REVIEW OF THE MANUSCRIPT ENTITLED:

Combination of UAV and terrestrial photogrammetry to assess rapid glacier evolution and conditions of glacier hazards

By D. Fugazza et al.

General comments

In this paper, Fugazza et al. present the results of photogrammetric surveys carried out on the lower ablation area of the Forni Glacier in 2014 and 2016. The surveys were performed using photographs taken from the ground and from unmanned aerial vehicles, and their results have been used for intercomparisons aimed at evaluating the accuracy of used techniques, for quantification of glacier changes across 9 years, and for the identification of hazards deriving from the current rapid shrinking of the glacier.

The work is interesting and potentially useful as a baseline for future developments of these remote sensing techniques. However, there are several points of the paper that require formal and substantial improvements. In particular:

i) there are several claims of uniqueness, originality, lack of previous work and of scientific knowledge, which are untrue and deserve a careful literature review by the authors. Consequently, the results are not unique, and have to be critically assessed in light of previous findings by other authors

ii) pros and cons of the tested methods require a thorough discussion, as well as their repeatability (e.g. the peculiar cloud cover conditions) and generalizability, costs, logistics and alternative solutions. A very weak point that requires discussion, in my opinion, is the limited areal extent of the surveyed zone, preventing possible applications aimed, for example, at the estimation of the glacier-wide geodetic mass balance. In addition, such a large glacier normally has several other hazardous areas along mountaineers' tracks, which cannot be comprehensively surveyed using the proposed approach. Which improvements (or alternative methods) would be required?

The paper is rather long and contains many descriptive sentences, too generic periods, scholastic explanations. In particular the Results section is difficult to read and wordy. My suggestion is to really focus on the results of the investigations, strongly shortening this part, and moving any (relevant) consideration in the Discussion section.

A careful English proof reading is also required to improve the readability and to make the manuscript appealing. The authors should also consider reducing the self-citations, which currently contribute to one third of the reference list.

In my opinion, the manuscript requires a major revision before being considered for publication in NHESS. A more complete description of the required formal and substantial improvements is reported in the following section.

Detailed comments

L30: snow cover thickness and/or duration?

L39: changes of glacier and…

L40: anthropic activities

L39-63: some of the mentioned processes are not strictly depending or worsened by climate variations. They are instead typical of the glacial, periglacial and paraglacial environments. I suggest rewriting this part to clarify which processes are typical and which ones are worsened by the current climatic phase. I also suggest mentioning debris flows

L68: please add some references concerning glacier change detection using DEMs

L72: remove indeed

L92: please replace battery support with e.g. battery life or charge duration

L110: must be completely observable from….

L114-118: previous work reporting comparison between photogrammetry and LiDAR or more traditional survey techniques on glaciers actually exists. Please, see for example Kaufmann and Ladstädter (2008), Piermattei et al., (2015 and 2016), Kaufmann and Seier (2016), Westoby et al., (2016), Seier et al., (2017), and contributions in the book from Pellikka and Gareth Rees (2010).

Kaufmann, V. and Seier, G., 2016. LONG-TERM MONITORING OF GLACIER CHANGE AT GÖSSNITZKEES (AUSTRIA) USING TERRESTRIAL PHOTOGRAMMETRY. International Archives of the Photogrammetry, Remote Sensing & Spatial Information Sciences, 41.

Kaufmann, V. and Ladstädter, R., 2008. Application of terrestrial photogrammetry for glacier monitoring in Alpine environments. Ele, 2700(2800), p.2900.

Petri Pellikka, W. Gareth Rees. Remote Sensing of Glaciers, Taylor & Francis Group, London, UK (2010)

Seier, G., Kellerer-Pirklbauer, A., Wecht, M., Hirschmann, S., Kaufmann, V., Lieb, G.K. and Sulzer, W., 2017. UAS-Based Change Detection of the Glacial and Proglacial Transition Zone at Pasterze Glacier, Austria. Remote Sensing, 9(6), p.549.

Westoby, M.J., Dunning, S.A., Hein, A.S., Marrero, S.M. and Sugden, D.E., 2016. Interannual surface evolution of an Antarctic blue-ice moraine using multi-temporal DEMs. Earth Surface Dynamics, 4(2), p.515.

L119: protected in which sense?

