Responses to Reviewers' Comments for Manuscript essd-2023-94

Multitemporal characterisation of a proglacial system: a multidisciplinary approach

Addressed Comments for Publication to

Earth System Science Data

by

Elisabetta Corte, Andrea Ajmar, Carlo Camporeale, Alberto Cina, Velio Coviello , Fabio Giulio Tonolo, Alberto Godio, Myrta Maria Macelloni, Stefania Tamea, and Andrea Vergnano

Dear Dr. James Thornton,

We extend our sincerest gratitude to the Editor and the esteemed Reviewers for dedicating their time and expertise to assess the revised version of the manuscript, "Multitemporal characterisation of a proglacial system: a multidisciplinary approach" with manuscript number essd-2023-94. Your insightful comments and constructive feedback have enriched the quality and depth of our work. We are grateful for the opportunity to undertake another revision. The Editor willingness to facilitate further improvements are genuinely appreciated. We approach this revision with enthusiasm and commitment to address all the concerns raised, striving to enhance the clarity, coherence, and impact of our work.

A summary of main modifications and a detailed point-by-point response to the comments from the Editor and Reviewers 1 and 2 are given below.

Sincerely,

Elisabetta Corte, Andrea Ajmar, Carlo Camporeale, Alberto Cina, Velio Coviello , Fabio Giulio Tonolo, Alberto Godio, Myrta Maria Macelloni, Stefania Tamea, and Andrea Vergnano

Note: To enhance the legibility of this response letter, all the editor's and reviewers' comments are typeset in boxes.

Authors' Response to the Editor

General Comments. The revised manuscript has now been re-reviewed by the original referees. Whilst Reviewer #1 is now largely satisfied that the manuscript is suitable for publication, Reviewer #2 still has major concerns about the lack of a dedicated section on data quality assurance / evaluation / uncertainty (especially with regards to the drone data). Therefore, in your next revision, please kindly ensure that thorough details and explanations of the methods are provided throughout, and that a section regarding the quality assurance / evaluation of the UAV data in particular (and other data, as necessary) is included, supported by appropriate and rigorous statistics.

Response: We appreciate the opportunity to address the concerns raised.

In our latest revision, we have diligently incorporated dedicated sections focusing on data quality evaluation, and uncertainty, targeting the aerial data. A thorough methodological section based on a rigorous statistical analysis has been included in section 2.3 (and in a new Annex), while the related detailed results have been integrated and thoroughly discussed in section 3.2 to demonstrate the robustness of our findings. We are committed to ensuring the strength and integrity of our manuscript, and we hope that these revisions meet the expectations outlined. Once again, we appreciate the constructive feedback and the opportunity to enhance our work.

Authors' Response to Reviewer 1

General Comments. I carefully read the authors' response to my comments and looked at the manuscript with the tracked changes. The authors have made a great effort to respond to my comments, and the article has benefited greatly from this work. As I pointed out in my previous paper, I consider this work a very good contribution and it demonstrates the importance of studying the functioning of proglacial margins with a multidisciplinary approach. The results presented here will allow further investigation of the Rutor Glacier and its proglacial margin, and the papers that follow will certainly be of interest to the geomorphological community.

Response: We are grateful for the extremely constructive comments and suggestions provided by Reviewer 1.

The authors agree the previous version of the manuscript benefited greatly from the reviewers' comments, in terms of paper structure, correction of errors, and integration of additional information to streamline the understanding from the reader perspective.

General Comments. The paper is now stronger, more informative, and better structured. However, I still have some concerns about the error assessment in the DSMs, but it may be that I am too picky.

Response: Thank you again for your thorough review of our revised manuscript and for your constructive feedback.

Regarding your concerns about the error assessment in the DSMs, we truly appreciate your diligence in scrutinizing this aspect of our work. We anticipate that a new section focused on the assessment of the error in elevation differences (referred to as DoD in the manuscript) has been added, according to requests from the reviewers and editor.

We are confident that this new version should clarify the quality assessment of the data, especially in terms of evaluation of the DoD (precision and possible systematic errors) and its LoD.

General Comments. The authors present a first DSM assessment in Table 4. They calculate the vertical and horizontal RMS of the GCPs and checkpoints, but it would have been more informative to include the mean error and the standard deviation of error as well (this would allow characterizing systematic and random errors in the DSMs).

Response: Thank you for the comment.

