



EGUsphere, author comment AC1
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Reply on RC1

Edgar U. Zorn et al.

Author comment on "Identification and ranking of subaerial volcanic tsunami hazard sources in Southeast Asia" by Edgar U. Zorn et al., EGU Sphere, <https://doi.org/10.5194/egusphere-2022-130-AC1>, 2022

The manuscript entitled "Identification and ranking of volcanic tsunami hazard sources in Southeast Asia" by Zorn et al. proposed a catalogue of potentially tsunamigenic volcanos in Southeast Asia and ranked these volcanoes by their tsunami hazards. The evaluation is based on a Multicriteria Decision Analysis (MDA) composed of five weighted factors. They identified 19 volcanoes with high tsunami hazard and 48 with moderate tsunami hazard. The proposed ranking system can identify the hazards of Anak Krakatau and Kadovar before a tsunami occurs as a retroactive study.

I agree that this study is meaningful to disaster mitigation of volcanic tsunamis. However, the ranking system proposed in this study is not objective and in lack of quantitatively evidence to support the assessment. Meanwhile, the linear combination of five individually weighted factors for MDA are questionable. Unfortunately, the present form is not suitable for publication in Natural Hazards and Earth System Sciences. Significant additional work is required to improve the methodology and contents. My suggestion is that the manuscript should be revised substantially and resubmitted. I am willing to review this manuscript again after their revision. Here are my comments on this manuscript.

Reply: We appreciate the feedback and the constructive comments that helped us to improve and clarify many points related to ranking system, objectivity of the analysis and interpretation. We especially improve the methods related to the individually weighted factors for the MCDA, we quantify the uncertainties, and rewrite large parts of the discussion section, where limitation aspects are now much more carefully illuminated. By responding to this point and the more specific points below, our manuscript has much improved.

1. My first concern on this manuscript is that the ranking system is not objective. The scoring (F) is based on qualitative analysis. There is not physical or experimental evidence to prove the reasonability of such scoring. For example, the scoring of H/D-Ratio has values ranged from 0.02 to 0.89 and these values are multiplied by 100 to get a 0–100-point scale linearly. In that case, it means that a H/D-Ratio of 0.4 has twice the score (i.e., risk) to a value of 0.2. However, such assumption lacks evidence. No numerical simulation or geological evidence are presented to support the scoring method. This problem also occurs in other four factors.

Reply: We appreciate this comment and now improve the reasonability and objectiveness of our scoring (F) analysis. We also better discuss the limitations of the approach. As the reviewer correctly points out in the next comment, our score should be seen as an estimation for hierarchy rather than strict empirical criteria.

We clarify this by firstly pointing to the purpose of our approach (creating a hierarchy) by adding the following text to the introduction. We make these changes:

Page 3, line 79: "While we incorporate some elements of review studies, which have been done extensively for the volcanic tsunamis in Southeast Asia (see e.g. Paris et al. 2014; Mutaquin et al. 2019), we expand on this by attempting to place the potential

source volcanoes in a hierarchical order and identify the most likely volcanoes to cause tsunamis in the future. For this purpose, we

create a comprehensive catalogue of potentially tsunamigenic volcanoes and further use this data to create a point-based hierarchical

ranking and identify the most likely candidates for sourcing potentially catastrophic tsunamis in the future."

We then further improve on the method description and reasonability. We note that our score, while being subjective and in lack of empirical hazard relation, is still reasonable to use for creating a hierarchy. We further point out that our data used to create the score is based on very objective criteria that can be measured and quantified. We add this in:

Page 6, line 137: "While there are numerous factors that can be considered to reflect the tsunami hazard from a volcano, most of them do not have a known empirical relation to the hazard. For example, it is reasonable that a steep volcano close to the sea

is more likely to produce a tsunami than a gently sloped one far inland, but exactly how much more likely this makes a tsunami is not

known. With our ranking, we can therefore only aim to compare these factors by assuming that certain higher values equal a higher

hazard. We consider the following five factors and point systems for the ranking. Each represents a set of data that can be recorded or

Page 13, line 265: "...our MCDA is based on arbitrary and thus subjective point scales, assigned to best cover the range of values used to build the ranking score."

