Nat. Hazards Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/nhess-2018-269-AC2, 2019

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.

Joaquín Meza et al.

joaquin.meza@usm.cl

Received and published: 26 January 2019

The article describes the development of a strategy to determine in a rational and reproducible way optimal positions for deep ocean tide gauges, such that tsunami early warning operations can be supported. The approach assumes a small number of deployable sensors and proposes a method that relies on an aggregate error measure, combined from different relevant quantities (arrival times, maximum wave height, and wave shape), to assess the forecast error. The result of this process is a list of optimal sensor locations.

While the report is generally well written and very relevant, and while the method proposed has some innovative potential, the manuscript still needs improvements before it

can be published as a full paper. I have compiled a number of remarks of more general character as well as specialized comments to specific parts of the text below. Unfortunately, for practical applications the data quality of deep ocean wave gauges based on ocean bottom pressure sensors (OBPS) located close to the rupture area is almost useless. The large amplitude seismic waves (in particular Rayleigh waves) cannot be separated from the sea surface elevation signal close to the source. So, a clear wave form cannot be extracted from these sensors (the ones shown in figure 7 are highly unrealistic for sources so close to the wave gauge location). This caveat of the proposed method is not discussed nor considered. This principle drawback was the major reason, why the buoys were abandoned from the German-Indonesian Tsunami Early Warning System (besides considerations regarding maintenance).

R:> Thank you for the comments. Indeed, there are some operational aspects that further constrain locations. We did not intend to address these in the present manuscript, as they can be sensor-specific. Instead, we note that if such constraints are well understood, the possible location of the sensors can be restricted, but the methodology and base of comparison can be used as presented. We have now explicitly mentioned this. See Page 4, L27-33, P9; L21-24.

Page 4, Line 27-33: "The method builds on the premise that, in order to determine the tsunami source, an inversion procedure must be implemented. While an inversion algorithm can be subject to a wide range of errors and uncertainties on its own, for the purpose of the present work it is hypothesized that using a single inversion procedure will weigh equally those errors allowing comparison 30 among different sensor configurations. Consequently, it is the procedure presented herein can be applied independently of the inversion method, and the nature of the sensors. Similarly, other sources of signal noise such as atmospheric variations, tides, etc. It is noted also that in actual operational conditions, incoming data from sensors could need to be pre-processed as to improve the signal to noise ratio.

Page 9, Line 21-24: Although in operational conditions, the time series recorded at an actual sensor will also include other signals that could be considered as tsunami noise, these are not considered here under the premise that the approach used herein is to discriminate among sensor configurations, assuming the treatment of tsunami noise is equivalent across sensors. Similarly, it is noted that possible operational restrictions regarding sensor placement are obviated although they could be implemented straightforwardly."

General Remarks

1. While the data quality obtainable by coastal gauges is discussed (and discarded) the quality of deep ocean sensors (in particular OBPS) is highly dependent on the proximity to the source. If short lead times are an issue then these sensors in practice do not provide accurate information, unless appropriately filtered (which is impossible for many such situations). However, in the methodology chapter, it is assumed that the free surface information can be retrieved with sufficient accuracy, independent of the sensor location. This is an assumption which cannot be fulfilled in practice. Please discuss this issue and consider it in the optimization approach.

R:> Effectively, sensor placement can be subject to constraints which have not been considered here, such as the ones mentioned. However, imposing such constraints should be straightforward and do not affect the overall methodology as presented. On the other hand, spatial dependencies are weighed equally because, at this stage, it is considered that the network would work at a wide range of possible sources. Inclusion of these effects could be subject of subsequent work.

For completeness, we have included a comment on this regard in the discussion. See for instance P4-30- P5L4, and in the Conclusions.

Page 4, Line 30 to page 5, line 4: "For instance, Tsushima et al. (2012) use a 60 s moving average to process data from ocean bottom pressure sensors. Moreover, in some cases, the quality of the data could be dependent on distance to the source. For the present case, these effects are not considered and it is assumed the inversion

process is free of its influence. In addition, it is expected that the resulting network could be used for a range of source locations, hence spatial dependencies are not considered and weighed equally."

2. Using a Green's function approach can be justified only, if the underlying process is (almost) linear. While far-field tsunami propagation can be approximated very well by a linear approximation, I doubt that is also possible in the near field, where wave interferences may lead to highly non-linear effects.

R:> It is true that nonlinear processes, complex interaction with bathymetry and coastal morphology can lead to nonlinear response that could prevent proper characterization of the source. Moreover, the use of coastal gages in inversion can be subject to inverting for coastal effects and trapping. However, it is common practice to use only the first few minutes of the incoming tsunami to minimize these effects. In the present case, we have considered this by placing sensors in deeper water (typically deeper than 50 m) and by considering only up to 30 min of elapsed time to be used in the inversion. Therefore, regarding the inversion, nonlinear effects might not be an issue. See comment on L29-31, pp 5.

On the other hand, we used the coastal points to assess the accuracy of the forecast once a source was found by the inversion. In this case, the role of nonlinearity and other effects could be more relevant, and it could depend on the characteristics of each inverted source. One way to address this would have needed to run the complete, forward simulation including nonlinearity, but it would have been too costly since a solution would have been needed for each sensor array. However, it is expected that omitting these processes will affect the accuracy but not does limit discriminating among sensor configurations. Moreover, coastal points are located at 200 m. depths to reduce the effect of nonlinearity.

