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Interactive comment on "Development of a decision support system for tsunami evacuation in the South China Sea region" *by* Jingming Hou et al.

Anonymous Referee #1

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Reviewer Comments for Manuscript Number nhess-2016-319

Title: Development of a decision support system for tsunami evacuation in the South China Sea region

Authors: Jingming Hou, Ye Yuan, Peitao Wang, Zhiyuan Ren, and Xiaojuan Li

Manuscript Type: Research article

Dear Editor and Authors,

General comments:

Firstly, thank you for submitting this paper and giving me the opportunity to review it.

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Discussion paper

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Please, consider all comments and suggestions simply as the opinion of this reviewer.

This document addresses the development of a decision support system for tsunami evacuation and its application to the Jiyang District of Sanaya City, located at the southern tip of Hainan Island (South China region).

From the reviewer's point of view, both the tsunami evacuation itself and the development of an evacuation-related decision support system are topics of great interest since this is a relevant key issue related to tsunami risk for communities affected or potentially affected by this threat.

However, the general comment for the whole paper is that the reviewer has not been able to find any significant point regarding the principal criteria of the reviewing process (scientific significance, scientific quality and presentation quality).

There is no clarity in all the process initially described, no description of how each one of the analyses have been performed (there is no possibility for replicability) there is no description of results either and almost no conclusions.

I think that there is no connection between the different parts of the process. Apparently, one feed each other but this is not shown throughout the document.

Considering the above mentioned and after reflection, the final consideration for the review is: major revisions.

Below, there are also some specific comments intended to contribute to the improvement of the article, but the general recommendation is to "rethink" the document.

Specific comments:

Lines 61-62 It is the reviewer opinion that agent based model is not developed for tsunami drills. There may be relation between AB model and tsunami drills but it is not the "objective" of the modelling.

Lines 62-66:

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It seems that this a justification for choosing LCD model but I would recommend to consider the revision of these sentences. The idea is not clear.

Figure 1: The population distribution is not included in the framework.

Line 73: I think that “effected” should be replaced by “affected”

Lines 73-74: The same idea is repeated in two consecutive sentences.

Lines 89-90: It is difficult to see the coherence between the idea of modern earthquake records overlooking the tsunami potential and that “Modern seismic analysis suggests that the 1918 Morro Bay and 1934 Luzon earthquakes were larger than their officially reported magnitudes”.

Lines 97-101: Is not clear what else is done after considering a “number of potential tsunami scenarios. . .” Are they simulated? Are they collected?

Figure 3: This seems to be the location map of the study area. However the study area is not clear (scale is very small) and there is that green line and the epicenter. I would recommend to include a figure with much more detail of the study area and the other one (together with figure 4, for example) related to the travel time calculations.

Lines 117-119: This is not a formal definition of evacuation time, but the interpretation of the authors. I miss either saying that the definition provided is how it is understood in this paper or the reference that has been taken.

Lines 117-130: The calculation performed to establish the tsunami travel times is not clearly described. I believe that the method should be replicable but any other person reading the document: for instance, How is the average water depth determined? epicenter coordinates? Results (2 hours) may be good as a rough approximation but I do not believe they are comparable (at least in general terms) with numerical tsunami travel time models. The approximation will be acceptable depending also on the purpose followed. Maybe for evacuation purposes in a decision support system in a local area this is not the best. On the other hand, it could be debatable if average velocity is

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appropriate to estimate the time. . .

Lines 147-195: From this reviewer's point of view, vulnerability is not really being considered and is not properly defined. Maybe it could reshaped on some way explaining that some aspects that could influence in the vulnerability of the system are taken into account. . .

There is confusion in the statement of lines 151-152

Paragraphs following the line 156 should follow the order detailed in the sentence, for an easier reading.

Line 160 and Line 168: I really believe that elevation is not a preferred consideration for vulnerability analysis. It is a crucial parameter to establish the affected area but that is hazard and exposure.

Lines 186 to 189. Is the "8 km distance from shore" calculated based on the previous equation? This is not clearly stated.

Lines 192 to 195: It is true but has it been done in the frame of this work? If yes, please show results. If not, clarify it. Is really the vulnerability being calculated or estimated in some way? Is there any result achieve?

Line 218: This is not very relevant when describing land use to estimate the impedance related to evacuation analysis

Line 224: There is no explanation of the figure.

Lines 236-237: Congestion-prone roads are calculated based on population census and road classification data. Which is the approach followed? There is no explanation. It seems obvious than "worst roads" and most densely populated areas will have more congestion-prone roads, but it is no explained. Results are not explained either.

Lines 241-246: It is no clear if vertical shelters are added to the system based on calculations that are not the objective of this paper or if this have been addressed in this

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work. It is stated, as the only apparent reason, that evacuation shelters are usually selected “following the principles of good accessibility and large capacity, among others”. This is a quite poor reasoning. As figure 10 has not been explained, the reader is not able to understand how those green points are calculated. On the other hand, if there has not been inundation modelling, which is the procedure followed to locate them?

Conclusions: I think they should be remade after a careful revision of the article.

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