Finally, we also appreciate the reviewers comment specifically on the linearity of our scale. Scales like this are usually linear as these are the simplest scales, but can still be reasonable and meaningful. In order to further improve the manuscript, we now have added a discussion paragraph in the limitations section referencing some relevant previous papers as examples. These are the changes:

Page 13, line 267: "In studies previous ranking volcanic hazards and risks, these could be done by a simple count of "yes" or "no" features adding 1 or 0 points, respectively (Yokoyama et al. 1984), or similar variations (Ewert 2007; 2018), or via the creation

of index values and adding them up to create a score (Scandone et al. 2016). Then categories (e.g. high, medium and low hazard) are

defined to best cover the range of scores (e.g. Ewert 2018), which is also what we do in our ranking. MCDAs in other fields often have

more quantitative scales such as 0-9 points (Fernandez et al. 2010; Rahmati et al. 2015) or 0-100 (Nutt et al. 2010), but the score

systems are still assigned arbitrarily. Thus, all these approaches and our ranking presented here use some degree of subjective

volcanoes could be made. However, as the rules with which points are given are kept strictly the same for all volcanoes, the comparability of scores is retained, allowing for a meaningful hierarchical order or scores. For our ranking, this means that the hazard score by itself should be seen as a rough hierarchy estimation rather than a strict empirical value as it has little meaning in terms of hard data, such as expected tsunami event frequency, possible wave heights, or impacts on shorelines and population. Similarly, we can thus not adequately assess the risk to shores and population in the traditional sense. Instead, we identify which volcanoes are the most likely to cause a tsunami in the future as these are expected to produce the highest hazard score."

2. Similarly, the weighting (W) of the ranking system is also subjective. I agree that the results of robustness testing are satisfactory. But the testing itself cannot show the importance (or contribution) of each factor for MDA. Therefore, the total weighted score can only be used as a rough estimation rather than a strict criterion. The authors may add a confidence level to each total weighted score.

Reply: We agree that the weights are subjective. We also agree that the weighted score is not a strict criterion. We thus further clarify and improve the subjectivity issue in the methods section by adding the following on:

Page 8, line 228: "For the factor weights, we have to choose values based on the importance of the factor data. A higher weight of a factor will result in a larger impact of this factor on the final score and thus make it more important. Here too, these choices

are largely subjective, but allow reducing the impact or importance of e.g. less reliable factor data and in-turn raise the impact of more

reliable factors"

Page 13, line 267-275: "For our ranking, this means that the hazard score by itself should be seen as a rough hierarchy estimation rather than a strict empirical value as it has little meaning in terms of hard data, such as expected tsunami event frequency,

possible wave heights, or impacts on shorelines and population."

Furthermore, we now follow the advice of the reviewer, by adding standard deviations to quantify this method and modify our robustness testing approach. We show that we can reliably identify the most likely volcanoes to cause future tsunamis despite the subjectivity of our weights, but can be less certain of the sorting with lower scoring volcanoes. We can also identify which volcanoes are more sensitive to the importance (or contribution) of single factors. We have replaced figure 4 and modified our method description and results accordingly.

Page 9, line 245: "We further tested how robust our ranking is with respect to used factor weights. This is done to confirm that the highest scoring volcanoes still retain their high score even when the weighing is significantly different, which can confirm that

these volcanoes really pose the highest tsunami hazard despite possible human error or misjudgement. The test was carried out by

changing the five factor weights, increasing one factor to 60% and all others are set to 10%. The procedure was repeated for all five

factor weights, so that every single factor was once set as the strongest influence. We also added one instance of all weights being

considered equal (i.e., all five factors being weighed at 20%). This then enabled us to calculate both an average score and standard deviations within this variability of weights. The results could then be used to judge whether our ranking can generally identify the highest scoring and most hazardous volcanoes well, despite the subjective weight choices. We could further determine which volcanoes were more sensitive to the influence of single factors, as this would result in higher deviations.”

We also updated the figure 4 caption.

Page 19, line 357: “Figure 4: Robustness test of the factor weights used in the ranking. This was done by calculating an average score and standard deviations from repeat scoring while systematically changing the factor weights. It shows that the

volcanoes we classed as high hazard volcanoes are generally well distinguished, with the highest values independently of factor

weights. This demonstrates that changing the factor weights may slightly change the order in which the volcanoes are ranked, but our

and low hazard volcanoes the ranking is less robust, due to a high number of volcanoes with similar scores, which can significantly change the hierarchical order depending on the chosen factor weights.”

