

Nat. Hazards Earth Syst. Sci. Discuss., referee comment RC2  
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## **Comment on nhess-2021-381**

Anonymous Referee #2

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Referee comment on "Modelling the sequential earthquake–tsunami response of coastal road embankment infrastructure" by Azucena Román-de la Sancha et al., Nat. Hazards Earth Syst. Sci. Discuss., <https://doi.org/10.5194/nhess-2021-381-RC2>, 2022

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The paper proposes a very relevant and interesting approach due to the concomitant approach of two processes that are usually analysed separately and in isolation: the seismic component and the hydrodynamic component of the tsunami.

The reader will find a new methodological approach to better understand and assess in more detail the possible infrastructural impacts occurring in an area exposed to seismic and tsunami events.

The paper is clear and well ordered, however there are certain aspects in the discussion and presentation of results that need to be improved in order to provide greater clarity in the research. I therefore consider that the paper should be accepted once the following (minor / technical) changes are corrected and the explanation extended.

- A) The title of the paper is too general as it may confuse the reader that the methodology presented is suitable (or may be suitable) for any "urban transport infrastructure", when in fact it applies to a very particular typology called "road embankment" which is a perimeter-oriented exposed on the coast, therefore I recommend changing the title of the paper to "Modelling the sequential earthquake–tsunami response of coastal road embankment infrastructure".
- B) For the tsunami wave simulation part, the authors do not explain in sufficient detail general aspects of the model set-up, the definition of the theoretical vs. real forcing wave, mesh resolution vs. bathymetry, the use of very low resolution bathymetry such as GEBCO, and the transient and 3D processes that the tsunami wave would experience on the coast under study. The authors are therefore invited to make a more detailed discussion of these aspects, especially the implication of approximating the analysis to a single coastal profile in a markedly 3D environment, what considerations/hypotheses are taken into account, is the 2D approximation sufficient, can the bathymetry used adequately represent these detailed processes, is the 2D approximation sufficient, and can the bathymetry used adequately represent these detailed processes? Is the error

committed by the use of low resolution bathymetry greater than the quantified results of the simultaneous earthquake+tsunami process? For example.

- C) In the final discussion of the paper, the authors do not really make clear or quantify what improvement is achieved by considering the simultaneous seismic+tsunami methodology, compared to a more traditional, decoupled approach. There is a description that attempts to clarify this point, but it is not entirely clear. Please include an in-deep discussion and a quantification of these effects.
- D) Finally, the authors do not carry out any self-critical work on the method in relation to the limitations it may have, when trying to apply it to different places in the world, with different coastal protection structures, in markedly 3D environments, etc. If no comment is included in this regard it would seem that the method can only be applied to Manzanillo or areas of the world that are similar in infrastructure (?).

Minor changes:

Figure 3 is not referenced within the text.

Figures 6 and 7, could be merged into 1, this would be better understood.

Figure 26. Not clear what the colours are?