Table 4 (table 5 in the revised manuscript) have been revised accordingly, detailing the mean values and the standard deviations (for the ΔX, ΔY, ΔZ residuals) for both GCP and CP. The outcome of the analysis suggests the absence of meaningful systematic errors.

General Comments. Then, the authors calculate such statistics in Table 5 by using the delta of Z of a number of stable points, however these results are not informative since those stable points are not well scattered across the study zone.

Response: Thank you for the comment.

The area was selected since it is the only large area the authors are confident is stable in the two dates, being the rocky areas around the glacier partially covered by snow. Nevertheless, i) the area has been revised and divided in three smaller polygons to exclude fluvial areas and water bodies and ii) as suggested below by the reviewer, these stable areas have been used only to showcase a possible alternative approach to estimate the LoD of DoD (while the LoD based on CP has been used in Figure 9).

General Comments. By looking at the DoD in Figure 9a, it appears that the DSMs are of good quality, and no apparent systematic deformation is occurring. However, without a deeper error assessment some doubts might arise. Building on my own experience in error assessment in DSMs and DoDs, I would suggest two paths to assess the DSMs.

1. Assess the DSMs by using your independent checkpoints, which are widely distributed across the study area, although their number is low (but this is OK in such extreme and dangerous environments). This analysis would inform if your DSMs are impacted by random errors (std of error) and systematic errors (mean error). In the presence of systematic errors, check if those errors have some spatial structure (e.g., systematic tilt, which I formerly named datum shift), and if any correct it in respect to your checkpoint network (if possible). Once the presence of systematic errors is assessed (and corrected), use the std of error to calculate your Limit of Detection (LoD), either at 68 or 95 confidence limits. In this way, you do not violate the assumption of the error propagation theory which states that the errors in your DSM should be random, independent and gaussian. I suspect that this approach would provide a large LoD, but this is fine.

Response: Thank you for the comment.

We confirm that we adopted this approach, which is now clearly explained in the new ad-hoc section 2.3.1 “DSM Validiton: DoD and LoD estimation”.

General Comments. 2. Assess your reference DSM (i.e., 2021) by using your independent checkpoints, and check for systematic errors. In the presence of systematic errors, check if those errors have some spatial structure (e.g., systematic tilt), and if any correct it in respect to your checkpoint network (if possible). This would inform about the quality of your reference. Then create a new set of checkpoints based on a number of widely distributed stable points (e.g., bed-rock, etc.). Calculate the shift between the reference (2021) and the DSM of 2020, and assess the presence of random and systematic errors. In the presence of systematic deformations such as tilt, correct them. Once the presence of systematic errors is assessed (and corrected), use the std of error of your stable points to calculate the LoD. Both ways are widely accepted, although (1) is probably more robust although your checkpoint population is somehow little. I would suggest discussing this with the editors, and get to know what their expectations in terms of error assessment are. I want to emphasize that my suggestions should not be seen as criticism, but as genuine suggestions to improve the quality of the article, which - as I have already said - in my opinion is already good. I am of the opinion that the authors can respond to my concerns fairly quickly and I don't think another round of review by the referees is needed.

Response: We truly appreciate Reviewer 1's comment, the extremely detailed feedback and the constructive suggestions.

We confirm that they have been exploited to improve the quality of the manuscript. It was an opportunity to further discuss among the authors pros and cons of the two different approaches that can be used to estimate the LoD of DoD. We are confident that this updated version should clarify the quality assessment approaches in terms of evaluation of the DoD (precision and possible systematic errors) and its LoD.

General Comments. I know the challenges of collecting (and processing) this amount of data in these extreme environments, therefore I compliment the authors for having produced and discussed this remarkable dataset.

Response: Thank you for your acknowledgment of the challenges associated with collecting and processing data in extreme environments. We greatly appreciate your recognition of our efforts in producing and discussing the dataset presented in our manuscript.

Comment 1

Line 192: "During the 2021 field activities, a total of 32 artificial photogrammetric markers"

Following Table 2, you deployed 33 markers. Either the text or the table are wrong.

Response: Many thanks for highlighting this source of misunderstanding.

The number of markers has been verified to be 32, which is the precise count. However, the total number of ground control points (GCPs) and checkpoint points (CPs) utilized for images acquired in 2021 amounts to 34. This discrepancy arises because two markers used for the aerial dataset were also leveraged for the UAV dataset, as elaborated upon in the manuscript.