Reply: We appreciate this comment and improve the manuscript by clarifying the factor dependency. The individually weighted factors we used are, in fact, largely independent. Taking the example above, we actually discuss this for the 2018 Krakatau event at page 22, line 403. The removed steep slope resulted in a lower slope score for Krakatau after the landslide, but a higher tsunami score due to the additional event, so these are measured separately and independently. The only exceptions are the H/D-ratio and slope, which are actually dependent, but this is not problematic for our ranking. We added a statement highlighting this point in the limitations, but use a different example.

Page 14 line 290: "Conducting a comparative ranking can be more challenging if there are major dependencies between the used factors. As an example for our case, it would be reasonable to assume that recent eruptive activity would more likely cause

hydrothermal alteration, thus making the eruptive history and hazardous features factors interdependent. However, in our catalogue,

only few volcanoes are recorded to have extensive hydrothermal alteration on their flanks and for many of these, no eruption occurred

for decades to centuries (e.g. Manuk, Teon, Serua). Hence, we think that these issues are unlikely to significantly affect our results. The

only exception is a direct dependence between the H/D-ratio and the slope angle as it is essentially the same value if the volcano is

steep slopes on a local level.”

4. The heat map (Figure 7) and travel-distance plots (Figure 8) cannot accurately represent the potential volcanic tsunami hazards because they do not incorporate the information of tsunami amplitude. It makes the hazard assessment less powerful. A tsunami with 1 m amplitude has evidently different impact from the one with 0.1 m amplitude. I believe it is a MUST to consider the potential maximum amplitude when analyzing volcanic tsunami hazards.

Reply: We appreciate this comment and make multiple improvements and clarifications to the text and figures, as outlined in the following. Indeed, figures 7 and 8 do not incorporate information on tsunami amplitudes. This is intentional as a reliable assessment of volcano-generated tsunami wave amplitudes requires knowledge of many of yet unknown source parameters. Specifically, there are multiple potential processes at volcanoes which may generate a tsunami (explosion, flank collapse, PDC etc.). Each of them has a specific set of parameters describing magnitude, direction, etc. and each of them would result in highly different wave amplitudes. Reliable modelling of volcanogenic tsunamis requires thorough collection and evaluation of these specific source parameters, in addition to the advanced numerical techniques beyond classical nonlinear shallow water (NLSW) algorithms, and is usually applied to specific singular (historical) events. Incorporating such modelling for multiple volcanoes at once (in a ranking study like present) would not only be highly demanding, but, without constraining all the principal source parameters, also highly speculative.

Instead, we would like to avoid producing highly unconstrained results and pursue a simpler and more robust approach by setting our ‘tsunami impact metric’ to a length of the coastline potentially affected by tsunamis within given propagation time. Note that these simple tsunami travel time models have the advantage that they are independent from the wave height and the generation mechanism (as long as it is a point source), so we can make meaningful assessments without assuming a yet unknown tsunami source.

To improve the manuscript, we firstly address this issue by clarifying the aim of the modelling. We particularly emphasise that predictive models (e.g. Giachetti et al. 2012) require in-depth understanding of specific local factors:

page 24 line 451:“Consequently, predictive studies remain rare (Giachetti et al. 2012; Paris et al. 2019) and are only possible because the specific local circumstances leading to the tsunami are very well understood, which is knowledge that is lacking for

most coastal volcanoes. Here, we provide multiple predictive models for the volcanoes we classified as posing a high tsunamigenic

hazard. As volcanogenic tsunamis are caused by a large variety of mechanisms (Fig. 6) we contribute to this aspect by providing a

simplified and broader view at the travel times of potential future tsunamis that are unspecific to the mechanism of tsunami generation and their magnitude (with the possible exception of meteotsunamis as seen at Hunga Tonga Haʻapai in 2022, which appear to have different wave propagation properties). We mainly account for the potential spatial impact of volcanogenic tsunamis and extend our tsunami hazard evaluation by assessing the total length of a coastline affected within one and two hours of tsunami propagation for the volcanoes categorised as high hazard in our ranking (except Didicas)“

Secondly, we highlight that the amplitudes and wave heights cannot be considered, but that comes with the advantage of the tsunami source independence.

Page 24 line 455: “This means that we can simulate the travel and arrival times of specific volcanoes independent of how

the tsunami was generated (as long as it is a point source), but we also cannot consider specific wave heights or runup as these depend strongly on the specific source mechanism and magnitude of the event and require additional and much more specific modelling data for individual sites.”

