We have replied to the comments in blue.

Review of 'The Glaciers of the Dolomites: last 40 years of melting' By Securo, Andrea and others.

Securo and authors present a multi-decadal estimation of surface elevation change for small glaciers in the Dolomites, Italian Alps. Their geodetic data used in this study consists of aerial photographs, uncrewed aerial vehicles (uavs) and LiDAR data. This data and their analysis indicate high rates of glacier mass loss with the Marmolada Glacier accounting for about ⅔ of the region's volume loss.

Overall, I found this manuscript to be generally well written with methods partially described. The presented data generally supports the conclusions made by the authors. However, like many papers, some clarification of the methods is needed, the English can be substantially improved, and manuscript could be shortened. Below, I outline my major points about the paper and follow these with technical comments.

MAJOR POINTS:

1. Methodology and error analysis needs further description - Given that the authors are using multiple geodetic datasets to calculate volume (and mass) change, the methods section should clearly lay out how the actual uncertainties are propagated to yield the total error budget and final uncertainties. If I understand it correctly, the authors obtained point clouds from all datasets, align these and then use M3C2 for the error analysis. There has been some discussion about the robustness of M3C2 as it assumes planar surfaces and could estimate significance of detectable change (e.g. https://doi.org/10.1016/j.isprsjprs.2021.06.011). I would like to see, for example, how the uncertainty over stable terrain changes due to slope. These are small glaciers in rugged terrain so their slopes will be steep. Also, are there assumptions made for missing data? How is this error source treated?

As reported in the technical comments and suggested by both Referees, we agree that the error analysis needs to be better clarified and improved in a few points, especially regarding error propagation which can be done better. We do not think section 3.3 needs further addition, it is more about the clarity of the text and the approach used, that we will improve in the revised manuscript. We will consider all errors from LiDAR, point clouds alignment and M3C2 distance measurements in the surface elevation change measurements. See also comments from Referee #1.

These small glaciers are in rugged terrain and have high slope values, we agree with the Referee that M3C2 uncertainty should be referred to areas with similar slope of the glaciers. And that's why we have done that. We will write a few more details about it on the methodology. We used verticality (geometrical attribute in CloudCompare) to measure the slope of the glaciers and selected only areas with similar values on stable terrain to calculate the alignment error (E_{AL}). We will implement the revised manuscript with more text and more information on how the uncertainty over stable terrain changes due to slope, as requested. This information specifically will be put in the supplementary materials.

In the case of completely missing data, we have not performed any kind of interpolation. This is mainly because the portions of no data are very small compared to the reconstructed areas and are assumed to be in line with the resulting average. We will specify better this in the reviewed manuscript. Just to be clear, missing data were present only in very small portions of Marmolada 1982-2010 and Antelao 1982-2010 (Fig. 4) and Popera Alto Glacier 2010-2023 (Fig. 5a). An additional part where noise in data was present is Marmolada Principale 2010-2023 (Fig. 5a). The influence of these voids in the final average result for surface elevation change is not relevant.

2. Manuscript needs to be shortened/tightened - I found the 'Introduction and Previous glaciological research' sections to be long and would strongly advocate for merging the 'Previous glaciological section' with the Intro so that the total length of both sections is about ½ of what it currently is. Also, I think some of the tables could be moved to a supplement as most people rarely need to read each line of these tables (they are, however, useful to have if a reader needs them).

While we agree with reviewer 2 that the manuscript appears long in some of its sections, we do not agree with the proposal of shortening sections 1-2 by approximately 50%. In particular, the second chapter that deals with the previous glaciological research in the Dolomites was made because there are no previous recent work dealing with the evolution of these glaciers. We think it can be insightful and potentially useful, especially out of the Italian/Alpine community, to read and find a recap of the previously available glaciological research in the area, which often is not easy to find and is almost entirely written in Italian and only available through grey-literature, local chronicles, and regional reports. Therefore, we propose only some smaller shortenings to the introduction, without changing chapter 2. The only additional paragraph that repeats information between introduction and chapter 2 is from L40 (… In the 1960s, the surface of…) to L46, and will be removed.

One possible alternative option, on which we ask the editor's opinion since Referee #1 did not object to the length of the manuscript, is indeed to move the section on previous glaciological research in the Dolomites (Chapter 2) to Supplementary Materials.

