpness, flight height and speed, grid pattern, camera angle, forward and side overlaps, the distribution and accuracy of GCPs, camera self-calibration, BBA, image matching algorithms, point cloud densification and outlier removal algorithms among other parameters. In this context, determining the quality of several SfM parameters is a complex mission, as the number of related variables is quite large (even though some parameters can be kept unchanged like grid patterns or image overlaps). Furthermore, a degree of spatial uncertainty accompanies the orthomosaics generation as well as the performance of the automatic image matching via correlation (Leprince et al., 2007; Redpath et al., 2016). Nevertheless, when deriving surface movement from sequential orthomosaics, we exploited the relative accuracy of the coregistration on stable sectors as the overall source of uncertainty. We expanded the section dealing with the coregistration assessment and added a new table. In doing this, we assume to account for the variability of these different parameters represented on either the orthomosaics or the point clouds (Cook and Dietze, 2019). We made this concept more evident through the manuscript. We also include more technical details about the UAV surveys on the new supplementary material.

Focusing the manuscript on the investigated landform would require adding many more data acquired on-site for near 20 years to completely reorganize it and shift the purpose to the landform dynamics. It was not our primary goal but to explore the benefits/complementarity of UAV-based survey to the ongoing kinematic monitoring. We prefer to keep the paper in its original objective but improve it accordingly (see our various responses below).

Concerning the survey design it would be necessary to first define what is required in terms of survey quality and the landform investigated in order to then conclude whether it was appropriate or not. Instead, the manuscript is characterized by a rather vague survey concept in order to subsequently conclude that the results are quite appropriate. In other words, at first it should be declared what survey quality is strived and estimated, which would then allow for assessing whether or whether not the results are appropriate.

We acknowledge that the UAV survey concept was also not well explained. The strived survey quality was not explicitly mentioned. As most of the landform is moving up to several meters between each survey and according to that, the relative stability of the surface material, which is exploited with any ground-based or remote technique is not fully insured (some tilting or sliding of the surface boulders can be expected in many places), a minimal accuracy of about 10 cm yr^{-1} or better in the resulting displacement rate would be adequate. Nevertheless, we maintained that the GNSS survey design follows the protocol design by Lambiel and Delaloye (2004), which has been established as a standard for rock glacier kinematics, at least for some national monitoring initiatives (see PERMOS reports). We depart from this framework with the addition of UAV surveys during the same TGS campaigns. The choice of the spatial and temporal characteristics of the UAV surveys was not trivial. The spatial resolution allowed to track rock glacier surface texture at 10 cm finely, and the biannual surveys are adapted to the high rock glacier speeds. We also indicated and discussed some factors that can degrade the quality of the UAV-derived velocities. The experimental design in this challenging terrain can provide a better assessment for destabilized rock glaciers, as this is the case for Tsarmine. We have made some amendments to the original manuscript to make this clearer.

The description of the processing needs revision. Without presenting and discussing photogrammetrically relevant parameters, such as exterior and interior orientation parameters, the quality of tie points and residuals of GCPs, photogrammetric studies are not comprehensible and meaningful. Therefore, the robust and detailed workflow that is stressed by the authors lacks these important components, which I think needs to be amended and the formulation revised. In addition, today, numerous articles are based on SfM-MVS photogrammetry. I suggest to expand and discuss the relevant literature in more detail, as there is no ample discussion and interrelationship with existing subject-specific-literature yet.

We agree that the data processing sections need revisions. We changed the emphasis to the evaluation of the UAV-SfM-derived products rather than the SfM processing. In doing so, we included a new section (3.4 Comparison with the TGS) where we better explained the validation protocol. We also expanded the section dealing with the coregistration assessment and added a new table. Regarding your last sentence, we are aware of most of those studies dealing with SfM-MVS photogrammetry, particularly in ESurf, but tried to keep the focus on the rock glacier applications.

