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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Structural and climate drivers of the historic Masiere di Vedana rock avalanche (Belluno Dolomites, NE Italy)" *by* Sandro Rossato et al.

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Dear referees, On behalf of myself and all of the co-authors, I would thank you sincerely for your work. We think your comments really improved the quality of the paper. We decided to answer to both of you in the same reply, as a couple of your remarks were similar. We accepted all of the grammatical/technical corrections. Here below is a list of your major comments with our replies and the changes we made to the text (when needed).

REFEREE #1:

Printer-friendly version

Discussion paper

Comment 1: Expand the introduction, describing the basic features of rock avalanches in stratigraphic, morphological and depositional terms and related bibliography (Varnes, 1978, Hungr et al., 2001). See for example: G. Bianchi Fasani, E. Di Luzio, C. Esposito, S.G. Evans, G. Scarascia Mugnozza, Quaternary, catastrophic rock avalanches in the Central Apennines (Italy): Relationships with inherited tectonic features, gravity driven deformations and the geodynamic frame, Geomorphology, Volume 211, 2014, Pages 22-42, https://doi.org/10.1016/j.geomorph.2013.12.027. E Di Luzio, M Saroli, C Esposito, G Bianchi-Fasani, G.P Cavinato, G Scarascia-Mugnozza, Influence of structural framework on mountain slope deformation in the Maiella anticline (Central Apennines, Italy), Geomorphology, Volume 60, Issues 3–4, 2004, Pages 417-432, https://doi.org/10.1016/j.geomorph.2003.10.004.

Reply: We agree on the lack of information you highlighted. We added a paragraph in the introduction, at line 53 of the original manuscript.

Text added in the manuscript: Amongst all types of landslides, rock avalanches are particularly relevant, being both difficult to predict (Hungr, 2006) and representing a very high risk for the population living in mountain areas (Guzzetti, 2000; Hungr, 2004; Geertsema et al., 2006; Evans et al., 2007; Sosio et al., 2008; Cui et al., 2011; Hermanns and Longva, 2012; Mitchell et al., 2019). The moving masses are composed of dry debris, that in subaerial settings range from about 0.5 to more than 10,000 Mm3 (Crosta et al., 2007). The initial phase, rockfall or rockslide, evolves into a flow-like movement of crumbling rock debris, sized from clay/silt up to decametric boulders, which can travel for several kilometres, even uphill, and overcome obstacles up to some hundred meters high (e.g., Hungr et al., 2001; Mangeney et al., 2010; Bowman et al., 2012). Rock avalanche deposits are characterized by inverse grading of the sediment, with large blocks dominating the carapace, the inclusion of path material and, in some cases, preservation of the stratigraphic sequence (Hewitt, 2002; Strom, 2006).

Comment 2: Probably in contrast with other reviewers (Authors write as Information about previous submission: “A former version of this paper has been submitted to

Geomorphology: one out of two referees was negative about the potential impact of the article on the community. We revised completely the article, introducing comparisons with other areas and events. Moreover, we enlarged the section regarding the impact of such events on human beings and lives.", I believe that all considerations relating to the hazard and associated risk are superfluous. These assessments require a rigorous approach, advanced numerical modeling techniques that are not present in the work. So, in the absence of such data, absolute considerations on the local hazard and even more regional, should be avoided (see lines 17-18; 257, 312). Undoubtedly, the work presented lays the indispensable knowledge for a future evaluation of these aspects, but hazard evaluation is not the aim of this paper. In addition, the hazard assessment of the slope refers to an unpublished technical report in Italian, of which the methodology used is not known. Therefore, please review the parts of the work in which the hazard is referred to; in particular I would avoid to expand the scale referring to the surrounding peaks (see Fig. 9 and text) as the structural, geological conditions may vary from place to place influencing the mode of failure of the slopes themselves.