Comment 2

Lines 223 – 225: "Outwash plains, which may have been affected by geomorphological changes (e.g. due to erosion and water deposits) between the time of the surveys, were also considered stable areas."
I do not see the point of using such points if you want to assess the error in your datasets. Outwash plains are extremely unstable as you pointed out. What is the rationale of choosing points within the outwash plain? You bring in this point later in the paragraph, stating that using such points may have worsen your statistics, which again questions the rationale of choosing them. Could you please clarify? How many stable points did you select?

Response: Thank you for pointing out this aspect.

Even if the approach based on CPs is used to estimate the LoD, we do agree with the comment. The stable areas have been refined accordingly, excluding the outwash plains and the water surfaces. Due to the steep slopes and the snow cover, especially for the year 2020, it was not possible to add other stable areas around the glacier (the main reason why the approach based on CP has been used).

Comment 3

Lines 352 – 354: "A standard Structure-from-Motion (SfM) photogrammetric approach was adopted, following a consolidated workflow (i.e. interior and exterior orientation, camera calibration, dense point cloud generation, DSM and orthomosaic generation) using the software Agistoft Metashpe."
I would move this sentence to methods. I am a bit picky here, but could you please clarify what consolidated workflow means. I guess you mean the estimation of the internal/external orientation parameters through the alignment in Metashape, then markers, re-fitting through a bundle adjustment (camera calibration), and so on. Is this the workflow? Perhaps, it would be useful for future readers to specify the parameters you used within the bundle adjustment (e.g., focal length, principal point offset, etc.).

Response: Thank you for your valuable suggestions.

The sentence has been moved to the method section as suggested. As for the consolidated workflow, we have further specified it as follows:

The dataset acquired during the UAV and aerial photogrammetric flights were processed to obtain a 3D model of the terrain and additional cartographic products, i.e. orthophotos and DSMs. A standard Structure-from-Motion (SfM) photogrammetric approach was adopted, following a consolidated workflow using the software Agistoft Metashape, i.e.:

- Image alignment, to estimate interior/exterior (relative) orientation parameters, generating a relative sparse point cloud using feature detection and matching and SfM-based bundle block adjustment with self-calibration.
- Collimation of Ground Control Points (GCPs, not relevant in case of a direct georefencing approach), to re-estimate interior/exterior (absolute) orientation parameters refining the SfM-based bundle block adjustment with self-calibration, to generate a georeferenced sparse point cloud.
- Evaluation of residuals on GPC and Check Point (CP) and iteration of the previous step in case of anomalies in the residuals.

- Generation of a dense point cloud.
- Generation of a Digital Surface Model (with respect to a cartographic plane) and Orthoimagery.

Considering it was only a suggestion, we eventually opted to not include the specific estimated parameters for each photogrammetric dataset. This decision was based on the fact that we already provided several technical details regarding the photogrammetric flights and their general processing. Rather than detailing a long list of the aforementioned estimated parameters, which is not common even in Geomatics manuscripts, we focused, as suggested, on the assessment of the elevation datasets.

Comment 4

Table 4: See my general comments about including the mean error and std of error.

Response: Thank you for the comment.

We confirm that Table 4 (Table 5 in the revised manuscript) has been revised accordingly, detailing the mean values and the standard deviations (for the ΔX, ΔY, ΔZ residuals) for both GCP and CP.

Comment 5

Line 365: "about 0.2 m"

Consider using 20 cm instead of 0.2 m.

Response: Thank you for your suggestion.

The text has been changed to '20 cm' instead of '0.2 m' as recommended.

Comment 6

Lines 377 – 379: "Glacier surface elevation differences were estimated by subtracting 2021 DSM to 2020 one, to quantify ablation and displacement (Table 5)"

I suspect that the reference to Table 5 is wrong here.

Response: Thank you for the comment.

Our apologies for the typo. You are correct, the reference should be to Figure 9. The text has been updated accordingly to reflect this correction.

Comment 7

Lines 380 – 385: The introduction of a LoD is indeed very good, however there is no reference to the works of Brasington et al. (2000) and Lane et al. (2003), both published in ESPL, that are the foundation of the LoD theory in DoDs (in my opinion). Furthermore, you use the RMS of Table 4 that was previously noted as accuracy and here as precision. When calculating the LoD, one should use precision i.e. the standard deviation of error as illustrated in both Brasington et al. (2000) and Lane et al. (2003) papers.

