

Reply to the comments from Referee #1:

In blue: reviewers' comments

In red: authors' reply

The manuscript, although original, seems to be a replication of the one published by the authors on August 22, 2017 in local language entitled "Investigation of awareness of typhoon and storm surge in the Mekong Delta – recollection of 1997 Typhoon Linda" doi: 10.2208/jscejoe.73.I_168, for which, only the abstract is available in English. For this reason, it is very difficult to understand if this is an original publication overall. This is confirmed by the fact that, according to the abstract insights, the authors come up with completely opposite conclusions regarding people's awareness.

We have been investigating this particular typhoon for last several years. Our previous paper using the name of the same typhoon in its title may have given such impression to the reviewer. However, methodologies, data, study locations, questionnaire used, and main conclusions in the present paper are substantially different from those in the previous paper. For your reference we uploaded the manuscript of the following paper to the RG server, though this will become open-access for public one year after the publication. We would appreciate if the reviewer confirms that our present paper is not a replication of the one already published.

Investigation of Awareness of Typhoon and Storm Surge in the Mekong Delta – Recollection of 1997 Typhoon Linda, Anh L. T., Takagi H, Thao N. D., Esteban M., *Journal of Japan Society of Civil Engineers*, Ser. B3 (Ocean Engineering), Vol.73, No.2, 2017.

https://www.researchgate.net/publication/316989654_Investigation_of_Awareness_of_Typhoon_and_Storm_Surge_in_the_Mekong_Delta_-_Recollection_of_1997_Typhoon_Linda?

There is an inadequate description of methods, an insufficient explanation of the rational of the study coming from an insufficient literature review that does not establish the background of the problem studied. Some of the results are poorly described (probably grounded on a poor questionnaire design) as the discussion that sometimes is out of context. There is a failure in emphasizing the impact of the study in a broad disaster management context, environmental and social planning.

We sincerely consider these reviewer's critical comments and will revise where possible.

Rather than investigating the overall awareness, authors analysed the role of memory that could have an influence on people's awareness 20 years later. The title is very effective, but the two different time periods of the survey bias the title timeframe. In addition, the survey has been enlarged to increase sample size, but neglects to consider that the not-exposed people added in the second survey are distorting the results, that therefore cannot be generalized. Some analysis about

exposed and no-exposed people should be added in this regard.

If the editor accepts, I would like to change the title as follows in response to this comment:

Original: *1997 Typhoon Linda Storm Surge and People's Awareness 20 Years Later: Uninvestigated Worst Storm Event in the Mekong Delta*

Proposed: *1997 Typhoon Linda Storm Surge and People's Memory 20 Years Later: Uninvestigated Worst Storm Event in the Mekong Delta*

Besides, in the revised manuscript the difference in response between exposed and not-exposed people in Con Dao Island will be addressed.

The closed questions asked do not provide any qualitative background to justify the effect of memory of Linda and their past decision to evacuate. The knowledge of typhoon storm surge has been investigated with a dichotomous question that does not provide any deeper interpretation about their awareness. The authors did not check if people's perceived knowledge was reflecting the real definition. What is the meaning of adding all these broad questions without a clear framework in mind?

We sincerely consider these reviewer's critical comments and will revise where possible.

Which were the hypotheses the authors wanted to test?

There are no statistics that support the findings. Why not cross-connect the results with each other in an attempt to identify patterns? Only from the footnote of Table 2 is it evident that some statistics have been done.

We hypothesized that the degree of awareness and memory among local people will be dependent on whether their locations were directly hit by Typhoon Linda or not. In this regard, we have already tested those difference between Con Dao and Can Tho by applying Chi-Square test, as shown in the footnote of Table 2. However, we didn't clarify the details on this analysis because significance appears to be obvious even without any statistical test. In the revised manuscript, we will try to provide more explanation about statistical characteristics to respond to the reviewer's comment.

According to the basic demographics, 10 year-old-classes have been assessed and then discussed in the results as young, adult and old. This mismatch is not clear.

Sorry for the inconsistency between the demographics and the description in the text. The manuscript will be revised to correlate them in a more consistent way.

In addition, do interviewee occupations have any impact on understanding the results?

It may not be so important statistically. However, such demographics should be useful for readers in understanding basic characteristics of interviewee. The information remains undeleted, while more discussion will be added in the revised manuscript.

