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Interactive comment on "The effect of uncertainty in earthquake fault parameters on the maximum wave height from a tsunami propagation model" *by* D. Burbidge et al.

Anonymous Referee #1

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Review of the paper entitled "The effect of uncertainty in earthquake fault parameters on the maximum wave height from a tsunami propagation model" by D. Burbidge, C. Mueller, and W. Power, NHESS discussion paper

The present document contains a review of the discussion paper entitled "The effect of uncertainty in earthquake fault parameters on the maximum wave height from a tsunami propagation model" by D. Burbdge, C. Mueller, and W. Power. The manuscript contains a sensitivity study of the effect of some of the basic source geometric fault parameters from earthquake tsunamis on the tsunami uncertainty. It starts with a series of simple studies with standard fault tsunami sources in a homogeneous medium,

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studying the uncertainty propagation in different direction from the source. Then, increased complexity is added to the model, and it is demonstrated that the tsunami uncertainty does not display a simple relationship as a function of the fault parameters. The methodology and structure of the paper are sound and easy follow. The manuscript is a useful addition to the tsunami literature, and should be suitable for publication in NHESS, after subject to moderate revision. The main suggestions for improvement are listed under the general comments below, with main concerns first. Also, some specific line-by-line comments are provided.

General comments

The study largely concludes that fault variability cannot be modeled through a simple aleatory uncertainty parameter. The uncertainty propagation, measured through the coefficient of variation becomes complex even for relatively simple test cases. While there is reason to believe that the authors overall conclusion is generally correct, some elements of their analysis call for a bit more subtle analysis and discussion. First, the conclusion hinges on the selection of a set of uncertainty values σ. These are only loosely supported, in most cases stated by a single reference. Their representativeness as "global" uncertainty measures are therefore somewhat doubtful, unless subject to a more rigorous discussion. For instance, is it likely that the σstrike is independent of the magnitude, a larger magnitude earthquake would generally be more constrained? On the other end, the variation in the fault depth is probably too limited, as the authors state. Another implication of the analysis is the use of an unbounded (normal) distribution, which may impose large anomalous values of H, for instance in the case of a large strike angle. I could probably add several other assumptions. Nevertheless, the point is that the discussion should clearly illuminate how these assumptions influence the results. It would be desirable if the analysis and discussion of the different patterns that arises from the ensemble simulations could be a bit more elaborate. A possible and suggested addition to demonstrate the sensitivity of their results, would also be to provide additional studies with reduced σ values for 1-2, for instance for the most simple cases to see if more transparent results appear.

The selection of the COV parameter as the main uncertainty measure is supported by this referee. However, it is still difficult to read many of the COV color maps that are provided. To remedy, I suggest to provide plots that projects the COV in terms of $\mu \pm \sigma$ along the latitude (or longitude) axis in the main wave radiation direction, at least for a couple of examples. This also add some additional useful information; namely the relevance of the variation. This could also illuminate where large values of the COV arises because of shadow regions with small mean values for instance.

Much of the analysis ends with the discussion of the use of aleatory uncertainties in tsunami hazard analysis and related application. Theoretically, the aleatory uncertainty should be the one inherent in the natural process, while the epistemic is the one related to our lack of knowledge. In practice however, and it seems indirectly to be the one assumed here, we take the epistemic uncertainty as the scientific expert judgement, whereas the aleatory are those that may derived quantitatively from evidence, through for instance a set of σ values to be included in the PDF of the tsunami metric. A clear borderline between the two is often difficult to distinguish. Given the strong focus on the aleatory uncertainty, a more in-depth discussion of the two types of uncertainties and their use in PTHA should be included. I've been pointed to Marzocchi and Jordan (2014) as a useful reference to the subject.

Some additional references should preferably be added to the introduction. Basic properties of tsunami generation from earthquakes are reviewed in a series of papers by Geist (1999, 2010, 2012), and review some of them may be useful. On the discussion on heterogeneous slip, I suggest adding references such as McCloskey et al. (2007, 2008) and Goda et al. (2014, 2015), the latter two demonstrating the effect of the COV parameter alongshore the northeastern Japanese coastline as well as fault variability. On PTHA, add reference to Lorito et al. (2015). A more general discussion of PTHA is also suggested. On the effect of friction during overland flow, please add a reference to Kaiser et al. (2011).

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Line-by-line comments

Page 3370, line 11: It is difficult to read the sentence starting with "The relationships.." without having read the entire paper. Please revise for clarity.

Page 3371, line 9: It would be useful to add that the relevance of dispersion are related to a time and length scale, see for instance Glimsdal et al. (2013).

Page 3371, last paragraph: I strongly suggest discussing the PTHA and the treatment of uncertainty in PTHA a little more elaborately here. The premise of the treating the aleatory uncertainty in a simple way, namely a linear behavior, should preferably be mentioned.

Page 3377, line 13-14. A depth of 10 km on a fault with uniform slip would generate an artificial peak in the initial displacement. This will have bearings on the analysis, that needs to be mentioned.

Page 3377, lines 15-20. Some of the most careful studies of the Japan earthquake and tsunami states that strike slip have a notable influence of the generate tsunami, which may be of some relevance here.

Page 3379, line 4, title, Replace "Chile 2010" with "2010 Maule earthquake and tsunami"

Page 3379, section 3.1. It is somewhat difficult so see the relevance of this section here, particularly given the vague comparisons that have been shown. First, the Easy-wave code must surely have been validated previously? Second, I do not the see the relevance of including figure 1 given the scope of the paper. Third, the figure 2 is of poor quality and hard to read, with too small panel, and finally, the comparisons are in fact not very favorable, given the simplicity of the hydrodynamic problem involved. Related to the latter comments, why does the two codes exhibit phase differences? If Easywave is validated elsewhere, I would rather cut this subsection.

Page 3380, line 15. What do the authors mean by "... neglected for distances up to

40°"? Comparisons with other parameters such as the Rossby radius is probably more meaningful.

Page 3383, line 6. This σ value seems too low compared to the other parameters. In this case, the normal distribution cannot not be the correct one due to the constraint of the surface, which needs to be mentioned / discussed. More generally, the choices of the PDF for the fault parameters could benefit from some discussion.

Page 3386, line 15. What is the beam? Please describe more explicitly.

Page 3386, line 20. A somewhat more in-depth discussion of the mechanism behind the variability pattern for this example and others would be highly appreciated. The discussion section spends only one paragraph on discussing the patterns, and is mostly devoted to general discussion and limitations. The discussion should be more balanced and pay more attention to the simulations that have been done. This will not alter the conclusions, but add more value to the paper. See also some of the suggestions in the general section.

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Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 3, 3369, 2015.