Response: Thanks for referencing these two important manuscripts related to considering the LoD when performing DoD.

They are properly referenced in the current version of the paper as follows:

[1] S. N. Lane, R. M. Westaway, and D. Murray Hicks, "Estimation of erosion and deposition volumes in a large, gravel-bed, braided river using synoptic remote sensing," *Earth Surf. Process. Landf.*, vol. 28, no. 3, pp. 249–271, 2003. DOI: https://doi.org/10.1002/esp.483. eprint: https://onlinelibrary.wiley.com/doi/pdf/10.1002/esp.483.

[2] J. Brasington, B. T. Rumsby, and R. A. McVey, "Monitoring and modelling morphological change in a braided gravel-bed river using high resolution gps-based survey," *Earth Surf. Process. Landf.*, vol. 25, no. 9, pp. 973–990, 2000. DOI: https://doi.org/10.1002/1096-9837(200008)25:9<973::AID-ESP111>3.0.CO;2-Y. eprint: https://onlinelibrary.wiley.com/doi/pdf/10.1002/1096-9837%28200008%2925%3A9%3C973%3A%3AAID-ESP111%3E3.0.CO%3B2-Y.

We also confirm that the terms accuracy and precision are now adopted consistently throughout the paper.

Comment 8

Table 5: I appreciate the choice of including Table 5 in the results, however I think that the way it is presented now does not provide an informative assessment since the stable points cover a fraction of the study area and they are not widely distributed across the study zone. I acknowledge the difficulty of selecting stable points in such unstable environments, but you might find stable zones even in close proximity of the glacier (e.g., bed rock) so that your assessment becomes more informative. Please refer to my general comments at the beginning of this report, and if possible calculate the statistics again to leave no room for any doubt.

Response: Thank you for the comment.

We confirm that the stable areas have been refined (three different polygons have been identified), excluding the outwash plains and the water surfaces. Table 5 (Table 6 in the revised manuscript) was obviously revised accordingly.

Due to the steep slopes and the snow cover, especially for the year 2020, unfortunately it was not possible to add other stable areas around the glacier. Therefore, this analysis was only used to showcase an alternative approach to the one based on CP to estimate the LoD of DoD.

Comment 9

Line 386: "Figure 9 (a) shows the differences between the 2021 and 2020 DSMs adopting a LoD threshold of 95% = 40 cm" Since you used the 95% LoD in your DoD (Figure 9a) I would remove the description of the 68%. I do not see any point of including the calculation of the 68% if you do not use it in further analysis.

Response: Thank you for the comment.

The reference to 68% is now excluded from Figure 9. It is still used in the theoretical description of the approach and while commenting on the results to clarify how the 95% LoD is calculated.

Comment 10

Figure 9a: The DoD is now clearer with the bivariate scale and the LoD. I would suggest using the color white for changes that fall within the LoD or even use the orthomosaic as background (with LoD range in transparent), so that significant changes are easier to see. Furthermore, consider using more classes to be more informative, and perhaps use better contrasting colors. I would also suggest reversing the scale, since now it shows that the glacier termini gained in Z between 2020 and 2021. This results from subtracting the DSM of 2021 from the DSM of 2020 instead of doing the opposite, that is subtracting 2020 from 2021.

Response: Thanks for all the suggestions.

Figure 9 has been thoroughly revised accordingly and we do agree that it is now more informative and easier to interpret (Figure 1).

