



EGUsphere, referee comment RC2
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Comment on egusphere-2022-153

Anonymous Referee #2

Referee comment on "How volcanic stratigraphy constrains headscarp collapse scenarios: the Samperre cliff case study (Martinique island, Lesser Antilles)" by Marc Peruzzetto et al., EGU Sphere, <https://doi.org/10.5194/egusphere-2022-153-RC2>, 2022

The manuscript by Peruzzetto and co-authors focuses on the assessment of collapse scenarios on volcanic islands using an example from the Martinique Island. To achieve the study goals, observations from assessable stratigraphy are used to construct surfaces that can potentially act as rupture interfaces.

An aspect that can also be worth clarifying since the start of the manuscript is what is aimed at when mentioning reconstruction of the paleo-valley. I mention this just avoid early misinterpretations as quite often the term is used to recreate a pre-collapse morphology and calculate volumes evacuated from the slope. In this work, the "reconstruction" focuses more on the interpretation of a paleosurface buried by posterior volcanic deposits.

The manuscript is well written and generally clear, apart from some occasional typos here and there. I made some suggestions on this, but a final proof-read is recommended. The figures are also of good quality and clear. However, and as commented also below, the colour codes used for the geological units in figure 3 do not correspond to the ones mentioned in the text, so this needs to be addressed. Please see below for my comments on this. Despite their number, I believe mostly are easy to address, and quite a few are just spot corrections.

Title:

The mention to 3D volcanic stratigraphy is good to draw attention, but after reading the content how much of 3d stratigraphy was actually able to be identified? There are some assumptions and manually created surfaces to create a hypothetical rupture surface, but not much beyond that. I'd suggest dropping the 3D from the title and keep the rest, as it will hold in relation to the content of the manuscript.

Introduction

Line 20: in addition to these aspects, shouldn't vertical volcano-stratigraphic heterogeneity be mentioned as well? The alternation between different lithologies is itself a factor upon which all other will act upon, from just the gravity effect to fluid-related processes (derived from differences in permeability and flow properties). Although the accumulation of eruptive material is mentioned in the previous paragraph, the implications of distinct poro-perm properties is not.

Line 36: not disagreeing with the statement that numerous small collapses may constitute a more immediate risk compared to large ones, how far can the statement of similarity between them be supported? Numerous small collapses may be limited to the proximity of the edifice, and if far travel by these takes place the flows will be somewhat contained to streams and ephemeral flow paths, with immediate risk to the vicinity of these features. A major flank collapse may obliterate a lot on its flow path. Can this similarity stand?

Line 51: on the mention that the determination of the landslide extent is a problem by itself: agree, but can a line of two be added on how this is a problem to complement the methods through which it is inferred? The first thing that usually comes to mind is the volume balance issue, as the calculated evacuated volume may not correspond to the inferred deposit. The deposit volume can be lower if part of the material is washed out and not trackable, or much larger as debris flows entrain more material. The latter is markedly significant for downslope risk assessment.

Lines 63 to 65: very identical statements on the complexity and heterogeneity of volcanic complexes were made just some lines above. Please edit where more appropriate to avoid repeated content.

Methods:

Line 85: based on what is described and referred to figure 1, the latter could include a map with the broader geographical location of the Lesser Antilles.

Line 88: can the "first stage" please be clarified? Is it first eruptive stage? The geological succession itself is not strictly a stage as well, but the sequences produced by the volcanic activity. I suggest a slight rewording of these sentences.

Please consider this comment on stage and the need to describe what it is (eruptive/active/volcanic/other as appropriate stage) valid for all instance ahead as well.

Line 110: as there was a change in paragraph, starting the sentence with "Such..." is not adequate. Either move this to the previous paragraph or add a few more words to clarify what type of avalanches the current paragraph will address.

Line 129: seems to be a word missing. "... need to study/analyse/other? the geology of the.."

Line 135: although mention to table 1 is made, please indicate just the number of additional sets acquired between 1988 and 2018.

Line 136: a reference to figure 2 would be appropriate here.

Line 145: is this 3D point cloud and the horizons it shows represented in any figure? If so, please cite it.

Line 153: please indicate where the reader can see these polylines. Are these the limits of interpreted areas in figure 3?

Line 163: I'd suggest finishing the sentence with "... successive destabilization events/episodes".

Line 164: "...units that are stable." Correct same typo in line 167.

Line 168: "... have remained.."

Line 188: please clarify the colour corresponding to unit La. The text mentions blue but the figure shows it as orange.

Line 190: I suggest editing to "Most of the cliff below units Pu and La is..."

