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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Speeding up and boosting tsunami warning in Chile" *by* Mauricio Fuentes et al.

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Dear Reviewer,

Santiago of Chile, April 11, 2019

We have read carefully your review of our article entitled, "Speeding up and boosting tsunami warning in Chile", written by Fuentes M.(1), Arriola, S. (2), Riquelme S. (2), and Delouis B. (3), from (1) Department of Geophysics, University of Chile, Faculty of Physical and Mathematical Sciences, Santiago, Chile, (2) National Seismological Center, University of Chile, Santiago, Chile and (3) Géoazur, Université de Nice Sophia Antipolis, Observatoire de la Côte d'Azur, Nice, France.

We are grateful for the time you spent to review our paper, for all your comments and

useful suggestions to improve the manuscript. In the following paragraphs we present in detail the answer to all questions, comments and suggestions you made.

Best regards, Mauricio Fuentes.

——— General comments

Reviewer: The paper discusses a rapid estimation of potential tsunami energy distribution along the coast (or at certain isobath) from any given earthquake parameters. As a typical forecasting algorithm, the main issue here is the tradeoff between the speed and accuracy. To obtain a timely warning, the proposed algorithm uses a rough source estimate from the W-phase inversion as well as a linear tsunami model. Despite the simplifications, the model produces a sufficient level of accuracy to facilitate the early warning system. Additionally, the proposed method is rigorously tested against historical tsunami events, which is another important factor of this paper that make it worthy of publication. In general, the paper is well-written (except for the discussion and conclusions section) and the main message to convey is easy to follow. However, I would recommend further clarifications in some parts, which can be found in the following specific comments, before the paper can be accepted for publication.

Response: We provided an annotated version of the manuscript with track of changes (red slanted stands for deleted text and blue for new text.) including all your suggestions.

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Specific comments:

(1)

Reviewer: Page 1 Line 8. "Our results show that ... non-linear tsunami code." The sentence can be misleading. I would suggest to revise it into "Our results show that, at a certain water depth, this linear method : : :".

Answer: We have included this suggestion.

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(2)

Reviewer: Page 2 Line 1. ": : : are based on precomputed scenarios". Adding a sentence here with reference to the previous related works would better justify the statement. Here are some papers that can be considered:

Reymond, D., Okal, E. A., Hébert, H., & Bourdet, M. (2012). Rapid forecast of tsunami wave heights from a database of pre-computed simulations, and application during the 2011 Tohoku tsunami in French Polynesia. Geophysical Research Letters, 39(11).

Gusman, A.R., Tanioka, Y., MacInnes, B.T. & Tsushima, H., 2014. A methodology for near-field tsunami inundation forecasting: Application to the 2011 Tohoku tsunami, J. geophys. Res.: Solid Earth, 119(11), 8186–8206.

Mulia, I. E., Gusman, A. R., & Satake, K., 2018. Alternative to non-linear model for simulating tsunami inundation in real-time. Geophysical Journal International, 214(3), 2002-2013.

Answer: We have included the new references and a sentence to mention them in order to improve the previous statement.

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(3)

Reviewer: Page 3 Equation 1. The first line of the equations is a linearized SWE, and the second line refers to the initial condition. What about the third line? Derivative of

elevations with respect to time at t = 0?

Answer: The first line is the linearized SWE, second and third line are the initial conditions. The second is the initial wave and the third is the equivalent condition for null initial velocity, which is the standard formulation in the static coseismic displacement approach.

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(4)

Reviewer: Page 3 Line 19. The tsunami propagation is limited at 100 m isobath, while in the supporting information the simulated runups are compared with the actual runup observations. I am aware that the paper aims to estimate the possible runup distribution in general by disregarding the physic of nearshore processes due to the nature of the algorithm. However, such an inconsistent comparison needs to be clearly defined. For example, by including the 100 m water depth contour on the plots and additional sentences in the figures caption explaining about the difference of runup locations between observation and model. Or, better yet, why don't use Green's Law as in the Raymond et al. (2012)?

Answer: As the reviewer correctly pointed-out, the linear estimation uses a "linear run-up" estimation which is the case of a reflective vertical wall boundary condition. Roughly, linear and non-linear approaches should be on the same order (Synolakis, 1987; Synolakis, 1991). Certainly, the approach of Reymond et al. (2012) is valid, however we aimed to keep our approach as straightforward as possible, an also, as it was notice by Synolakis (1987), this kind of boundary condition somehow retrieve the Green's law. There is no way to predict detailed run-up heights without a fully coupled non-linear method nor a high-resolution bathymetry, which is out of scope on this work. We have added some sentences to make this clear as well we have added minor modifications in the figures.

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(5)

Reviewer: Page 5 Table 1. The use of "lon" and "lat" is rather confusing without seeing the corresponding figures. I suggest to add a reference to the supporting information in the Table caption, though it has been mentioned somewhere in the text. Furthermore, mathematical formulation of the correlation coefficient can also be a good addition for the supporting information.

Answer: We have added this reference.

(6)

Reviewer: Page 6 Table 2. Please explain why the computational time of the elliptic slip distribution is longer than the FFM? From the figures in the supporting information I can see that the elliptic slip models have a smaller subfault size. If that is the case, information on the subfault size should be added in section 2.1 including the reasons for using finer resolution in the elliptic slip model. Also, tT in the caption is written tR on the table.

Answer: The reviewer is right. The size element for the elliptic sources is in general smaller than the FFM. This is to ensure enough resolution on the source model. The typo "tR" was fixed. We have added some sentences making this clear.

(7)

Reviewer: Page 6 Line 15. It is difficult to grasp the meaning of the last sentence. Please rewrite it.

Answer: The whole paragraph has been rewritten.

(8)

Reviewer: Page 7. The flow of descriptions in the discussion and conclusions section is not very smooth. Improvements can be done by either rewriting the whole paragraphs or using bullet-points or numbers to indicate different topic of discussion.

Answer: The whole section has been rewritten.

(9)

Reviewer: For the Java case (Figure S7), it seems like the fault of the elliptic source is located seaward of the trench (in the outer rise region). If this is true, then the model needs to be revised, because the 2006 West Java event was a shallow interplate earthquake (a typical tsunami earthquake), which is better depicted in the FFM solution (Figure S8). Other than that, the Java Island map in the left panel is inaccurate. I believe this may be caused by a wrong color map scale used for plotting. Please also check the other locations.

Answer: Thank you very much for noticing this mistake. This it was a misunderstood when typing the data with a closer event in the same area. We have verified the whole catalog and we have fixed this problem and remaking this scenario and figure.

Please also note the supplement to this comment:
https://www.nat-hazards-earth-syst-sci-discuss.net/nhess-2019-9/nhess-2019-9-AC2-supplement.pdf

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