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Review and rebuttal of the paper

Field Survey of Flank Collapse and Run-up Heights due to 2018 Anak Krakatau Tsunami

Miguel Esteban et al.

Editor handling the paper: Bas Hofland

Review round 1

Reviewer A

Review “Field Survey of Flank Collapse and Run-up Heights due to 2018 Anak Krakatau Tsunami” by Author et al.

The manuscript report results of a field survey undertaken 8 month after a flank collapse of Anak Krakatau occurred in Indonesia. The tsunami as a result of the flank collapse resulted in severe inundation and run-up, and lead to significant numbers of casualties, injuries and damage along the Javanese and adjacent coastlines. The authors present some additional run-up data not to be confused with those in an earlier publication (Takabatake et al, 2019) of some of the authors. The manuscript also presents bathy-topographic data which are a prerequisite to successful numerical modelling of the event, and to enable entangling how the sequence of wave generation unfolded. These data are however not used to conduct a numerical simulation but the analytic outcome remains collapse volumes that were presented alongside of cross-sectional data of the after-collapse condition. There is also a 360° video presentation, above- and under-water footage that is briefly discussed.

The study is generally interesting and in line with the scope of the young journal as it addresses a topic (coastal hazard) from a post-hazard perspective, providing some of the basic data required to set-up further numerical or even experimental work. The topic is generally timely, and relevant to the readership of the journal. I, however, see some serious concerns, and these are explained below where I have listed some general comments I made when reading the research reported in here.

In general, the work presented has currently a low-to-medium level of maturity, reflected by the very low amount of data used to compile the work. The data and analysis that is presented, although generally relevant, remains fragmentary, limited to a collection of preconditions to a numerical study unravelling the causes of the observed tsunami waves.

Overall, the reviewer reasons that the manuscript has a level of maturity below the standard of the journal to make it fit for publication.

Assessment: “Reject, with encouragement to resubmit”

Major and minor detailed comments are provided below to help the authors to further develop their manuscript.

The authors would like to thank the reviewer for the time taken to provide such constructive criticism about the manuscript. The authors agree that there are many places where the description could have been improved. In this resubmission, the authors have addressed the comments made by the reviewer, as will be outlined below. The authors welcome this discussion, and remain open to make further changes to the manuscript, should the reviewer have any additional comments.

General comments:

The authors would like to note that each of the comments by the reviewer have been sub-divided into several sub-comments, in order to indicate how each of them has been addressed.

a)

1. The introduction, as well as the entire work is very brief on the topic. This does not have to be necessarily a bad thing, however, in this case, I have missed more background, and a more complete literature review pertaining to slide-induced tsunami events.

Thank you very much for your suggestions. The authors added the following paragraph to explain about recent landslide-tsunami events.

"Landslides can generate significant tsunamis, with a number of significant events having taken place in recent decades. In 1963, a subaerial landslide at Vajont Dam, Italy, impacted a reservoir and generated a significant wave, causing more than 2,000 fatalities (Panizzo et al., 2005; Genevois and Ghirotti, 2005). During the 1988 Papua New Guinea earthquake, it is believed that a submarine landslide generated a tsunami having over 15 m at Sissano split (Tappin et al. 2001, 2008). In 2007, subaerial landslides at Chehalis Lake in British Columbia, Canada, caused a tsunami run-up of 38 m (Brideau et al., 2012). In the 2010 Haiti Earthquake, coastal landslides caused by an earthquake generated a local tsunami waves of up to 3 m (Fritz et al. 2013). During the 2018 Palu Earthquake, multiple submarine landslides generated devastating tsunamis with inundations height of up to 5 m in the City of Palu and the Donggala Regency, in Sulawesi Island, Indonesia, (Sassa and Takagawa 2018; Mikami et al. 2019; Stolle et al. 2020; Harnantyari et al. 2020)."

2. The authors could have extended their work also to review how bathymetric data could influence tsunami modelling taking into account that dispersive waves as slide-induced tsunami waves are, are pretty much dependent on bathymetric features.

A part of the discussion was modified and expanded. The authors wish to note that it is difficult to definitively say more at this point, given the substantial complexity of the present tsunami (from the discussions the authors have already had on the topic it appears that different hypothesis have started to be formed amongst various authors about the exact nature of the tsunami and how it moved within the islands of this small archipelago). Hopefully, the extra discussion can provide some insights, yet the final answer might take some time (in the same manner as the Sulawesi tsunami in the same year, where many authors are still discussing the exact mechanism that gave rise to the tsunami).

"This tsunami was generated in shallow water, and would have had a short wavelength (Takabatake et al., 2020). The fact that the bathymetry was rather deep (over 200 m) in the southwest direction from Anak Krakatau would have helped the tsunami wave propagate in this direction (away from the islands of Sumatra and Java, which can explain why inundation heights in the populated coastline in these islands was limited, see Fig. 1). However, the opening is rather large in this direction, which would have helped the tsunami wave energy disperse into the wider ocean.