L121: two distinct aircrafts

L124: please provide a length for short and long time scales

L125-126: please try to improve this sentence, which is too long

L129: confluencing ice tongues?

L134: retreating rate? Shrinking rate?

L135-136: AWS1 Forni was not the first. There is at least one precedent, i.e. the AWS that has been operated on the Careser Glacier (10 km from Forni) from 1989 to 1998 (Rossi and Stojkovich, 1992; Novo and Rossi, 1998).

Rossi G. C. and Stojkovic, P. (1992) Scientific programmes for meteo-climatic and environmental observations in Alpine glacial areas. Presented at First Ev-K2-CNR Scientific Conf. on Scientific and Technological Research at High Altitude and Cold Regions. Milano, 10–11 April 1992.

Novo, A. and Rossi, G.C., 1998. A four-year record (1990–94) of snow chemistry at two glacier fields in the Italian Alps (Careser, 3090m; Colle Vincent, 4086m). Atmospheric Environment, 32(23), pp.4061-4073.

L142-155: I suggest shortening these points and possibly moving some of the concepts and references in the discussion section (if relevant). In section 1.2 all references are self-references. I wonder how much they are functional to this work.

L159: the meaning of reconstructing is not fully clear, I suggest writing explicitly that it is a topographic survey (also in the following)

L169-171: it is not clear why morning hours should be preferable to the central hours of the day. Please state it clearly

L171-173: it is not clear how low cloud cover inhibits direct solar radiation, it should be the contrary. Moreover, what to the authors mean with low cloud cover? Which fraction of the sky covered? By which type of clouds? And why should the direct solar radiation be avoided? How often can these ideal meteorological conditions be met in the alpine environment during summer? Which is the impact of ice ablation during the three-day survey period? Is there any measurement? In my opinion this information is of relevance for future applications and repeatability of the proposed survey techniques.

L177: potentially causing motion blur to the acquired imagery

L179: at a relatively low altitude of 50 m

L216: please consider replacing coordinate frame with coordinate system (also in the following)

L218: same days of the UAV survey?

L225: consider replacing pipeline with workflow

L245: evolves rapidly, or is rapidly evolving

L246: it is unclear why a complex shape and a rapid evolution make the glacier terminus not suitable for quantitative evaluation of the ice bulk? What do the authors mean with this sentence?

L249: including GCP surveying

L251: same days of the UAV survey?

L268: remove the purpose of

L274: how much stable has to be considered a GCP placed at the glacier surface, close to the terminus and for more than one day during the ablation season? Please discuss this issue

L284-285: with which consequences? Fewer than planned surveyed GCPs? Why not using post-processing correction?

L287: in my opinion it should be better arranging the methods in chronological order

L294-301: here is the explanation why early morning is preferable. Another reason for moving this part above the 2016 survey, according to me. What about cast shadows? Are they a further reason to avoid direct solar radiation and/or surveys carried out later in the day, with the possible occurrence of shadows from scattered cumulus clouds? What is the repeatability of this method if applied to east-exposed glaciers?

L332-340: this part has some repetitions from previous paragraphs. Please rephrase

L358: what about spatial trends in elevation differences? Are they inexistent, negligible or not taken into account?

L361-390: this part is too long and does not present results

L391-409: why not using the entire overlapping area? The area surveyed by terrestrial photogrammetry is already small, therefore I do not understand why the authors decided to perform a (subjective) sub-sampling taking very small areas, which on the other hand are very similar to each other. I suggest comparing the entire area in common among the different surveys, and then analyse separately glacier areas with peculiar characteristics

L410-411: please avoid describing in the text what figures and table present (their caption already does it)

L414-415: a more dense point cloud? The term consistent has a too general meaning (and here is misleading)

L415-417: the flexibility of terrestrial photogrammetry, compared to UAV photogrammetry, is questionable

L419-432: please summarize this part and avoid too scholastic sentence such as the first. I suggest simply stating which metrics are used and which results they provided

L437-443: here the authors skip to the concept of point cloud completeness, introducing a heuristic evaluation method that is not fully described. Afterwards, they resume with point density. My suggestion is to rearrange paragraphs in a more logic order

L445: please remove the sentence: The following general considerations can be made (and other analogous sentences in the manuscript).

L448: comparable or three times smaller?