3. Comparison to previous work - The authors do a commendable job compiling datasets for these small glaciers, but they should explicitly show how their results compare to those, for example, of Hugonent and others (2021). The authors can download data for each of their glaciers (since each glacier has an RGI number this should not be a difficult task). How do their estimates and uncertainties compare to Hugonnet? This is an important test of the reliability of Hugonnet for small glaciers in this region (I would posit that perhaps the present study has better estimates for these small glaciers but I simply don't know). It would be good to examine this in some detail.

Hugonnet et al. (2021) work is certainly valuable and precise for larger glaciers, but in this specific study area the resolution is simply not enough to evaluate the surface elevation changes correctly as we did using a higher resolution dataset. As you can see from the figure below (Fig. R1) there is a different order of resolution (pixel size of Hugonnet et al. data is 100 m) between our calculations the one in Hugonnet et al. (2021), due to their much larger effort in terms of total area. Comparing our results with those would therefore not be useful and can be misleading, as shown in the example below.

We will add a sentence in the discussion about this issue as it can be valuable information. Possibly with future studies based on much higher satellite remote sensing imagery (e.g. Airbus Pleiades) this gap can be reduced, and the values can be compared. Furthermore, there is a problem with the difference in the timesteps of the comparison that do not match between our work and Hugonnet et al. (2021).

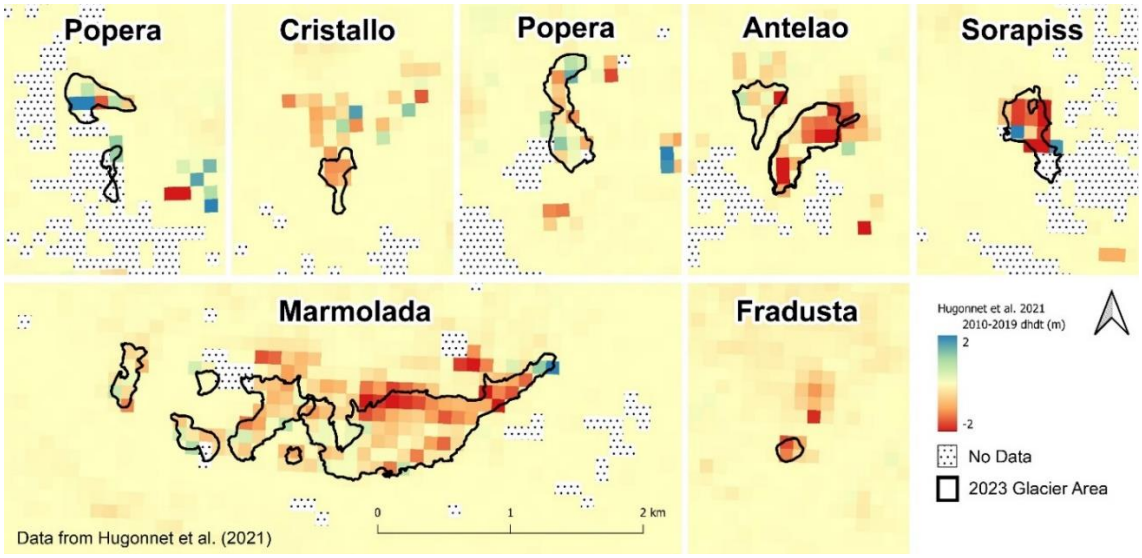

Fig. R1. Example of Hugonnet et al. (2021) surface elevation change dataset from 2010 to 2019 (m) in the Dolomites Glaciers.

4. Manuscript should be shortened. I found the length of the discussion section to be somewhat unbalanced with the length of the results section (the former is longer than the latter). I would recommend shortening the discussion section to balance in light of statements that can be backed up with results shown in the results section. Some of the figures/materials in the discussion seem, to me, to be more results and less discussion.

It should be noticed that the discussion are this long because of the first part regarding climate in the area

We agree regarding the fact that part of Discussion material can be moved to Results and will do that.

Furthermore, section 5.1 dealing with climate in the Dolomites region could be of less relevance considering the aims of the paper compared to 5.2, where the results are compared to WGMS reference glaciers. We will try to shorten all the three sections leaving more relevant parts only.

We can move Fig. 8 and eventually Fig. 9 to Results.