Other methodological questions could deal with potential issues using the RTK-based survey configuration, e.g., are UAV-based surveys solely using direct georeferencing really capable of achieving the required quality? This could be worth discussing.

We thank the reviewer for this comment, but this methodological issue has been already discussed (see Benassi et al., 2017). The EBee RTK cannot achieve the so-called "direct georeferencing" since the camera attitude angles are not constrained by the IMU (only default values are available). That is why we performed a GNSS-support Aerial triangulation (GNSS-AT) with at least four GCPs. We included this explanation in the revised manuscript.

The presentation of the rock glacier's velocities based on an area-based matching algorithm needs revisions. The validation of the velocity values should be expanded and the description revised, as the matching settings (grid size, correlation window size) are necessarily adjusted to the specific case and pair of images, e.g., the approx. 13 m large windows should be able to track 13 m long displacements (cf. P8L227). As a consequence, this results in high correlation values (R^2 values presented in Section 4.1) and therefore, such a comparison of geodetically and NCC-derived velocities inherently cannot be *completely independent. And this at least should be mentioned. Similarly, the presentation of the movement's directional quality (Section 4.2) could be more comprehensible by explicitly mentioning that the directions were smoothed, as a directional filter was applied (Section 3.3).*

We thank the reviewer for pointing on this valid concern, which we have been aware of during our CIAS analysis. The decision to use a particular combination of reference and search window sizes was pragmatic because the expected maximum displacement can be estimated beforehand from the visual inspection of sequential orthomosaics. In this regard, keeping the exact configuration of the search and window sizes provided only a theoretical maximum displacement limit of about 13 m (more than twice the maximum value observed) for each pair of consecutive orthomosaics. Repeating the CIAS processing using larger windows sizes increases both computing time and the number of bundlers (for a detailed explanation, see Kääb and Vollmer, 2000). As such, we do not agree with the argument of high r^2 values (Figure 3) are related to the limit of 13 m, as we were able to quantify different maximum displacement values during different periods.

We removed the application of the directional filter, as we found it to be irrelevant for the few serious mismatches, which can be manually removed. The overall change caused by this filter was relatively small, and therefore, there is no smoothing effect in the results presented in Section 4.2.

The surface velocity fields should be revised, as thus far, categories of different velocities are presented and instead, vectors that show the exact displacement value should be presented, which also would allow for including and presenting the displacement vector-specific LoD value. As mentioned in Section 3.3 (P9L251), it would be a real benefit for the reader if these vector-specific LoDs would be shown in maps.

We thank the reviewer for this suggestion. However, we performed several tests and reached the conclusion that standardized velocities categories are better suited to highlight the rock glacier velocities changes during the study period, as this is also the case for displaying elevation changes. For instance, previous publications in ESurf also prefer to show such quantities in categories rather than individual vectors (see Clapuyt et al., 2017; Monnier and Kinnard, 2017; Piermattei et al., 2016) as well as other rock glacier kinematic studies (Micheletti et al., 2015; Scotti et al., 2017; Seppi et al., 2019). We also performed some tests to integrate the LoD values in Figure 4, but we did not reach satisfactory results. The mean LoD for each period is now indicated in Figure 4, and LoD statistics for each period are shown in Table 3.

Detailed comments:

P1L10-11: As the survey configuration details are not completely outlined, it is not tenable to contend that a rigorous procedure is introduced.

Agreed, we removed "rigorous" from the abstract.

P1L20-21: As the comparison with GNSS-based measurements is mentioned here and the description of the rock glacier dynamics, the main research contribution is the investigation of the specific rock glacier's dynamics and behavior rather than methodological questions.

We included more arguments about the need for the improvement and validation of the monitoring protocol.

P2L36-48: What about rock glacier kinematic surveys beyond the European Alps?