Page 26 line 475: "While our models are limited to the travel time, they can be used to estimate the warning time for shores in case a tsunami occurs at one of the considered volcanoes."

Thirdly, we agree with the reviewer and recognize the value of models with specific wave heights. While we prefer our simplified broader models, we instead provide an additional paragraph summarising some previous studies specific to single volcanoes and historical events:

Page 23 line 444: "In order to assess the risks and impacts of volcanogenic tsunamis, numerical simulations are commonly used, both for distinct future scenarios and in retrospect for past events. For Southeast Asia, a large number of such studies had been

conducted. Most models were done for Anak Krakatau looking specifically at the 2018 flank collapse with some using the known event

to calibrate and confirm the quality of current simulation methods (Grilli et al. 2019; Borrero et al. 2020; Mulia et al. 2020; Omira and

Ramalho 2020; Paris et al. 2020; Zengafinnen et al. 2020), some using the known tsunami data (e.g. from tide gauges) to identify

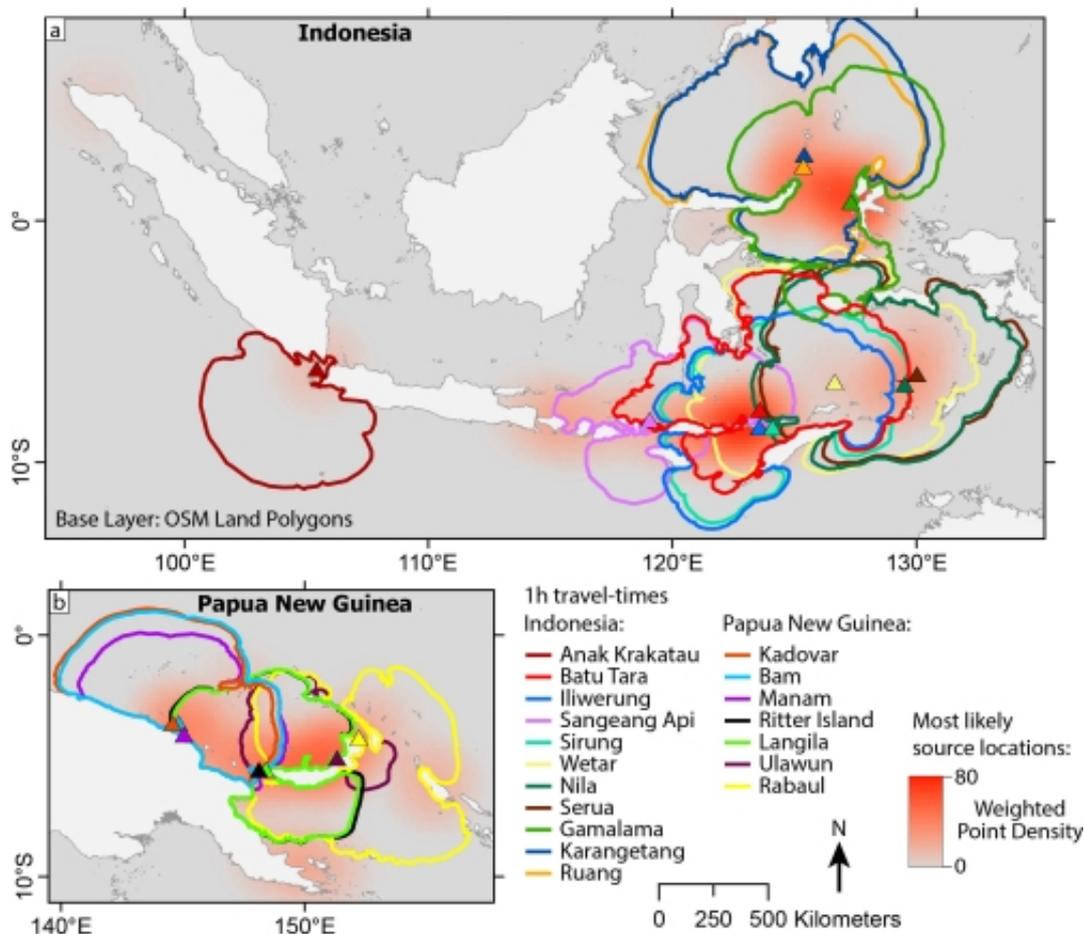
source parameters (Heidarzadeh et al. 2020; Ren et al. 2020; Grilli et al. 2021) and some testing variations in the source parameters