5. Terminology needs clarification - I would recommend that the authors consult the glossary for the standard definitions used in glacier mass balance (https://wgms.ch/downloads/Cogley_etal_2011.pdf) . There are multiple appearances of mass balance when the authors are referring to 'geodetic balance'.

We agree with reviewer 2 on this point, especially regarding the term "geodetic balance".

TECHNICAL COMMENTS:

Abstract:

'Use lowercase 'Alpine' unless it starts a sentence.

Pg 1, Line 3: This sentence isn't technically correct in light of Worldview or Pleiades (very high resolution)

Pg 1, line 11: 'between used for two items, among for more than two'. Also use of 'amplitude' is vague

Pg1, line 13: replace 'areal reductions' with 'area loss'

Pg1, lines 13-15 - This sentence is out of place and likely not needed

P1, line 18: 'with greatest emphasis on regions of the world' - unclear what authors are referring to here.

We agree with Reviewer on the proposed corrections for Pg 1 and changes of the sentences of introduction

We will remove the sentence of L 13-15

Regarding L18 we mean that regions like e.g. the European Alps, and the Arctic global warming hotspots and here increased ice losses rates are reported. We can reformulate for more clarity as: "Glaciers worldwide have been losing mass at alarming rates over the past decades (Zemp et al., 2019; Hugonnet et al., 2021). This is particularly evident in regions where warming is occurring at a faster rate than the global average (Rantanen et al., 2022; ICCI, 2022)."

Pg 2, lines 25-33 - Authors could easily jettison this section to shorten first two sections that need to be put on a diet.

Agree on reduction despite this being the first paper dealing with this study area makes it hard to reach the proposed shortage of 50%. We think, as written in the general comments, that an overview of the previous glaciological research in the Dolomites is useful. Regarding these specific L25-33 we think they introduce the Dolomites region (to eventual people that don't know it) and specify why it's an important area. We will shorten the paragraph a bit, reducing it to 2 sentences.

111 Pg 2, line 50 (and throughout) - ‘unmanned’ is an outdated term these days. Typical use is ‘uncrewed aerial
112 vehicle’

113 Will change the terminology throughout the text

114 pg.3 , line 63. Unclear what authors mean with ‘active’ - deformation, ice flow?

115 For active we mean that these have been classified as mountain glaciers in the latest available Italian glacier
116 inventory (Smiraglia et al., 2015). We will specify it better in the text

117 Section 2 - Shorten this section

118 See general comments above on Section 2

119 Figure 1. The figure could use a little bit of work. The inset (upper left) needs at least some lat/lon coordinates
120 for a reader not certain where the Dolomites are. I was initially confused with the color of the glaciers and the
121 colors of the geodetic datasets (numbers). Maybe change the color of glaciers to avoid confusion? (a) - replace
122 ‘position’ with ‘location’

123 We will improve the figure readability, and we will add coordinates and adjust the colors/labels as proposed

124 Pg. 6, lines 115-118. Were these photos not available as photogrammetric scans? Also, it’s too bad that the
125 internal orientation (if available) information isn’t used as that might help reduce overall error budget.

126 Unfortunately, the photos used were not available as photogrammetric scans and internal orientation was not
127 available either. These problems are also evidenced in the discussion. We will add in the sentence that also
128 internal orientation was not available

129 Pg, 6, lines 115-126. I had a hard time understanding how GCPs were collected and how they were used. This
130 section should be revised to make it clear exactly what was completed and for which datasets.

131 We agree this should be clarified better in the text and we will improve it. Perhaps Fig. R2 (see below) that will
132 be put in the supplementary materials could be helpful for understanding the methodological steps more easily.

133 Ground Control Points have all been taken from 2010 LiDAR dataset, which I the best available so far, and used
134 for all SfM processing. The entire procedure of retrieval of GCP was done in CloudCompare, while GCPs
135 coordinates have been used in Metashape during SfM processing.

