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Natural Hazards and Earth System Sciences
Discussions

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.

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Thank you for your review. Overall, we agree with most of the comments. However, we feel that the amount of additional work some of the suggested improvements would require is considerable and are unlikely to change the main conclusion of this work. Adding, for example, a new set of models with a different PDF for the inputs would greatly increase the number of models we would have to run and the number of figures. However, the effect of any uncertainty in earthquake fault parameters on maximum wave height would still end up being very complex, probably even more complex. Given that the other reviewers did not feel any extra models were necessary we would argue

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Interactive Discussion

Discussion Paper

that amount of models in the current version of the paper is adequate. We also note that even this reviewer feels that our “overall conclusion is generally correct” even without the additional models that they suggest we do.

For the rest of the comments we have added detailed responses to the text below in paragraphs starting with the word “Response”. Text from the original comment is indicated by quotation marks.

"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. Burbidge, 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, 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. These are only

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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 strike is independent of the magnitude, a larger magnitude earthquake would generally be more constrained?"

Response. We agree that the selection of these particular standard deviation values is fairly arbitrary, although we feel they are not completely unreasonable numbers as the references demonstrate. However, we would argue that our conclusion does not depend on the particular values shown here. For example, in Figure 4d we show the CoV map when the standard deviation for strike is reduced from 10 degrees to 5 degrees. The pattern is still complex even with this change.

Response: For the second point, yes the strike of larger earthquakes are more likely to be known as the magnitude increases since the larger earthquakes are more likely to be a known fault (eg a subduction zone). However, we see no way of knowing how much it would reduce as the magnitude increases. In other words, we do not of any existing relationship we can use between the standard deviation for strike and magnitude. We would probably have to make one up but it would be even more arbitrary than our current choice of standard deviation values. It would also make interpreting the figures more complicated and would require another large set of models to characterise. We feel this would better as a topic for another paper. Nor do we feel that it is necessary to do this in order to demonstrate the main conclusion. The idea here was to see the effect of a simple uncertainty model that was the same for all the input earthquake parameters.

"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

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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 sigma values for 1-2, for instance for the most simple cases to see if more transparent results appear."

Response: It is true that the details of the variation probably depend on the assumptions used, for example whether the PDF for the parameters in an unbounded normal distribution or not. We agree that it would be interesting to see the effect of the choice of shape of the distribution might have on the results. However, this would involve running even more models, at least several hundred, depending on the thoroughness of the comparison. In order to make this problem tractable we had to limit its scale. A thorough investigation of (for example, PDFs) could be a topic for another paper. Again, we don't see that adding more models would change the main conclusion. This paper is only meant to describe the main effect, i.e. the complexity of patterns arriving from even a relatively simple uncertainty model describing the source parameters.

Response: For the second suggestion, we have already reduced the standard deviation values for one example. As mentioned above, the standard deviation of the strike was changed from 10 degrees (Figure 4a-c) to 5 degrees in Figure 4d. It did not make the results more transparent and further examples for different parameters are unlikely to do so. Again, we could do many more examples but this would require a lot more modelling and would not, in our view, change the main conclusion of the paper.

"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 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."

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Response: Adding some extra figures which show a cross section of the COV maps is certainly possible. However, it is not clear to us how much this would actually add. Firstly, we feel that the CoV maps are sufficiently clear to demonstrate the argument. We note that the other reviewers did not mention that the CoV maps were difficult to read. Second, the maps vary a lot spatially so we are likely to need a lot of different cross-sections and thus figures would be busy and/or we would a lot of them. All of which would show the same thing as the maps do but in a different way. Third, it is not clear to us what the reviewer means by “relevance of the variation”? Relevance to what exactly? Finally, for the last sentence, a cross section of the COV alone wouldn’t show where the COV is large because the mean value is small. You would need to show the mean value as well, which would make these figures busy. However, we would argue that the regions where the mean value is small are in fact fairly clear. They are the purple to blue areas in Figure 3. In summary, we can certainly add these figures if the editor agrees with this reviewer but we are not sure that they would add enough to the paper to justify the extra length.

"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 sigma 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."

Response: OK. We assumed most readers would be familiar with the difference. We can certainly add some discussion of the difference for the benefit of readers not famil-

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iar with the terms.

"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)."

Response: Thanks a lot for the references, we are happy to add them in to the final paper.

"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."

Response: Changed from “The relationships become. . .” to “The relationships between these parameters and CoV become. . .”

"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)."

Response: OK. Thanks for the reference.

"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."

Response: OK (see above).

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"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."

Response: OK, we will add a mention of this point to the text.

"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."

Response: OK, but we would note that the importance of rake to tsunami generation has been well understood since well before the Japan tsunami.

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

Response: OK.

"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."

Response: Actually, EasyWave hasn't been validated in many papers before this which is why this section is here. However we agree that we could cut this section without losing much to the paper. It isn't the main focus of this work.

"Page 3380, line 15. What do the authors mean by "... neglected for distances up to"? Comparisons with other parameters such as the Rossby radius is probably more meaningful."

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Response: True, but this section could simply be cut instead. See above.

"Page 3383, line 6. This sigma 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."

Response: Yes, but the correct distribution is not known. In the absence of anything better, we chose the normal distribution for the standard deviation for depth to keep it similar to the other parameters. We do not know the effect of the choice of PDF on the results, that would require hundreds more models (at least) and is beyond the scope of this paper. More generally, we chose a normal distribution as that is a simple, commonly used means for characterising the uncertainty in a set of observations. We will mention that in the text.

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

Response: It is main direction of propagation for the tsunami. Text will be added to this effect.

"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."

Response: We feel we have explained the mechanisms as much as we can; the first paragraph covered strike, the second dip, the third magnitude and the fourth depth. Beyond that the mechanisms are not known to us. We also feel that the main point is not so much the details of the mechanisms but potential implications for PTHAs,

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warnings etc. Given the importance of these implications, we feel that the discussion is fairly balanced as it currently stands.

"References not in the manuscript ..."

Response: Thanks for all the references. We will add them.

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