L454-455: this is expected and confirmation of findings from previous works. Please add references

L455-458: which are the practical consequences? Which method for which application? Please discuss in the appropriate section

L469: this is another highly-expected result. However this is a very small area, compared to the entire tongue (or the entire glacier). Further considerations are required, e.g. in the discussion

L477: similar point densities were found

L484: the former are more suitable….

L486: please see comment L445. These sentences make the paper boring and difficult to read

L491-492: the suitability of a survey technique depends largely on the final aims of the survey. LiDAR DEMs obtained with point densities as low as 2 pt/m2 are enough for glacier-wide and/or regional scale glacier change assessments, for example. Please comment on that in the discussion

L493: please see comment L410

L496: please replace here and elsewhere “exposed upward” with horizontal, or sub-horizontal, or moderately sloping (maybe adding slope thresholds for improved understanding).

L516: the sections 3.1 and 3.1.1 are very long and can be highly summarized, presenting just the results and moving further considerations in the discussion section.

L518-523: I suggest removing or strongly summarizing this part

L523: do the authors have ablation measurements (or estimates) during the survey period? What is the impact of glacier ablation in calculations?

L540: retained or based on some metrics/methodological constrains?

L576: ΔDEM could be replaced by the more commonly-used dem of difference (DOD)

L575-579: this part is poorly written and hardly readable/understandable. Please reformulate

L579-581: this part is obvious and redundant

L593: please complete numbers with minus sign and measurement units

L594: the eastern part of the ablation tongue

L603-610: I am not fully convinced that the paper deserves section 3.3. My suggestion is to remove it and move concepts above, when the authors write about the complementarity of the two survey techniques.

L613-622: the authors try to validate their geodetic mass balance estimates in the lower glacier tongue, using specific mass balance estimations at the surface, for one point (whose location is not reported). Their approach is not correct, because they are comparing single-point vs. mean areal estimates, which can be highly different in the study area given the high lateral gradients in mass balance and elevation changes (Fig. 11), likely attributable to debris cover and differential ablation. Moreover, local geodetic and glaciological mass balance estimates seldom match on glaciers, because the surface elevation change is the result of a complex combination of surface, internal and basal mass exchanges, and of ice dynamics. In particular, vertical displacements (emergence velocity) have to be quantified for local comparisons of the two methods (see for example Fischer, 2011; Sold et al., 2013).

Fischer, A., 2011. Comparison of direct and geodetic mass balances on a multi-annual time scale. The Cryosphere, 5(1), p.107.

Sold, L., Huss, M., Hoelzle, M., Andereggen, H., Joerg, P.C. and Zemp, M., 2013. Methodological approaches to infer end-of-winter snow distribution on alpine glaciers. Journal of Glaciology, 59(218), pp.1047-1059.

L612-645: in my opinion this is not discussion, but mostly a presentation of results. Here the authors should discuss the accuracy of their results, the problematics in data collection and processing, the generalizability and the added values of the employed techniques. In particular, they should provide a discussion of the pros and cons of the proposed approaches, a comparison of their results with the existing literature, and critical evaluation of local-scale high-resolution surveys vs. glacier-wide surveys, which are required for geodetic mass balance estimates and comprehensive glacier hazard mapping. Which of the used methods has the highest potential for monitoring rapid glacier evolution and deriving hazards? Is there a method that has the potential to become a standard in glacier monitoring strategies, according to the authors? With which improvements/adjustments?

L687: I wonder if there is a more quantitative approach to be used here (such as DOD) to better exploit the new technologies. All the surface features described in this section are so large to be clearly visible by quick field observations and the tourist path can be easily changed accordingly. In my opinion the advantage of AUV and/or terrestrial photogrammetry lies in the possibility of automatically mapping and measuring these features from the DOD. Therefore, I suggest to add this quantitative assessments, starting from elevation changes as displayed in Fig. 11, where the collapse structures are evident.

L695: increased rate of surface lowering (not necessarily equal to surface ablation).

References: the reference list is rather long and, notably, one third of the references are self-citations. Please check if all these references are pertinent and functional to the paper

Table 2: please provide explanation for GSD

Table 3: I guess that the last column shows elevation differences “with” co-registration shifts

Table 8: I suggest showing in a figure the extent and location of the common reference area

Figure 5: I think that a) and b) are inverted

Figure 7, 12 and 14: these figures can be merged in a single image