Without being Eurocentric, we must acknowledge that the majority and best quality rock glacier kinematic data are found on the European Alps (Kellerer-Pirklbauer et al., 2018). There are intermittent examples of early rock glacier kinematics surveys outside the European alps, such as the Pedregoso rock glacier in the South American Andes (Marangunic, 1976) or the Galena Creek rock glacier in North America (Potter, 1972). Promisingly, recent remote sensing research has become critical to reducing this gap between Europe and other rock glacier regions (Blöthe et al., 2020; Kääb et al., 2021; Strozzi et al., 2020). We added some information for extra-European rock glaciers.

P2L52-53: Style – "[…], evidence of […] identified as evident signs […]".

Agreed, we removed "evident" from the sentence.

P2L61: Style – "The quantification […] has been measured […]".

Changed: "Rock glacier kinematics have been traditionally…."

P3L72: "[…] Unmanned Aerial Vehicles (UAV) systems […]". Plural ending not correct, please check throughout the manuscript.

Thanks. We have corrected this issue.

P3L79-80: Expand the list of published studies dealing with UAV-based rock glacier surveys. As there are already studies demonstrating the benefits and challenges of UAV-based rock glacier surveys and as generally, the landform rock glacier does not entail a specific methodological treatment compared to other landforms and as SfM photogrammetric UAV-based surveys are today already established, this cannot be the research question.

As mentioned before, we have included the work by Kaufmann et al. (2018). However, we did not find other studies dealing exclusively with UAV-based rock glacier kinematics. Furthermore, we argued that some rock glaciers are currently destabilized (like Tsarmine), and they can behave in a unique fashion that does not follow other types of landforms (they have a different dynamical response than glaciers, solifluction lobes, debris-cover glaciers or landslides). Therefore, we emphasised the challenges raised by the unique response of destabilized rock glaciers and the potential benefits and limitations of a monitoring strategy based solely on sequential and systematic UAV surveys.

P3L81: Yes, but both a protocol and relevant details of the UAV-related configuration and processing are not discussed in this manuscript and therefore, this should not be mentioned and understood as the research question.

We cannot totally agree with this statement. The study area is characterized by the availability of high-quality ground truth information like permanent GNSS stations and biannual GNSS surveys, together with complementary close-range remote sensing techniques such as terrestrial laser scanner (TLS) surveys and webcam devices. Our results are discussed against similar research (Dall'Asta et al., 2017; Fey and Krainer, 2020), focusing on quantifying rock glacier displacements and their related uncertainty, together with the validation strategies. Additionally, the discussion also provides some perspectives when our results are compared with concurrent InSAR investigations on the same rock glacier (Strozzi et al., 2020). With the details added to the manuscript (see other responses above) and the improved discussion, we are convinced that the stated research question is justified. To make those arguments more evident, we have changed the sentence: "However, as UAV systems are continuously changing and improving, there is still scope for the assessment of monitoring protocols using different UAV configurations in high altitude and challenging terrain."

P3L82: It is meant "[…] high altitude please clarify.

Done.

P3L84-85: General statement that I do not support. One could also argue, as the surface velocities are in a hardly observable cm-range, a thorough validation is necessary. Therefore, no, different rock glacier velocities are not an argument.

We modified this sentence. “In the context of rapid rock glacier acceleration and destabilization, new and customizable monitoring techniques are necessary.”

P3L86: So far it was not mentioned in the manuscript that the quality of kinematic data is an issue, so, maybe it should be clarified at first that kinematic measurements are of diverse quality, which is why a thorough validation is necessary.

Thanks, we clarified this: “Likewise, as the quality of kinematic data obtained from different remote sensing techniques can be variable, a systematic ground validation through consecutive periods is required to improve the monitoring techniques and shed light on rock glaciers' kinematic behaviour in great spatial detail.”

P3L92-93: This is the formulation of the study’s aims, which are a better understanding of the rock glacier behavior and dynamics. Therefore, your study contributes to rock glacier monitoring rather than the technique or methodological questions and therefore, the presentation should not focus on UAVs for the simple reason that these are maybe not that commonly used on rock glaciers yet, which is, if indeed, possibly coherent with the number of rock glacier studies.