While the main flank collapse appears to have been towards the south/south-west, the tsunami would have reflected off Rakata island towards the north. There is also the possibility that part of the volcanic collapse would have been towards the north (or other directions), explaining the considerable run-up in the other islands in the group. However, the bathymetry to the north of this group of islands is relatively shallow (around ~25m in most area). This would have hindered propagation (due to a reduced flow rate due to the low velocity of the tsunami). The high density of the tsunami wave would have resulted in the tsunami growing in height, and possibly causing some localized wave-breaking (though this last effect would have been minor). The worst inundation heights recorded by Takabatake et al (2018) were in western Java, which would have been facilitated by the slightly deeper bathymetry between Panjang and Rakata. However, far more important than the bathymetry is the effect of the shielding provided by the islands themselves, which would have reflected much of the wave energy and contained it within the inside of the archipelago."

3. I also note that meanwhile more than a handful of new publication has appeared that covers relevant parts of this manuscript.

This is true. The present manuscript was drafted by the authors immediately after their field surveys, but while computer simulations were being attempted many other papers have been published. The authors wish to emphasize how nothing that has been published so far actually does what is being reported here. Most other authors are using very course bathymetry from before the eruption, and at best satellite images that estimate the volume of the collapsed volcano. A few days before the review of this paper arrived to the authors, the paper of Borrero et al. (2020) was published, which is (to the authors' knowledge) the only other paper that reports on run-ups around these islands. However, these authors do not survey the bathymetry and the volcano itself. The following paragraphs have been added:

"The challenges of producing reliable simulations without having a reliable bathymetry are mentioned by Heidarzadeh et al. (2020), who approximated the tsunami wave by trial and error using twelve source models. This is a recurring problem from other authors, who furthermore have to rely on witness photography to estimate the possible run-ups (Paris et al., 2019), or just rely on the parameters proposed by Grilli et al, 2019 (for example Ren et al., 2020)

A number of authors have attempted to find out more details about the volume of the flank collapse, for example through the use of synthetic aperture radar (SAT) satellites (Williams et al. 2019). However, to the authors' knowledge, the only actual field surveys conducted up to date around Anak Krakatau itself are by Borrero et al. (2020) in February 2019. However, while these authors report run-up heights, they did not conduct bathymetry measurements, and for their computer simulations they used a landslide scenario from before the actual 2018 eruption (thus failing to take into account for the actual changes that took place in the mountain)."

The following paragraph has also been added to the discussion, to compare the results here with those of Borrero et al. (2020).

"At the southern side of Palau Sertung Borrero et al. (2020) report run-up heights of just around 50 m, raising to over 80 m. The authors only surveyed one point here, given that further north those interviewed indicated that the wave went over the island (thus it is not clear whether actually reporting the run-up height would be a good measure of this value). In the area of Rakata surveyed by the authors Borrero et al. (2020) indicate run-ups of 60 m, though they reported difficulty in obtaining results from Panjang."

b)

This being said, I do also have problems to accurately identify what the authors objectives were as I typically see an in-depth literature review that allows to extract objectives based on knowledge gaps found in literature. I admit that brevity of the introduction and literature review is probably the main reason for concise objectives.

Hopefully the points made above will have convinced the reviewer that there is considerable importance for these results to be provided to the community, so that other modellers can start tuning their results using real data (the authors would like to emphasize that, upon publication of this paper, all data collected -bathymetry, elevation, etc- will be made public, so that anybody who reads this paper can download it). It could well be that such data would belong more naturally in other journals. However, the lead author is incredibly tired of "commercial publishers" (indirectly) charging for the download of data that has been collected through taxpayers money. Hence, the authors hope that the reviewer sees the originality and contribution that this data will make to other modellers in the field of coastal engineering.

c)

I have serious doubt that the run-up data presented by the authors is particularly helpful, as the time that had elapsed after the collapse had taken place is fairly long. Climatic conditions, along with heavy rain, wind, and the effort to clean and rebuild would have made a comparison with other's data very difficult. I indicate that the authors, as they are discussing this problem in section 5 are equally aware of this serious limitation.

The authors indeed agree with the reviewer, and have clearly discussed this in the paper. At this point, the reporting of any results (to the best of the authors' engineering judgement) can hopefully be seen as better than not having any data at all. Despite all this, the reports collected by the park guards (who did enter the area in the weeks following the event) do provide some feeling of certainty to the data collected.

d)

When I was reading through the manuscript, I had envisioned an attempt of the author collective to use the bathymetric data for a first numerical model, as accurate bathymetry is a must for slide-induced tsunami modelling; availability and presentation of these relevant data is probably the main merit of the manuscript so far. I would be much in favor of accepting such a manuscript were tsunami simulations based on the bathymetric data be presented. I encourage the authors to complete their study, by using a numerical model and drawing some conclusions from these simulations.