volume that occurred both with a subaerial and a submarine component is mostly consistent with the observed and modelled runup heights at the adjacent shores. Similar models also exist for the 1883 tsunami at Krakatau, with the main purpose being the identification of its generation mechanism (Maeno and Imamura 2011) and how such a tsunami propagates in the far-field (Choi et al. 2003). Predictive studies only considering possible future events are not as abundant, but have been done for Anak Krakatau before the 2018 tsunami (Giachetti et al. 2012; Badriana et al. 2017), with Giachetti et al. (2012) making a remarkably close prediction to the later event. Other volcanoes in Southeast Asia are not as commonly considered. Pranantyo et al. (2021) test the tsunami propagation from Ruang volcano, Indonesia, using and comparing both historical observations and data from the 2018 Anak Krakatau event and reproducing a 25 m runup in the near-field. In Papua New Guinea numerical tsunami models have almost exclusively been considered for the Ritter Island tsunami in 1888 and the reconstruction of its generation (Ward and Day 2003; Karstens et al. 2020). Similarly, numerical tsunami models in the Philippines are mostly limited to Taal volcano, where models are based both on a past tsunami in 1716 (Pakosung et al. 2020) and a predictive study considering scenarios with different explosion sites and energies (Paris et al. 2019). Considering these works, it is clear that tsunamis sourced by volcanoes can be well explained with numerical models, but the considered volcanoes remain limited to a few select sites and scenarios. These models are also typically restricted to one particular volcano and one specific mechanism of tsunami generation as a retrospectively investigation."

We also make a brief point that our travel-time models could be supplemented with more specific scenario models in future studies.

Page 26 line 489: "For future hazard and risk assessments, we thus recommend supplementing the knowledge from our TTT-models with specific detailed scenario calculations using established numerical modelling approaches, particularly for those high-hazard volcanoes where no such models exist (e.g. Batu Tara, Iliwerung, Nila)."

Finally, we combined figures 7 and 8 to avoid confusion regarding our TTT models and the heat map highlighting the likely future focus areas for tsunamigenic volcanoes.



5. The conclusion of this manuscript is too simple. It is necessary to discuss the limitation of this ranking system.

Reply: We thank the reviewer for raising this point and thoroughly improve the

conclusions of the manuscript and now also mention the limitations of the ranking approach. The new conclusion now reads as follows:

Page 31 line 607: "Based on our MCDA analysis considering 131 volcanoes in SE-Asia we identify 19 that pose a high tsunami hazard and another 48 with moderate tsunami hazard. We find our ranking system to be robust for the higher scoring

volcanoes, meaning that we can reliably identify the most likely volcanoes to produce a tsunami in the future. For volcanoes with

moderate to low scores the ranking is less robust and more susceptible to subjective judgement. The main limitations remaining are (1)

a lack of knowledge how much individual factors contribute to the tsunami hazard of a volcano, instead requiring subjective

assumptions, (2) erroneous, incomplete or insufficient data availability for many volcanoes (e.g. bathymetry or historical data), and (3)

assessment challenging.

Our results show that the Indonesian Lesser Sunda Islands and northern Molucca Sea as well as the southern Bismarck Sea in Papua New Guinea are areas with a high number of hazardous volcanoes and may thus be particularly prone to tsunamis sourced by

volcanoes. Many of these volcanoes such as Batu Tara, Indonesia, are not commonly considered for this type of hazard. We therefore

emphasise the need to reconsider the current state of monitoring and risk assessment in these areas. Since tsunami warning systems

are mostly not designed to detect volcanogenic tsunamis, our results highlight the importance of a reassessment of the current network

and additional suitable equipment on the ground and through earth observation satellites. Due to the inherently short warning times of

coastal communities and the marking evacuation routes along populated coasts.”

Other minor comments:

Line 71: I agree that “the inherent problem of volcanogenic tsunamis is the lack of warning time and quick response options”. However, even if we successfully identified the high-tsunami-risk volcanoes, this problem still exists. Please discuss potential solutions (e.g., radar, bottom pressure gauges) to fix this inherent problem.

Reply: We agree and add a short paragraph. While we cannot fully solve this problem, our work can help prioritise where best to implement these potential solutions.

Page 3 line 77: “This may then allow for a targeted implementation of disaster mitigation strategies and warning systems at critical sites, e.g. by placing additional tide gauges as proposed for Krakatau (Annunziato et al. 2019) or improved volcano monitoring.”