Some of the results have been presented for the first time in the discussion (e.g. page 9 lines 17-22; page 10 lines 1-2 and 5-6), that generally appear to be poorly described and concluding to some statements that are not supported at all from the results obtained (e.g. page 7, line 8-9; page 8 lines

12-16). The discussion should provide answers to hypotheses, and if so, to interpret the findings. In addition, set the paper in the context of others work is the key to achieve a complete view of the problem under study. The implications of the findings should be discussed within a realistic framework (e.g. disaster management, socio-political background, awareness, role of trust, evacuation, message delivering, etc.). Additionally, specific discussion chapters seems to be totally out of context (e.g. Sea level rise and land subsidence) and are not connected to each other (e.g. Population increase in MD) that are further poorly described. In addition, the increased population means that there could be an increase in exposure rather than vulnerability. The Low frequency but high impact typhoon hazard chapter needs to be more articulated with the characteristics of Linda rather than recall other events in relation to disaster characterizations. Also, there seems to be some confusion about the disaster terminology evidencing an unclear understanding of the risk perception analysis developed further.

The reviewer pointed out the discussions without supporting information. Even if not fully supported by evidence, we believe that the Discussion Section can be used for addressing authors' views from their studies to explain the meaning of the results to the reader. Nevertheless, we thank these reviewer's critical comments and will revise the manuscript accordingly.

The methodology needs some detailing regarding the TC Landfall Analysis and the Tidal Analysis (see the comments on the file). This concerns even the field survey. Has the questionnaire been pre-tested? What is the literature consulted by the authors for developing the awareness questionnaire? What is the representativeness of the sample interviewed? How people have been recruited and where? Any bias related to the form of recruitment?

We agree that the paper should be further improved by describing more detailed explanations on the basis of methodologies used.

Conclusions need to include both the characterization of Linda than the awareness findings, trying to underline the relevance of this study and its socio-political implications. Prove to the reader, and the scientific community, that your findings are noteworthy. The abstract needs to be enriched with the methodology used. I suggest revising the choice of keywords (see comments on the file).

We thank the reviewer for these comments. Yes, we agree that the Conclusion as well as Abstract should be further improved by describing more findings and methodology noteworthy for the reader.

There is a collage of pictures without any caption/title/reference to the text on page 14.

What the collage was made for?

Sorry for the collage that abruptly appeared. This was initially intended to combine with the main body of Figure 2. However, it has been divided into 2 pages because of a space limitation. These photos that show scenery of interviewing are not fundamentally important, and will be removed from the revised manuscript.

I think that Figure 2 could be mapped with a GIS software. Google Earth map does not give a

comprehensible overview. The spatial reference for the smallest pictures is not visible at all. The resolution is also a critical point.

I think Google Earth Pro is still one of the best tools for simply presenting a satellite map. A GIS software may not greatly improve the resolution unless the background satellite image is much improved. At least, we will try to replace the close-up image with a more visible one.

I think it would be useful to understand at which speed the typhoon came across each location evidenced in the map. Besides, all the story telling in the caption should be put in the text and not in the caption, as highlighted in the file attached. Specify in the caption the meaning of the blue line.

We thank the reviewer for the idea of indicating speeds when the typhoon passed through. We will do it in the revised manuscript. The points raised about the caption will also be modified accordingly.

Fig. 6 has been never cited in the results. In addition, it is linked with a set of results coming from another study never being published but conducted by the authors. What's the meaning of adding such information? This is not scientifically acceptable.

As the reviewer criticized, we acknowledge that this figure is not directly relevant with the main topic of the present paper. We will remove Fig. 6 and related discussion from the revised manuscript.

Fig. 1 to 3 could be improved by lettering each figure and relating them all into the caption.

We agree. These figures will be revised to clearly correlate the figures to each other.

All the station names in Table 1 appear to be stand-alone without any reference in the text. I think a map could improve the understanding of the table and the analysis.

We thank the review for pointing this out. The caption of Table 1 will be improved, as the reader should understand the geographical locations of all these towns.

Some of the results presented in Table 2 do not match the results in the text (e.g. lines 11-12 of page 6).

The reviewer mentioned the following sentence:

“Many local residents, who lived by the river, recalled that their houses were inundated with about 0.2–0.5 m (Table 2).”

This description will be modified to be consistent more with the results in Table 2.