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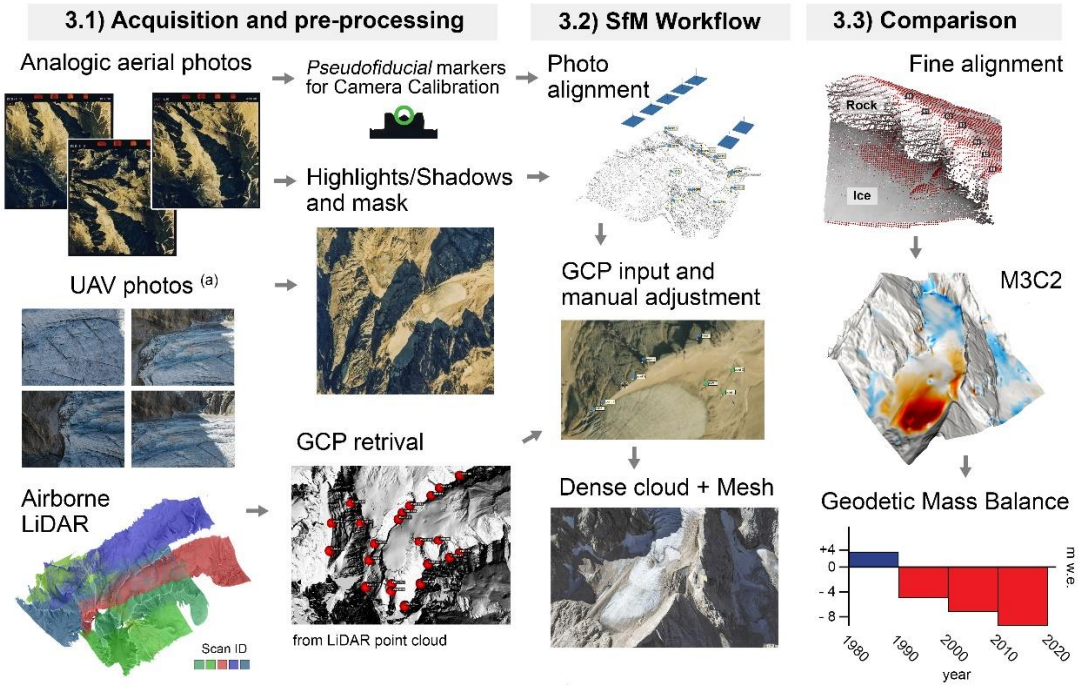

Figure R2. Summary of the data processing proposed to be integrated in the Supplementary Materials.

Pg. 8, lines 153-154. I'm surprised that there were only small areas of voids. Was hypsometric interpolation attempted?

The areas of void were very small without hypsometric interpolation, as stated in the method we used Metashape meshes instead of dense point clouds when the voids were too big to fill. Metashape in this processing does a default linear interpolation and works only within a certain radius depending on the resolution of the point clouds. From our visual inspection this does not introduce any change in the dataset and works as a simple linear interpolation

We think that the areas of voids are this small because photos have a lot of details and are not shot from very high altitudes, despite not having the photogrammetric scans. The areas of the glaciers with snow featureless pixels or are not that impacting in the study area, at least not in these dataset

Pg 11, lines 210. Area change. How were areas of the glaciers digitized? Any uncertainty in area change? Unless I missed it, planimetric mapping is not described in methods.

Areas were manually digitized but we can better explain the procedure especially in regard to the role of debris in the area evaluation. For this reason, we propose to improve the current paper adding the presence of debris in 2023 areas to current Fig. 3 to let the reader know where the glacier areas could be susceptible to major uncertainties due to the debris cover

Pg. 11, lines 219: Unclear what 'topographic bounding' is. Surrounded by rugged terrain?

Yes, that's what we meant; we will specify better in the text

Pg. 11, lines 228-229: Uncertainties needed for these estimates.

Agree, see previous comments on the uncertainties improvement

Pg. 13 lines 215: Terminology needs to be changed to include term 'geodetic'

Agree, see answer to general comments 2 and 4, above

Figures 3, 4 - Generally well drafted, but uncertainties would be useful.

Uncertainty for area are harder to quantify. As stated above, we propose to implement the area figure with debris cover but without quantifying the uncertainty of this, lacking geophysical data. This problem is already specified in the discussion.

Uncertainty for surface elevation change (Fig. 4-5) are shown for each comparison in the bottom right corner (grey).

Tables 3, 4- I would recommend moving these perhaps to a supplement. Also it would be good to have a summary line for weighted mean (table 3). Does table 4's all glaciers line imply this is a weighted mean (by area)?

We agree with the Referee, and we suggest to move Table 3 to the Supplementary Materials. Table 4 in our opinion should stay in the manuscript.