Page 26 line 489: “For future hazard and risk assessments, we thus recommend supplementing the knowledge from our TTT-models with specific detailed scenario calculations using established numerical modelling approaches, particularly for those high-

hazard volcanoes where no such models exist (e.g. Batu Tara, Iliwerung, Nila). This coupled with the prioritisation of specific volcanoes

provided by our ranking can provide a well-founded basis for future disaster mitigation strategies. While the detection of volcanic

processes triggering a tsunami will remain challenging to detect due to the multiple possible generation mechanisms, other steps can

be done to improve warning times. These include the addition of strategically placed tide gauges as suggested by Annunziato et al.

data (e.g. InSAR) can also help to preemptively identify volcanic unrest or destabilising flanks.”

Line 105: Add a figure and use an example to show the process of defining the edifice boundary.

Reply: We agree, but since the edifice boundary is not critical for our scoring, we do this in the supplement and point to it in the text.

Page 5 line 110: “A full example for the NETVOLC and MORVOLC output and an illustration of the edifice boundary definition is provided in the supplementary material A.”

Line 332: Remove the repeated word in “high high-hazard”.

Reply: Much appreciated, we corrected this.

Line 333: Please explain the reason why there are some volcanoes with high scores but not prominently considered for their tsunamigenic potential.

Reply: This statement is confusingly phrased, we apologise. What we meant to say is that these volcanoes are not as prominently known to be a major tsunami hazard (e.g. Batu Tara, which is not well studied in this regard). We changed the sentence accordingly.

Page 15 line 332: “However, we also identify high-hazard volcanoes that are not as well known for their tsunamigenic potential, but received similarly high scores.”

Figure 5: Please add a subpanel to show the respective distribution between the countries

for all considered volcanoes.

Reply: We agree and reworked the figure as suggested, the new subpanel is a)