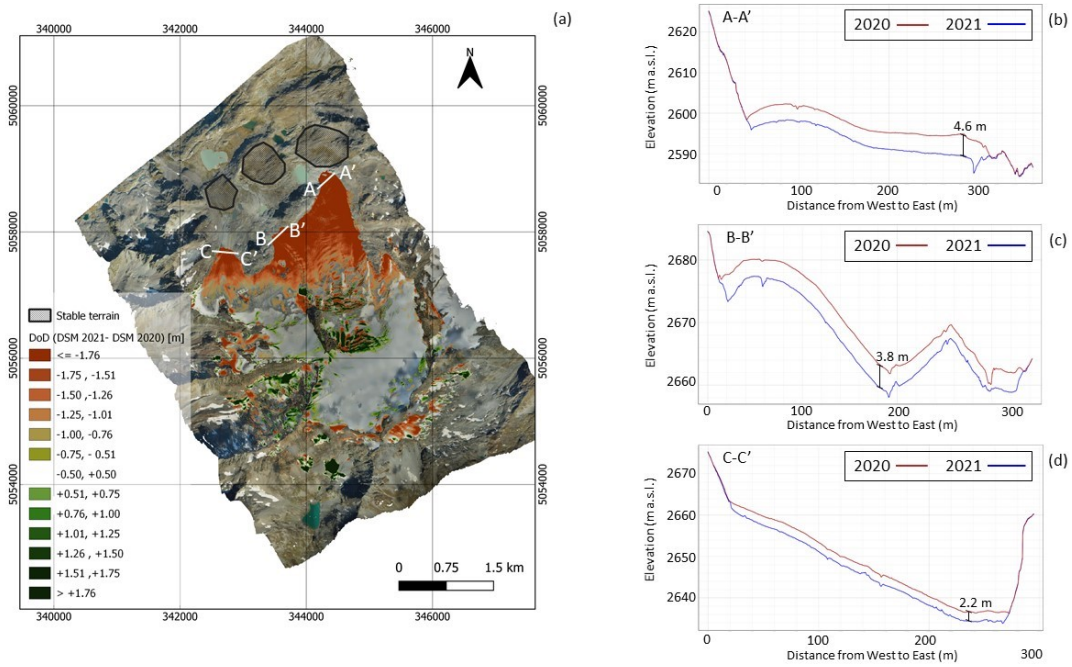

Figure 1: (a) 2021-2020 DoD. The white lines refer to the cross-sections A-A', B-B', and C-C', whose 2020 (red) and 2021 (blue) elevation profiles are shown in panels (b), (c), and (d) respectively.

Authors' Response to Reviewer 2

General Comments. Thank you for giving me the opportunity to review this paper for the second time. I would like to thank the authors because this manuscript has been improved compared to the first version submitted.

Response: We appreciate the reviewer recognised the effort

General Comments. However, even though more details and explanations have been proposed, I think there are still some major methodological weaknesses (especially on the UAV-derived datasets) and underlying problems in this work that, from my point of view, could question the acceptance of the manuscript.

Response: Thank you for the comment.

We are confident that this new version will clarify the concerns that have been raised. Nevertheless, it's important to clarify that our primary focus is on analysing the photogrammetric datasets based on the crewed aerial surveys rather than the uncrewed aerial vehicles (UAV) surveys. The DoD analysis has been carried out on the aerial datasets since the UAV surveys cover only a small portion of the glacier and were conducted solely in 2021, which doesn't align with our multitemporal comparison of 2021 versus 2020. We hope this clarification addresses any concerns you may have had.

General Comments. The manuscript lacks a comprehensive quality assessment section (after the results), especially for the UAV data. This issue was raised during the first round of review, but no real attempt was made by the authors to address it. Without this, and without validation of the data obtained, the usefulness of such a rich dataset is not assured. The quality of the results is also highly dependent on the data acquisition and therefore requires proper planning and description. For example, image overlap, which is a fundamental factor in any photogrammetric approach, is not even mentioned (and this is just one example).

Response: Thank you for the feedback

We have introduced a comprehensive section (2.3.1) dedicated to quality assessment, encompassing both theoretical aspects applicable to UAV and crewed aerial surveys and a results section primarily (3.1) centred on crewed aerial DSM (for the aforementioned reasons). Our emphasis lies in evaluating the quality of Difference of DSMs (DoD), with a specific focus on introducing, discussing, and applying the concept of Limit of Detection (LoD) of DoD.

Additionally, it is important to emphasize that this manuscript adopts a multidisciplinary approach, extending beyond the realm of Geomatics. We have included detailed planning activities related to the UAV surveys to provide a comprehensive understanding. The focus, however, remains on the rigorously assessed final products, as previously mentioned.

General Comments. If the authors want to resubmit their paper, they will of course have to be more careful about the statistics used, especially when quantifying precision, accuracy, systematic errors in general, systematic errors in UVA-based models according to photogrammetric reasoning. More and clearer statements are needed, backed up by solid statistics and numbers. For example, I do not believe in the elevation changes obtained. The LoD analysis (map visualization) is not achieved (Figure 9).

Response: Thank you for the comment.

We are confident that the current revision is properly addressing all the aforementioned topics.