Figure 5 - I think this is a well drafted figure, but I'm not fond of the color bar. It really should be a standard diverging color (red to blue). The dark ends for both red, blue are problematic for diverging data. Sorapiss Glacier's mid elevation I presume is debris covered? I would explain before it is brought up in the discussion. As stated in the major comments, I think some of the discussion and plots should be moved to results section.

The color bar is made with darker ends to improve the spatial visualization of largest changes, which would otherwise appear flat and not be distinguishable in the maps (see Fig. R3). The same color scale is used also in the previous Fig. 4 for the same reason. We have done some tests and can report here an example for Marmolada to show what we mean. Dark ends can be problematic, but they are still distinguishable especially in this case where the positive "blue" extreme (+ 20m) is not present on the map.

Regarding the debris cover, we think improving Fig. 3 with the current presence of debris cover in 2023 for all glaciers would help the overview of area change and current situation of the glaciers. Will produce a revised version of the Figure including debris cover. This should clarify also Sorapiss current debris coverage (visible also in Fig. 8b).

Regarding the Figures in the Discussion-Results sections, we think Fig. 8 can be moved to Results while Fig. 9 is more a subjective choice.

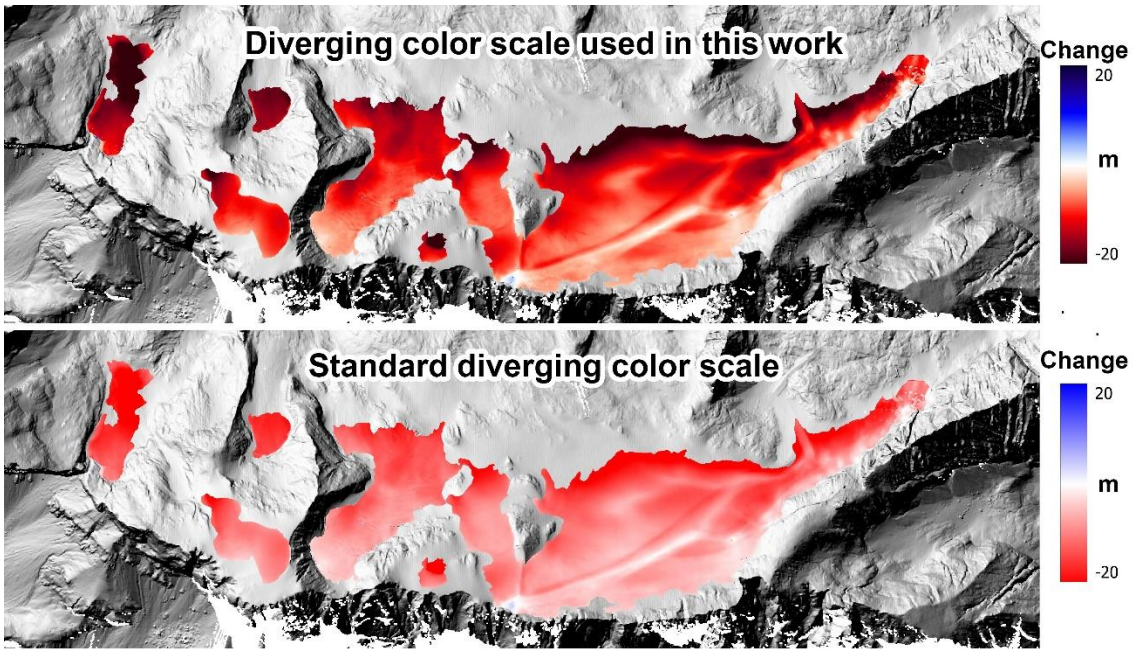

Fig. R3. Example of the chosen diverging color scale with darker colors at the extremes and the standard diverging color scale.

Figure 7. Not certain what color bars on top of the graph refers to. WGMS trend is the dashed line? It's not evidently clear to me.

The colors on the top graph pf the bar refer to Reference Glaciers (gray) and to the Dolomites glaciers, following the color scale they have on the plot. We think it's clear but to avoid confusion will specify this in the figure caption. WGMS average is the only text in black and the only line in black, looks clear to us but will use a thicker black line without dashes to improve visualization.

Pg 16, Results - How do these results compare to Hugonnet? Add those values perhaps to one of your tables with both estimates (yours and Hugonnet) including uncertainties.

See the general comment above and related figure example on why we think this comparison is not that useful considering the small size of the glaciers of the Dolomites.