Thank you for these comments. We have improved our arguments, and we indicated that a particular monitoring technique and its validation are still needed for rock glaciers, especially during acceleration and destabilization phases.

P4L111: “[…] 2016. the []”.

We do not see this error in the original manuscript.

P4L114: Please clarify, that this specific landform “Tsarmine” was meant.

Done.

P5L427: As it is a specific number of points (58), it cannot be “around”.

Corrected.

P5L132: I would say that 2 cm are also in the cm range, please clarify.

We changed to “1–2 cm range”.

P5L141: Table 2 is mentioned before Table 1, modify.

Revised.

P5L152: Add the focal length’s unit.

Added.

P5L154: Do you really mean “accuracy” of the RTK-based camera positions, as I cannot see the relationship to other, independent measurements, please clarify.

We changed to “precision”.

P5L155: Explicitly mention that the low accuracy is too low in terms of the study-relating aims, because low accuracy per se could still suffice.

Clarified: “The UAV camera orientation values (Roll, Pitch, and Yaw angles) recorded by the Inertial Measurement Unit (IMU) device have low accuracy, hampering a reliable block orientation by the direct sensor orientation (DSO) method.”

P5L155-156: Please expand with a few words the tests mentioned in order to make the statement more comprehensible. In addition, with “mainly” you mean “mostly”?

We changed to: “However, independent tests using different combinations of GCPs have found that position accuracies are mostly too optimistic, suggesting that some sort of ground or check control should be included during the UAV block orientation (Benassi et al., 2017).”

P6L160-161: As you describe the planned image overlap of your survey, a citation does not make sense; delete the reference or clarify.

Agreed.

P7L198: It should be "[…] GCPs next to the […]" or similar.

Corrected.

P7L203: In order to discuss the quality of the bundle adjustment it is necessary to present and discuss the data of exterior and interior orientation parameters and the quality of the tie points.

Thanks for pointing out this. As we mentioned before, we are not thoroughly discussing the quality of the SfM procedure but rather the quality of the sequential datasets derived from it. We included other results in Table 2 (Mean Reprojection Error). We argued that the checkpoints are the most suitable way to independently gauge the quality of the bundle adjustment (see James and Robson, 2014) rather than the interior or exterior orientation parameters.

P7L210: Check plural ending and indefinite article. Also, mention for clarity that the orthomosaics were resampled to 0.01 m, as the GSDs differed (cf. Table 1).

We corrected this and changed the manuscript accordingly.

P7L213: Mention the subsections number instead of "previous step", as this would be

clearer.

Changed.

P7L21-216: As you describe the principle, it does not make sense to relate to rock glaciers; the principle is valid for every surface surveyed.

We changed to “moving surface”.

P7L217: With “extensive” it is meant “larger”, which is more applicable, I think.

We changed to “larger”.

P8L237: Please check with the journal requirements whether or not the citation (“their Fig 2”) is appropriate.

We are not aware of such specific requirements. Nonetheless, we will leave it to the potential typesetting and copy-editing services provided by Copernicus Publications.

P8L240: As it is a specific number of points (69), it cannot be “around”. Also, show the 69 subareas in the map.

We changed to “between 60 and 69 stable rock surfaces” and included a new table with the results of the coregistration assessment. Also, there are not 69 subareas, but there are 69 points circumscribed to the stable sectors in Fig1C.

P9L255: As it is a specific number of points (35), it cannot be “ca.”. Moreover, I cannot find the relating results of the mentioned comparison. In addition, show the spatial distribution of these 35 points in a map, as this is not the case yet.

We changed to “35 TGS points”. The results of this comparison and the spatial distribution of these points are included in a new supplementary material (table and figure) related to this manuscript.

P9L272: For understanding: these are the same 35 points mentioned in Section 3.4 (here used as independent z check points)?