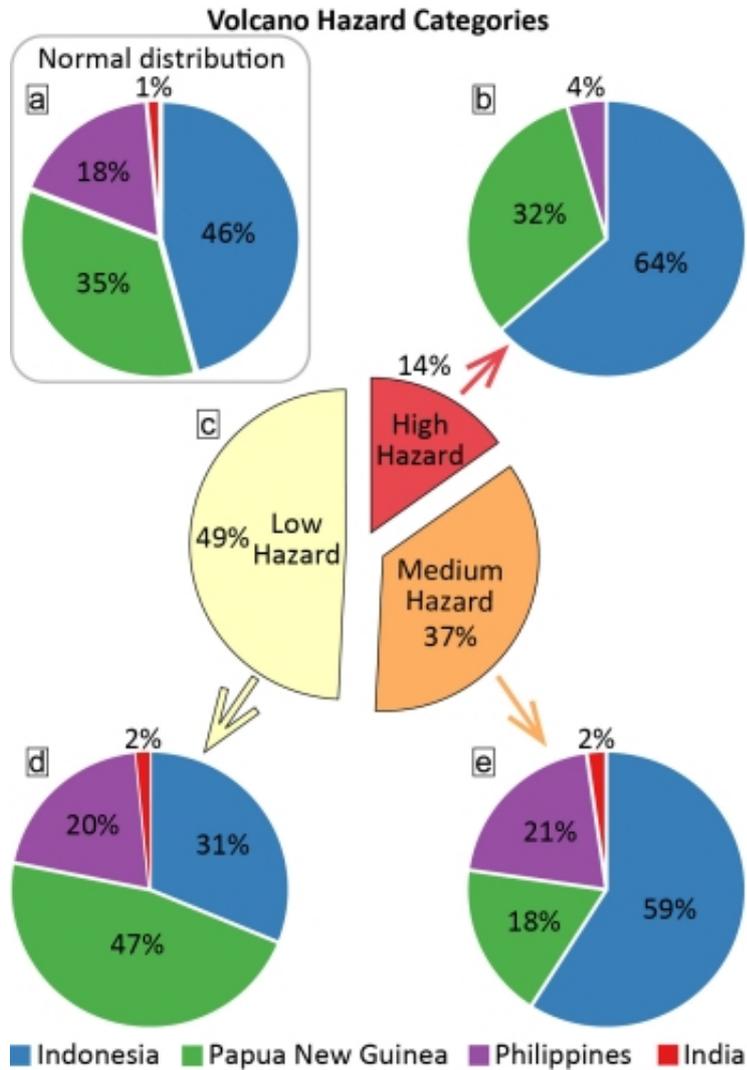
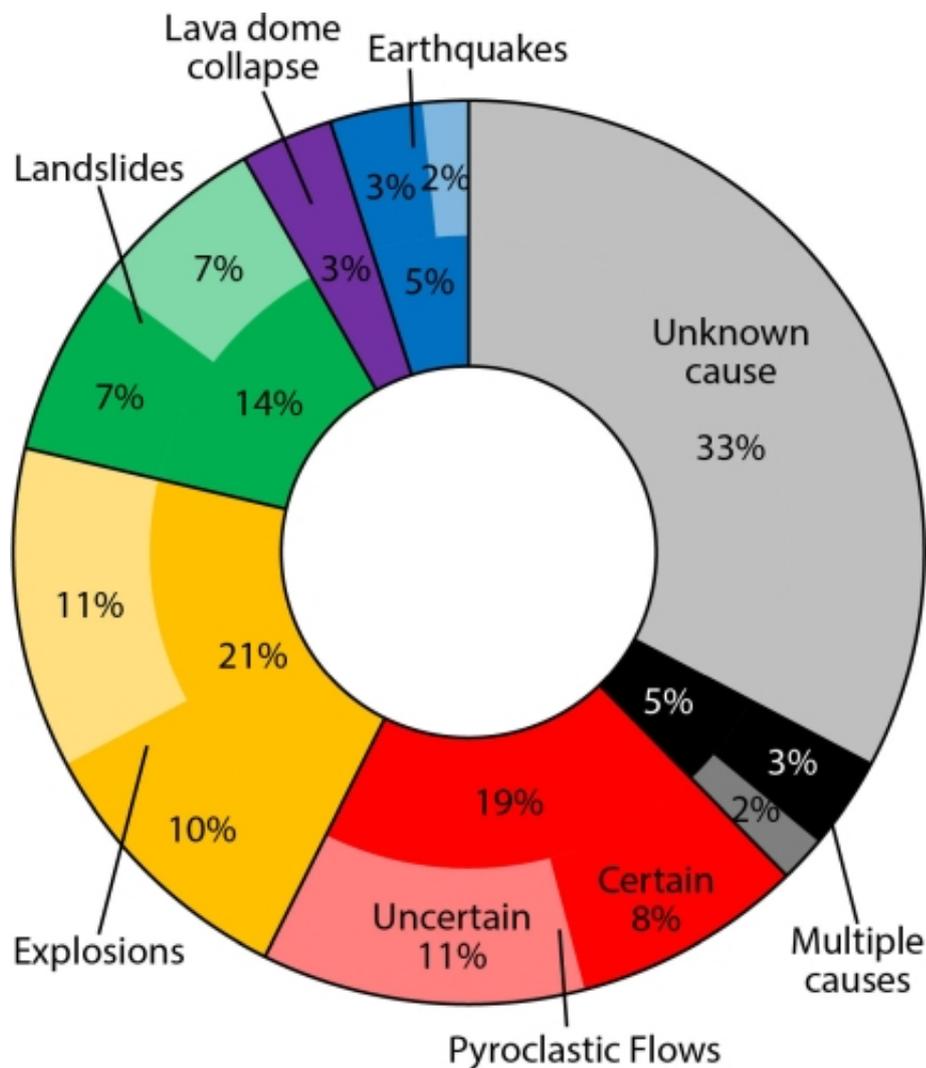


Figure 6: What are the different meanings between dark red and light red (also blue, yellow, green, etc.)? Please specify.

Reply: The dark and light colour are transparency settings mentioned in the figure caption and symbolise that the source mechanism is suspected but uncertain. We clarify this by reworking the figure to a donut plot that we can label more clearly.



Section 4.3: This section seems verbose. The authors may present Batu Tara here and move others to supplementary material. Instead, it is better to have more discussions on potential tsunami scenarios of Batu Tara.

Reply: We agree and moved this section as suggested. We now include Batu Tara in the earlier discussion section, including a new figure. The other volcanoes discussion generally serves as a contextualisation of the various high-hazard volcanoes, which may indeed fit best in a supplementary text.

Page 23 line 443: "A brief feature of individual high-hazard volcanoes can be found in the supplementary material C."

Please also note the supplement to this comment:

<https://egusphere.copernicus.org/preprints/2022/egusphere-2022-130/egusphere-2022-130-AC1-supplement.zip>