Reply: You are right: hazard is a matter to not discuss in-depth in this paper. Nonetheless, we think that the structural setting is a fundamental part of the research and a discussion about the extension of such a deformation can be inserted. We modified the text at the lines you suggested to remove specific comments about potential hazard, especially in the abstract and introduction. The structural part in section 5.3 has been re-arranged to better fit with previous paragraphs (as requested by Referee #2). Lines 295-309 of the original manuscript have been replaced by the text here below.

Text added in the manuscript: Where a structural setting similar to that at Mt. Peron is present, the occurrence of huge landslide events deserves evaluation. The Belluno Dolomites experienced a long deformation history since the Miocene, related to regional-scale stress connected to the counter-clockwise rotation of the Adria plate, indented with the Alpine orogeny (Márton et al., 2003; D'Agostino et al., 2008). Such forces overturned the bedding, formed the thrusts and backthrusts (WSW-ENE ori-

ented), the two conjugate fracture sets (NW-SE oriented) and led to re-activation of the Jurassic faults (N-S oriented). The belt characterized by these deformations, where the Mt. Peron is located, lies between the Belluno thrust and the Val Carpenada - Val di Vido - Val Madonuta backthrust (Fig. 1), and extends from the Piave Valley to the east to the Caorame Valley to the west (Bosellini et al., 1981; Masetti and Bianchin, 1987; Bigi et al., 1990; Costa et al., 1996; Fig. 1). The Belluno Dolomites are also seismically prone and active tectonics has been suggested to contribute to intensification of slope instability registered in this sector during the last 1500 yr (Galadini et al., 2005). Moreover, the area is densely inhabited (Fig. 9), and some artificial lakes are present (e.g., Lake Mis; Fig.3). A massive rock failure that would hit such lakes or damage the dams may pose a serious threat, possibly triggering a tsunami, as happened for instance at Vajont (e.g., Ward and Day, 2011).

Comment 3: Title: in accordance with the above, I suggest a modification of the title including the word dating (this represents an important contribute to the paper); for example "Structural and geomorphological characterization and dating of the historic Masiere di Vedana rock avalanche (Belluno Dolomites, NE Italy)".

Reply: We really liked your suggestion and replaced the title accordingly. As proposed, the new title is: “Structural and geomorphological characterization and dating of the historic Masiere di Vedana rock avalanche (Belluno Dolomites, NE Italy)”

Comment 4: The term "toma/s" is present 7 times in the text. As the word belongs to a local lexicon/peculiar landforms, a detailed description is necessary (line 154) (see e.g. description in cited ref. More and Wolkersdorfer, 2019).

Reply: You are right: “toma” is a term that deserves a specific introduction. We added a brief description of the main characteristics of toma, including where they are found and their typical internal structure. The discussion on their genesis remains in the discussion part (section 5.3) as at line 154 we are only presenting the results. New text has been inserted at original line 156.

Text added in the manuscript: Such morphological structures are “toma” (Turnau, 1906; Abele, 1974; More and Wolkersdorfer, 2019). They are found in association with some large rockslides and are mainly made of landslide material, in many cases showing a gradation from very comminuted fragments in the outer part to less fractured material at the core (cf. von Poschinger and Ruegg, 2012; More and Wolkersdorfer, 2019).

Comment 5: You suggested to add at line 243 the term “bedrock” after “Pleistocene conglomerate”. We do not agree with this suggestion: “conglomerate” is a term that intrinsically implies that the sediment is lithified but the term “bedrock” would refer to the underlying rock formations limestones, i.e. older than Quaternary. To avoid confusion, we prefer to not use the word “bedrock” for these conglomerates.

REFEREE #2:

Comment 1: Lines 6-9: detailed names of the geologic formations are out of place in the abstract; better to stress that the stratigraphic sequence of the crown area is mimicked in the deposit area.

Reply: We agree on your remark, evidenced also by referee #1. We replace the original lines 6-9 with the following.

Text added in the manuscript: The stratigraphic sequence is preserved in the deposit with the formations represented in the boulders becoming younger with distance from the source area.

Comment 2: Lines 128-130: is the thickness estimation from literature or an original analysis? Provide references, or explain how you reach that figures.