Yes, they are the same. We made this more evident.

P9L274: Is the term 'significant' in the sense of statistically significant appropriate here?

We removed the term "significant."

P11L335-337: Agree, but why are then the relating details of acquisition, processing and analysis not presented yet?

The requested details of acquisition and processing are presented in sections 2.3 and 3.2, and the corresponding analysis is delivered through the "Results" section.

P11L337-339: Disagree, the inability of potentially new UAV-users cannot be anticipated, which is also attenuated by the high degree of automatization and usability of modern UAVs. Also, the depreciation of commercial UAV flight providers is not comprehensible as the necessary information needs to be negotiated and should be part of the contract, which is why commercial UAV survey providers of course could deliver the required data and metadata.

In the framework of remote sensing approaches, our sentence makes a discussion about platform operation between classical aerial surveys or satellite programs (notably companies) and new UAV surveys (primarily researchers). In this regard, we stated the benefit of low-cost surveying tools, which allows the scientists to be in charge of the data acquisition (Anderson et al., 2019; Eltner et al., 2016), potentially closing the gap between commercial aerial photography and small research initiatives.

P11L342: This discussion could be more controversial as 30 minutes could be the net flight time, but usually the setup and pre-flight checks also need time as well as accompanying geodetic measurements and moreover, UAV flights are more weather-dependent (so probably it is necessary to wait for optimum conditions), which is why usually a UAV survey takes remarkably more time than 30 minutes.

To our experience, the typical UAV setup and pre-flight checks take less than 5 minutes. Since the permanent GCPs are fixed and already provided, this step is not needed for subsequent UAV surveys. In any case, we now indicated that the actual data capture from the first to the last images usually takes no more than 30 minutes (roughly the time between takeoff and landing) for the size of the Tsarmine rock glacier, which is considerably less time than the regular GNSS data capture (which is 2 hours from the first to the last measurement). Please, check Table 1, where the individual UAV survey times are indicated. The flight plan mission is now included in the supplementary material.

P12L344: The different LoD values resulting from NCC and GNSS are not completely shown yet, only partially in Table 3.

The LoD values from GNSS are now included in Table 3.

P12L352: Elaborate, what are the "[…] suitable environmental conditions […]"? In addition, what is the point of " […] that means about four months a year […]"? Usually, also geodetic surveys (except permanent measurements of a single point) are conducted in the snow-free season, so this is not specific for UAV surveys.

We changed to "suitable weather conditions". We changed to "For our study area, the snow-free period usually spans for four months a year".

We have to disagree with the second sentence, as geodetic surveys can be conducted during the snow season, particularly exploiting prominent boulders at the rock glacier surface (see Perruchoud and Delaloye, 2007).

P12L350-356: This is a rather general discussion and the relating pros and cons were not investigated in the study

Thanks for point out this. We included more precise details about the pros and cons. We made clear that the investigated area size, weather conditions and specific UAV regulations are all potential limiting factors for our study area.

P12L360-361: As it is obviously necessary to make SfM photogrammetric studies more comprehensible using protocols and standards, consequently, why don't you present and discuss the complete survey details including planning, theoretical and actual quality estimates, processing settings etc.?

From previous studies, we highlighted two main protocols issues that we have accounted for in the present contribution. The assessment of the stable areas (uncertainty estimation) and surface velocity validation by independent GNSS data were explained for five consecutive periods. We have indicated the most relevant survey and processing details, particularly in sections 2 and 3. Additional details are included on the new supplementary material (UAV surveys settings, DEM assessment and Helmert results).

P12L362-376: This is a repetition of how (only) some others (i.e. not all relating studies) performed UAV-based studies on rock glaciers and as the text passage stresses you performed something what others did not, the character of this text passage is rather contemptuous. Therefore, I suggest to revise or exclude this text passage.

