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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "From regional to local SPTHA: efficient computation of probabilistic inundation maps addressing near-field sources" *by* Manuela Volpe et al.

Anonymous Referee #1

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Overall comments

This looks to be a good paper about an important topic that is of clear interest to NHESS readers. It is mostly well written, and describes innovative ideas which are likely to be of broad utility in tsunami hazard assessment.

My only 'significant' concern is that the authors do not provide a 'conceptual justification' for the differences in the results of the two filtering methods they apply, for the case of high H_{max}. As it stands, as a reader I don't know why this happens. Intuitively the reasons are not obvious, and as it relies on some rather delicate calculations (which we know are sensitive to choices of coefficients in filters, etc). At the moment I

cannot be confident that the results are 'stable' enough to justify the conclusion that "it is important to distinguish near and far-field sources in the filtering approach".

If the authors can provide some 'conceptual backing' to support these results, then in my judgement the paper should clearly be accepted for publication in NHESS.

In saying this, please note that I accept the fact that some aspects of these filtering approaches cannot be completely stable (e.g. in the authors example, results with H_{max} < 1m are not meaningful). This is expected, and not a problem. However, they need to provide more justification that the results at higher return periods are stable enough to justify the key conclusion.

Specific comments

- P7, around line 10: I think you mean that you neglect a bunch of 'other' important sources of uncertainty, but, you do comprehensively test the filtering procedure (??right?? – actually upon reading the full paper I'm still uncertain). At the moment the paragraph doesn't make it clear if your example is actually a 'strong' evaluation of the filtering procedure, given the idealized assumptions on the source. Please make this clearer.

P8, top of page – it would be good to report on some sensitivity analysis of this to give the reader a 'feel' for how severe these approximations are (e.g. you could halve the number of clusters, so you don't have to do more simulations).

P8, bottom of page – 'it is worth noting that results at H_{max} < 1m ...' – OK, but because those results are not meaningful, can you please 'clip' your figure limits so that they do not include H_{max} < 1m. That will help the reader focus on parts of the curve that you do consider meaningful, and ease the interpretation of the figures.

P9, paragraph around lines 10-15 – It's not evident to me why method 3a should 'over-estimate' rates for H_{max}>3 (or indeed why the difference is reversed at lower H_{max}). Can you give a heuristic explanation of why this could happen? Without some idea of this, my thinking is 'maybe a calculation/convergence type error' (!). Or is it that, for large enough H_{max}, the associated local sources have a greater tendency to be filtered than the distant ones, for some reason – and the converse for smaller H_{max}? Definitely not obvious to me – please discuss it.

P9, paragraph around line 5 – I agree that you've shown that a 'blind' cluster analysis might produce quite different results from the 2-stage approach proposed in the paper. However, I'm less confident about the stability of either procedure. Can you really say that the 2-stage approach is better, based on the results presented here? Consider the following "devil's advocate" theory – from what you've presented, I hypothesis that "Both of your approaches are strongly affected by the details of the filtering coefficients, and equally big differences could be expected from merely adjusting those in reasonable ranges". In other words, how can readers be confident that the results are not just 'noise'? Probably you can justify this, but I don't see it from the current text. So please add in some discussion that explains 'why' these results happen, and why you expect them to be 'basically robust' {notwithstanding that you have to make some severe approximations for low events – that's ok – but at least for high events, we need some conceptual explanation of the results}.

P11, line 6 – as mentioned above, please provide more 'conceptual explanation' as to why this happens.

Detailed comments

- P3, L31 – suggest changing 'is released' to 'is not used'.

- P5, L5: – suggest changing 'will produce as well similar inundation patterns' to 'will also produce similar inundation patterns'.

- P5, lines 6-7 – Please provide the equation for the cost function. I looked up the 2010 paper, but it appears to refer to time-series comparisons rather than H-max comparisons. Better to make it very obvious to the reader.

- P6, around lines 10-11 – It's not clear to me how you use the co-seismic deformation as a metric for source-proximity in the cluster analysis. Ahh, I see you do this below around lines 25. Give that, please add "(details below)" at the end of the sentence that finishes on line 11.

P7, line 20 – there is a number with multiple '.' inside – this is not familiar notation to me, do you intend to use some other separator?

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