The authors contacted a tsunami modeller to conduct such simulations, and this is the reason why the present work is being reported so late after the original fieldwork was carried out. However, preliminary attempts indicate that this is a challenging event to simulate, given the complex bathymetry and topography of the mountains. The introduction now points out how the simulations made by other authors are rather preliminary and, to the authors' knowledge, nobody can accurately simulate the run-ups around Anak Krakatau (very few attempt to do so, and those that do use too many assumptions for the simulation to be considered realistic, as indicated in the new paragraphs in the introduction).

Finally, the authors decided to simply report all their findings, following the format in which such field surveys of tsunami events are reported in the Coastal Engineering Journal (CEJ, where such field surveys are now long-established practice).

The authors wish to note again that, upon the paper being accepted, all bathymetry data collected will be made available as a dataset. Hence, while the authors continue attempt their own simulations, this data will be freely available to all other tsunami modellers. The authors hope that making such painstakingly collected data freely available can be considered a significant contribution to the field of coastal engineering (partially offsetting their current failure to accurately reproduce the tsunami).

Reviewer B:

The submitted paper presents an overall description of the main results obtained during a recent field survey of the Krakatau bay area, following the Anak Krakatau tsunami in 2018. The paper is interesting, well written and it provides useful and novel information for the numerical simulation of landslide-generated tsunamis.

The authors would like to thank the reviewer for the time taken to provide such constructive criticism about the manuscript. The authors agree that there are many places where the description could have been improved. In this resubmission, the authors have addressed the comments made by the reviewer, as will be outlined below. The authors welcome this discussion, and remain open to make further changes to the manuscript, should the reviewer have any additional comments.

The reviewer finds this manuscript worth of acceptance, although there is no direct link with Coastal or Hydraulic structures. I believe that the readership of this journal might be more interested in the hydrodynamic properties of the tsunami propagating inland (velocities and flow depths) rather than the dynamics of the collapse.

A part of the discussion was modified and expanded. The authors wish to note that it is difficult to definitively say more at this point, given the substantial complexity of the present tsunami (from the discussions the authors have already had on the topic it appears that different hypothesis have started to be formed amongst various authors about the exact nature of the tsunami and how it moved within the islands of this small archipelago). Hopefully, the extra discussion can provide some insights, yet the final answer might take some time (in the same manner as the Sulawesi tsunami in the same year, where many authors are still discussing the exact mechanism that gave rise to the tsunami).

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Besides a couple of points that could be further expanded and/or discussed, hereafter the authors will find a few minor comments to improve the quality of the submitted manuscript:

- Line 8: please clarify what you mean with "*source directionality*"

The term "source directionality" is widely used in tsunami simulation studies, to indicate the direction of propagation of the tsunami wave from the source. This is now explained in line 292 "(i.e. the primary direction of the propagation of the tsunami as it propagates over the ocean)

- Line 11; the key word "Sunda Strait" reads odd, I would recommend to change it with "Field survey", "bathymetry" etc. etc.

Changed to "field survey"

- Line 27: please provide a source for the estimated volume.

A source is now provided (Geospatial Information Authority of Japan, 2018).

- Line 60: Inundation should not be capital

Changed to lowercase.

- Line73: collapsed

Corrected, thanks.

- Line 77: “them” refers to the islands? It’s unclear

Correct, “them” was changed to “the archipelago”

- Line 98: it would be interesting to have an estimation of the distance between the islands and Labuhan

Labuhan is located at S6°22', E105°49', around 50 km away from Anak Krakatau. This is now indicated in the text.

- Line 118: closing bracket is missing

Added, thanks.

- Line 118: analySed

Changed, thanks.

- Line 134-135: was these data also corrected taking into account the tide level during the tsunami? If the level was higher during the measurements, how was the underwater part of the bathymetry measured?

No, this was only corrected taking into account the tidal level at the time of the survey (which is normal in such surveys).

- Line 164: remove first “and”

Corrected.

- Figure 3: please add legend of blue line in the picture

This is now indicated “Continuous blue lines show the track of the two survey boats”.

- Figure 6: it would be cleared for the reader to clearly indicate that the first 3 measurements were conducted before the tsunami and the last 2 after the tsunami.

The following was added to the caption of the figure “AW3D, DENMAS and SRTM represent the data available prior to the tsunami, with UAV data representing the authors’ own surveys.”

- Line 284: what explanation do the authors give for such difference with the numerical results of Grilli et al. (2019) ?

See next comment.

- Line 288: could this difference in mountain volume be responsible for the failure in estimating the run-up heights (line 284)

This was poorly explained in the text, but yes, this is what the authors meant. The following note was now added “and could at least partly indicate the difference in run-up heights).”

- Line 306: propagation (space missing) (due…

Thanks, corrected.

- Line 320: do the authors have a gross estimation of the error in the run up height?

No, this is not really possible to do. Basically, the terrain was very difficult, and it was already difficult to try to understand what had happened. Providing a gross estimation of the error would indicate a false sense of confidence.

- Line 335: clarify what the authors mean with “elsewhere”

This was changed to “in LCDV (2015)”

- Line 419 and 450: for consistency, references non in capital letters.

Changed to lowercase.