We acknowledge the early work of Dall’Asta et al. (2017) to establish a UAV monitoring protocol on rock glaciers. Their work inspired us to test such approaches on a rapid rock glacier with systematic kinematic measurements since 2004. However, we also have to highlight the main differences with previous researches to show the advances in data interpretation and validation, which are part of the current contribution. However, we recognize that the tone is a bit contemptuous, and we made the changes accordingly.

P12L373: Disagree, it cannot be concluded that companies are not willing to deliver what is necessary, it is rather necessary to adequately negotiate a contract by those interested in processing the data.

We have not concluded such a statement as the original article reads: “Since the UAV flight and the data processing were performed by an external company more detailed information are not available”(Fey and Krainer, 2020, p.4). From this evidence, we stated that Fey and Krainer (2020) did not provide relevant error metrics such as RMSE, mean error or standard deviation error, which could have helped to evaluate the overall quality of the UAV-SfM derived products presented in their article.

P12L374: The term “intrinsic” is distracting, as interior orientation parameters could be meant with this but which are not presented in Table 2 or elsewhere in the manuscript.

We changed to “relevant quality data”. Table 2 expanded with more relevant results.

P12L375: To perform all working steps by oneself is not a guarantee for high-quality results. I fail to see any argument and therefore suggest to revise or exclude this paragraph.

Agreed, we removed this sentence.

P13L395: Add “[…] Tsarmine rock glacier […]” for clarity.

Added.

P14L411-412: Not the kinematic points but their velocities are shown.

Changed “kinematic points” to “velocities values derived from…”

P14L432: Which proposed uncertainty analysis you are relating to here? I cannot find relating data.

We removed “based on the proposed uncertainty analysis.”

P14L434: Disagree, there are no details presented concerning the customized data

acquisition and robustness of processing.

We included more relevant details about UAV data acquisition and processing results. We changed to “The customised UAV data acquisition, and the subsequent data processing workflow delivered spatially distributed kinematics for this destabilized rock glacier during the 2016–2019 period.”

P14L434-440: As UAV-based SfM-photogrammetric studies are already established and as the landform of rock glaciers does not entail any specific methodological adaptations, I fail to understand what is concluded in this text passage and therefore suggest revision.

We changed our argument, and we explained why a destabilized rock glacier (i.e. Tsarmine) entails a specific UAV monitoring strategy.

P15L449: Disagree, the current form of the manuscript does not show all details relevant in SfM-photogrammetric studies.

We removed “SfM techniques”.

P18L564: The list of references needs revision, e.g., the publication in P18L564, which is not mentioned in the text yet.

Thanks for spotting! We added this reference to the manuscript.

Figure 4: In the legend: shouldn’t the category be “1-LoD” instead of “LoD-1”? As already

mentioned, I think it would be favorable to show real vectors instead of categories, which would then also allow to show the displacement-specific LoD values.

Please, see our previous response to the same commentary.

Figure 6: The hillshade could be more low-contrast or so, which would then probably better allow to perceive the lines.

Thanks for this constructive suggestion. We modified the hillshade accordingly.

Figure 7: The categories of elevation differences are partially overlapping and therefore, seem to be wrong, e.g., categories "-4.50 to -3.70" m and "-3.80 to -3.00" m etc.

Thanks for spotting! We modified the categories accordingly.

Table 1: This table could be expanded with values of theoretically achievable precision estimates, which are survey configuration-specific.

We have not found such a type of table together with the "theoretically achievable precision estimates" as an example on relevant publications. Therefore, as previous UAV related publications on ESurf (Clapuyt et al., 2017; Cook and Dietze, 2019; Van Woerkom et al., 2019; Zhang et al., 2019), we only indicated the most relevant UAV surveys parameters.

Table 2: Expand the table by showing both, the RMSEs of GCPs and CPs on the modelled surface as well as the image plane.

We expanded the table with the GCPs and CPs mean and standard deviation errors.

Table 3: Mention the relating confidence interval.

Agreed, we included the confidence limit.

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