Reply: You are right, information is lacking. The thickness estimations have been made by us, by means of open sections and outcrops. A clarification has been inserted in the text. In this part of the text, we added a general reference to “open sections”. Specific considerations and descriptions have been inserted in the following paragraphs.

Text added in the manuscript: Line 147: “VB2 boulder (Calcari Grigi) gave an age of

1.49 ± 0.26 ka; it lies on top of ∼10 m thick sequence of rock avalanche deposits." Line 148: "This sediment is >30 m thick and is composed of sub-rounded clasts (up to 20 cm in length), some of them striated, supported by a silty clay matrix." Line 159: "Cenozoic lithologies crop out at the base of this incision, the rock avalanche deposit being ∼15 m thick." Line 165: ". . .southern boundary of the Masiere (white asterisk in Fig. 6), where the deposit is only ∼5 m thick."

Comment 3: Lines 209-213: identifying if the Masiere di Vedana rock avalanche is related to a single or multiple failure has huge consequences for hazard assessment purposes. The authors claim that "a single rock avalanche occurred in historical time" I think that such a strong statement is not fully supported by available data, because the occurrence of multiple failures during – geologically speaking – short time (e.g., several decades) cannot be ruled out. Please better support your statement or consider toleave open the possibility for multiple failures.

Reply: Also Reviewer #1 asked for more details on the morphology of the crown, to better sustain the "single-event" scenario. We added a statement on the absence of secondary scarps in the niche, that we think support such hypothesis. Moreover, we added a statement comparing our event's data with (H/L)vs(Volume) plots of other rock avalanches (e.g., Aaron, J., McDougall, S. (2019) Rock avalanche mobility: The role of path material. Engineering Geology, 257, DOI: 10.1016/j.enggeo.2019.05.003). The Masiere di Vedana event falls in the domain of extremely mobile rock avalanches. Given that the occurrence of multiple events would imply a reduction of the volume, the event that reached the distal sector would have been even more exceptional. In our opinion, this consideration supports the single-event scenario.

Text added in the manuscript: Line 210: "Moreover, the volume and H/L ratio of the landslide (see Section 5.2 for further discussion), together with the morphology of the scarp and the absence of secondary scarps, indicates a single huge catastrophic event." Line 223: "The H/L ration of ∼0.2 (apparent friction angle of 11°) and comparison with empirical plots of H/L vs. volume (e.g., Aaron and McDougall, 2019) mark the

Mt. Peron event as extremely mobile."

Comment 4: Line 257 – Section 5.3. I acknowledge the authors' effort in looking for driving factors. As they point out, this is a challenging issue, and rock avalanches may not need a triggering event at all. The discussion on a possible seismic trigger looks a weak point of the manuscript: several Mw > 5 earthquakes are documented in times more recent than the avalanche. None of those events triggered rock avalanches with size comparable to the Masiere di Vedana one. This fact may be read as an element against the seismic trigger. Line 316: "a single event" see comment above at line 209.

Reply: We discuss seismic activity to have acted not only as a potential trigger but also more effective as a driver for collapsing (sensu Gischig et al., 2016). We changed the text to make clearer the role of earthquakes in rock damaging and producing rock fatigue. The emphasis on their potential as triggers has been also reduced. The text has been changed slightly in many places. The most important change has been made at lines 289-291 and is reported below.

Text added in the manuscript: The most important effect of the frequent seismic activity, even of markedly different magnitude, is the progressive increase in the rock fatigue, with the formation of failure surfaces and the removal of rock bridges and roughness on discontinuity planes (Friedmann et al., 2003; Brideau et al., 2009; Parker et al., 2013; Stead and Eberhardt, 2013; Preisig et al., 2015; Gischig et al., 2016).

Comment5: Lines 295-303: this paragraph is not connected to the rest of the text.

Reply: You are right: a connection is lacking. The final part of Section 5.3 has been completely rearranged, also to accept other comments by Reviewer #1. Please refer to Comment 2, Reviewer #1 for details and new text.

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