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Discussions

Interactive comment on "Fast evaluation of tsunami scenarios: uncertainty assessment for a Mediterranean Sea database" *by* I. Molinari et al.

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Dear Dr. Armigliato,

we appreciate your careful and constructive review that helped us to substantially improve the manuscript. We indeed answered to all your comments and we adjusted the manuscript accordingly to most of your suggestions.

We would like to point out that, further than the suggested modifications, we made some progress in the reconstruction method of the initial displacement field that we added to the revised paper that we are now submitting. For this reason, there is now a new paragraph ("Improvement of the initial field reconstruction"). This new part allows us to obtain an unbiased distribution of the misfit and, in our opinion, this strengthens the structure of the paper. We also slightly modified our discussion/conclusions accord-

ingly to the new findings. Also, numerical values in Figure 3 are now slightly changed with respect to the submitted version: this is due to the removal of a few tens observation points (OPs), because we found that they were erroneously placed in too shallow waters (10-20 m). This has the effect of eliminating some outliers in the distribution of the reconstructed waveforms and amplitudes.

Some of the answers to your comments contain also further explanations of the above-mentioned modifications.

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Below we report point-by-point responses (in Italic) to your comments; the latter are reported in bold.

The paper by Molinari and co-authors is a very interesting contribution regarding a methodology to rapidly reconstructing tsunami waveforms at a number of observation points starting from a given static tsunami initial condition. As the authors themselves underline, the methodology is not completely new per se, but the novelty resides in the full analysis of the uncertainties related to a number of different factors. The core of the methodology consists in reproducing the expected waveform at a given observation point by linearly combining the waveforms computed numerically (non-linear equations) at the same point for a number of Gaussian-shaped elementary sources. The coefficients for the linear combination are obtained by reconstructing a given static tsunami initial condition by proper superposition of the elementary sources. The performance of the approach is studied by quantifying the misfit between reconstructed and numerically simulated waveforms, the correlation between the maximum tsunami amplitudes, depending on the earthquake magnitude, focal mechanism, focal depth. Furthermore, it has been found that the main error source is related to the reconstruction of the initial condition rather than on the linearity assumption.

This last point is probably the most intriguing. From the theoretical/mathematical point of view, linearly combining results from non-linear simulations should be regarded as a wrong approach. Practically, the conclusion that only a mean 1.7

The mean values of percentage errors resulting from our analysis define the level of the introduced bias when our method is used (overestimation if the mean is positive, underestimation if the mean is negative, no bias if the mean is close to 0.). On the other hand, the corresponding standard deviations quantify the dispersion around the mean values (in other words, the introduced uncertainty).

Keeping this in mind, what we got when we traced back the origin of our âĹij8

We now propose a new method (new section 3.4) aimed at reducing this bias: by comparing Fig. 3i and Fig 4d, we can see that the new method reduces the mean value from âĹij8

Conversely, the value of the standard deviation does not change significantly, meaning that the dispersion of our estimates is still âĹij15

We recognize that we needed to better clarify this point in text, too. We rephrased some sentences in Section 3.3 and in the first half of Section 4, pointing out what stated here above.

Looking at the problem from another side, one may ask why the database of pre-computed scenarios was populated by running simulations solving the non-linear equations instead of the linear ones. This would have probably resulted in a shorter computational time, at the same time fully justifying from the mathematical point of view the linear combination of the elementary solutions. I ask the authors to write a paragraph or two, maybe in the discussion section, where these aspects are commented and the adopted choices more deeply justified.

We added the following paragraph (page 8, lines 26-32, in the revised manuscript) in the discussion:

"We point out that we have populated the ES database using non-linear shallow water equation (NLSWE) simulations since performing non-linear simulation with Tsunami-HySEA code increases the computational time by only <10

Still in the discussion section, I would like to see a paragraph with a detailed example of the consequences of the obtained results on tsunami hazard analysis and/or tsunami warning.

As already stated in the original manuscript, we did not mean to present specific detailed applications, which we reserve to address in future works, but we only tried to provide conceptual examples. According to your comment, we then developed the examples that were already present in the original manuscript in greater detail, concerning their logical steps. Basically the last page of the manuscript in the discussion session is now devoted to illustrate and refer to examples for both hazard and warning applications.

I am attaching an annotated version of the manuscript where I included some further remarks that I ask the authors to take properly into account.

All the annotated comments have been addressed in the new version of the paper.

Here below our detailed replies to the comments in the attached document of the reviewer.

P3 L9: May you briefly comment on the reason of these particular choices?

We commented this in the text (page 3, lines 12-15, in the revised manuscript): "The choice of these parameters is based on a trial and error procedure, during which different Gaussians sizes where tested. The chosen σ ensure to reach the spatial resolution needed to represent the deformation field caused by earthquakes of M=6.0 or greater, while h is a constant used to increase the size of the unit source and to allow a more stable wave propagation. All the resulting waveform amplitudes are then divided by h."

P3 L22: Is the choice of this value suggested by the need to limit the final stor-

age dimension or also by physical reasons? Can you briefly comment on this choice?

We addressed this point in the text (page 4, lines 1-2, in the revised manuscript): "The waveforms are sampled each 30 seconds, value that allows to limit the final store size to 5 Terabytes while still sampling densely enough typical tsunami wavelengths."

P4 L15: Is there a particular reason why the Wells and Coppersmith (1994) formulae were used, and not more recent regressions such as the Leonard's (2010) ones (for instance)?

We used Wells and Coppersmith (1994) with no particular reason, since our test is mainly a synthetic test. We only need a scaling law for fault dimensions, to be able to calculate the Okada initial displacement field to be reconstructed. This choice here does not have any particular influence on the ability of the method to reproduce the initial field (and subsequent waveforms). In principle, we could have set all the exercise using any initial displacement field, but we decided to keep the exercise based on simple but realistic assumptions.

With kind regards,

The Authors

Please also note the supplement to this comment:
http://www.nat-hazards-earth-syst-sci-discuss.net/nhess-2016-145/nhess-2016-145-AC1-supplement.pdf

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