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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "A Methodology For Optimal Designing Of Monitoring Sensor Networks For Tsunami Inversion" *by* Joaquín Meza et al.

Anonymous Referee #1

Received and published: 19 November 2018

-A description of the inversion algorithm used to compute the initial ocean surface deformation from sensor-obtained time series should be provided. A great deal of the error and uncertainties the authors are trying to quantify have as much to do with limitations in the inversion scheme as with location of the sensors. Some of the observed errors may have little to do with sensor location, but a lot with other inversion parameters.

-Since the proposed sensor location is in the seismic rupture area (pre-seismic sensor depth will be different from post-seismic resulting in a BPR time series drift) an explanation of how the inversion process handles the drift in the sensor signal due to co-seismic displacement of the sea-floor should be provided.

-The authors should explain whether sensor signal noise (seismic (Raleigh waves),

tidal cycle, atmospheric pressure variations, electronics noise,...) has been included in the modeled sensor time series and justify why it was included or left out, and what the effects of inclusion or exclusion may be on inversion results.

-While the combination of three metrics into a single Accuracy Parameter is convenient for evaluation, it does not always result in the most accurate evaluation. For instance two identical time series with only a small phase shift originating from a small discrepancy in arrival time will result in very large error estimate in the "skill" (waveform) metric (as defined in Eq'n (5)), although both waveforms are basically identical, such wave forms should only show error in arrival time since H and 'skill' accuracy are 100%, but according to the metrics used, it will also sure large errors in the "skill". The authors should provide some comments on that or at least acknowledge the limitations of the metric used.

-The authors should explain the rationale for capping the error in all three metrics at 100%.

-A brief description of the JAGURS tsunami modeling code(mentioning physical model, discretization approach, validation tests) should be included in addition to the reference, for the benefit of the unfamiliar reader.

-pp8, l25: The authors state " 30 arc sec GEBCO global bathymetry is used since that resolution is sufficient for subfault size larger than 40x40 km", but in pp5, l10 they state the size of their finite faults is 700x700 arc sec ($\sim$20x20km), this seems to imply they will need higher resolution than 30 arc sec for the bathymetry. Perhaps, in their statement they mean "smaller than 40x40km", not larger?

pp5, l12-15: The authors state that due to their current locations outside the study area, DARTs will not be used as sensors. An explanation of why DARTs cannot be relocated if it is determined they would be more useful at different locations, would be appreciated.

-pp13,ll9: One of the conclusions states that the highest accuracy results are achieved when, at least, one sensor is located along the main energy beam. This is an obvious a priori conclusion, however it also points at the possibility that the identified locations may not be optimal for events with slip distributions disimilar to the test cases, particularly larger events with longer rupture lengths that may exhibit an energy beam not necessarily focused on the sensor. They authors should comment on that.

-The manuscript is for the most part well-written, however, some stylistic improvements and typos should still be reviewed and corrected. For instance: "...ar considered...", "...against an historical...", "...Hence, it highly desireable,...", "...range os sources...", "capable of constrain the source...", "it benefits for the fixed...", '...at leas in..."

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