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

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Dear Dr Babeyko,

Santiago of Chile, April 00, 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

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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:

Two remarks which may improve the paper:

(1) I see a logical inconsistency in this Manuscript. On one hand, Authors note that near-field tsunami heights are highly sensitive to slip distribution (e.g., Geist, 2002) and, accordingly, criticize pre-computed scenario databanks for their simplified slip models. On another hand, Authors propose to apply fast W-phase source model for real-time forecasting. But, W-phase CMT is a point source model; there is no slip heterogeneity inside that model. Whether slip distribution is assumed to be uniform, or "elliptical" – makes no difference in sense of source complexity. (One could pre-compute scenario databank with elliptical sources as well). Fast propagation simulations may improve local early warning only in combination with fast complex source inversion (i.e., slip distribution). Authors do mention FFM modeling on their final flow chart, but there is no any discussion about how they are going to perform fast FFM inversion in 5-10 minutes.

(2) Authors propose linear propagation modeling to forecast tsunami coastal impact. They mention terms "run-ups" and "maximum inundation". However, both run-up and inundation take place on land whereas linear propagation is stopped at 100 m depth. There is no any explanation in the manuscript of how do Authors close the gap between the 100m-isobath and run-ups. Simple Green‘s law? Such an explanation is very important and must be present in the paper. Same for the non-linear simulations with JAGURS – did they compute the "true runup“ over inundated topography (at 15 arc second resolution)? Or extrapolated from some offshore isobath as well? What is then

exactly compared between the two models?

Answer:

(1) It is a good observation. That is the reason we try to give one step in-between the uniform slip models and FFM which are not currently available in real time. Elliptical models for the same magnitude, allows to concentrate more slip in a given region than the uniform slip, which turns this elliptical model "worse" (in terms of threat) than the uniform one.

The scope of this work is on the power of the linear solution, on the reference list you will find the articles that decribe the "why and how" of obtaining FFM models within 5 min.

(2) We have made a new version of the manuscript that better explains this point. However, the problem is the same for both, linear and non-linear since we don't have fine bathymetry data to nest grids and "truly" compute the run-up/inundation . The approximated run-up is just the peak that is registered in the vertical wall (boundary condition). If users would want to use finer bathymetry, the ideas proposed here will be the same.

Please also note the supplement to this comment:
https://www.nat-hazards-earth-syst-sci-discuss.net/nhess-2019-9/nhess-2019-9-AC5-supplement.pdf

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