Line 190: Unit UPd is shown in figure 3c as a pinkish colour. Clear purple is associated with unit LPd, which in line 209 is described as orange. Please correct all colour descriptions.

Line 193: wouldn't it be clearer to just azimuth as N254 instead of N254E?

Line 198: "dip angles.." "dip directions.."

Page 9 in general: some observations on lithology and/or water seepages are made. Not doubting their presence or interpretation, are there any aspects in the figures that can help to support such observations?

Line 221: "...pyroclastic deposits..."

Page 9 and 10: these bibliography-based interpretations of the rock units are adequate given the limitations of sample collection. Are there any further descriptive aspects taken from the pictures that can support some of them, given the good quality available. For instance, is anything else observable for unit Pu to support the interpretation of pumice, beyond its colour?

Line 231: do the authors mean "were previously covered"? If this is to refer to multiple episodes, use re-covered instead or recovered as these imply distinct meanings.

Lines 230-235: a possibility to support the premise of hardened units C0 and LPd would be to show small topographic profiles across them. Being hardened, they would show as small bulges less prone to erosion and remobilisation than the adjacent UPd.

Line 236: This sentence needs to cite figure 7 for the reader to understand what is being talked about and see the surfaces. The sentence also needs to be re-written for clarity, namely in what regards the use "respectively X". An issue is that citing figure 7 for this will compromise the figure citation order as figure 6 has not been cited in the text so far.

Line 244: Was the post-collapse infill of the accommodation space by LPd associated with some sort of stratal dip, i.e., the LPd beds were not horizontal at deposition? If so, that is not captured neither by the written interpretation nor the diagrams in figure 6. Some clarification on such property could be useful. The same is valid for the stages where UPd deposited. Those clearly have evidence of dip, supported by the data, which could be represented in the diagrams (even if exaggerated for representativity).

Line 245: this interpretation needs to be supported by figures for the reader to follow the process.

Discussion:

Lines 285 to 296: this paragraph discusses and supports quite well the interpretation of a stabler/indurated CO. It was one of the first questions that popped to my mind at the start of the discussion, and there may be a comment or other above on that, but this adequately addresses it using literature examples. However, these may still be considered somewhat speculative given the lack of sampling to support the ideas

Line 300: as we cannot be truly sure, at least not without samples, I suggest changing "certainly" for "likely" and keep interpretations open.

Line 326. It is hard not to consider that water drainage, at surface and subsurface levels, is not be a key driver in erosion and shaping of the slope, be it at continuous sediment removal or more dramatic collapses. Is there any possibility of comparing rainfall data to morphological changes in the different periods of 2018, if relevant or applicable? If not driven by drainage, what other processes could be speculated for collapses on this setting?

As follow up , and despite the claim for further data to support or not the link of collapse to drainage, reading further ahead the authors dedicate a discussion point to the effect of groundwater circulation and links to precipitation as the main destabilisation mechanism. This somewhat contradicts what is stated or doubted in point 5.2. Even if miss-interpretations could derive from a possible ambiguous meaning of "drainage" in the paleo-valley as only surficial runoff, the frequent mention to water seepages does seem to imply that groundwater flow is part of it. Please adjust the discussion to make it concise on this matter.

Line 335- Point 5.3: I just want to add that this point seems, to me, well achieved. It mentions scenarios, impacts and uncertainties. Questions that could be raised for some discussed aspects were adequately clarified in following sentences.

Line 393, Referring to the whole paragraph: can averaged volume remobilisation per year be a reliable indicator to estimate evacuation trends? Collapses tend to be relatively "instantaneous" and frequencies are variable, so what is the risk of averaged rates leading to inadequate comparisons between different examples?

Line 403: This comment could have been made before, but still relevant here: Brunet et al 2016 refers to a flank collapses in offshore settings, a scenario that is quite different from the one presented for the Samperre ravine. The volumes involved are also drastically different as submarine slides tend to be much larger, as the numbers here provided show. Despite the interpretation of multiple collapses on the flanks of the volcano, and some material derived from slope collapse, there is a large amount of material derived from basin sediment, so flank retreat is not directly comparable with the volumes provided as example. Referring back to the previous comment, how valid are averaged rates, especially for such long-spaced events within a time frame of 130k yr?

Line 416 - on the examples of other erosive processes listed that may be dominant over edifice collapses: The retrogressive erosion can happen derived from different processes, so it is valid, although big collapses can still be the main driver for retrogression and sediment removal, with posterior morphological smoothing. On the other two examples mentioned, landslides (s.l.) were the main cause for strata removal, even if triggered by distinct processes. How is the distinction in process dominance established, when based on the examples given the dismantling seems to mainly occur through the same one, i.e., slope collapse?