We have now acknowledged this aspect and added it to the manuscript. See L30 and following, pp11.

Page 5, Line 29-31: “Typically, these are located at absolute depths such than nonlinear 30 effects can be neglected thereby linear superposition of tsunami time series can be performed with minimal errors. The coastal observation points correspond to tide gages, which are used to evaluate the predictive performance at the coast”

Page 11, line 31-35: “It is of note that the use of linear superposition at shallow coastal points might be subject to inaccuracies arising from neglecting nonlinear interactions, and other process such as bathymetry–induced effects such as resonance. However, it is expected that omitting these process will affect the accuracy but not does limit discriminating among sensor configurations. Moreover, coastal points are located at 200 m. depths to reduce the effect of nonlinearity.”

3. I do not understand what you mean with the listening area and placing sensors within that same. Maybe a sketch would help?

R:> The listening area is meant to be the space enclosed by the tsunami wavefront at any given time. (Tsushima et al., 2009, op.cit. denotes it “influence area”) It allows determining which sensors would be able to record some of the tsunami signal for a given elapsed time since triggering. It is determined by computing the forward-propagated wavefronts from each tsunami event using ray tracing.

We have attempted to clarify the wording in the update version of the manuscript. See paragraph L18-23, pp8.

Page 8, Line 18-23: “On the other hand, the relative distance of the sensors will depend on the time allowed to record the tsunami (e.g. Bernard et al., 2001). Hence, given an earthquake, it is possible to define the area (henceforth termed listening area Al) over which the tsunami waves have already propagated away from the source(e.g. Williamson and Newman, 2018). A minimum of two sensors must be considered inside this area. This equivalent to defining a data observation time, if the tsunami propagation speed is known. This time, termed T0, represents the time the TWS allows for recording of the tsunami before performing an inversion.”

4. I also do not clearly understand what metric you use (you call it error and it is depicted in figures 2, and 3) to assess the forecast quality. As far as I understand, you compute the (virtual) reality by using a forward simulation from the given source by COMCOT. Then you use the Green’s function approach (computed by JAGURS) to invert from the COMCOT reality to an inverted source, if I am not mistaken. But then, what do you use to compare? Do you use that inverted source run yet another forward simulation and compare? You should probably describe this approach more clearly.

R:> The procedure is as described by the reviewer, with the exception that a linear combination of the unit sources (computed by JAGURS) are used to estimate the “forecast solution” at each tidegage, where they are compared with the COMCOT-modeled full forward model.

We have now updated the description on this regard in the updated manuscript to clarify it further. See L21-35, pp11. We have also updated the caption of Figures 2, 3

Page 11, Line 11-35: To carry out the analysis, each scenario is propagated forward to all forecast points (FPs), using the Nonlinear Shallow Water Equations as implemented in the model COMCOT. The time series at nodes in deep water are considered as target (observed) time series to be used in the inversion process, whereas the use of coastal forecast points is aimed at establishing the quality of the assessment (see below). The use of a different tsunami model in propagating the signal and in preparing the database of Green’s functions allows for differences in the target tsunami time series to those of the database, to reduce possible overfitting in the inversion. No other source of variability, such as noise, is considered. It is also assumed that tsunami-tide interactions do not play a significant role in this area.

For each of the 212 sensor arrays (99 and 113, for each scenario), these observed tsunami time series are used to invert the tsunami source. Once a source is determined, the linear combinations of the Green’s functions consistent with the source weights are used to estimate the forecast time series in the coastal points. It is of note that the use of linear superposition at shallow coastal points might be subject to inaccuracies arising from neglecting nonlinear interactions, and other process such as bathymetry–induced effects such as resonance. However, it is expected that omitting these process will affect the accuracy but not does limit discriminating among sensor configurations. Moreover, coastal points are located at 200 m. depths to reduce the effect of nonlinearity"

5. Since only two major scenarios are used for the sensor positioning and the optimal sensor locations are then close to the two epicenters, it should be evaluated how the proposed optimal positions depend on the choice of scenarios.

R:> It is true that the performance would degrade for actual cases where the slip distribution would be such that no sensors are located near the main energy beam or away from the original two epicenters. Acknowledging this, we evaluated scenarios where the target earthquake was located in between the original target earthquakes, and also tested the performance for an existing earthquake with non uniform slip distribution.

We note the sensitivity to the scenario choice has not been tested. For instance, what would have been the case if a Mw7.5 target scenario would have been used instead. However, the method would allow for a solution when other scenarios are used. One can speculate they will be different to the one obtained here. Determining which one is best, would be debatable. Here, we have attempted to find a configuration that works for a certain type of scenario, mostly controlled by the earthquake magnitude.

We have now acknowledged this in the discussion on this regard in the updated manuscript. See P18, L7-10

Page 18, Line 12-16, "This suggests that the proposed network is capable of identifying smaller events with non-uniform slip reasonably well, in reasonable time. It is of note also that the location of this scenario does not coincide with either of the scenarios used to design the network. However, two of the sensors are located close but not directly into the main energy beam. When considered in 15 unison, these two tests suggest

that the sensor network considered would be appropriate for tsunamis generated by earthquakes of magnitude similar and larger than that of the initial target scenarios"

Specific Comments

1. P1, L14: ar -> are

R:> The suggestion was considered.

Page 1, Line 14 - ar -> are

2. P2, L25: I think it would be fair to include the following reference (sorry for the self promotion, but it is relevant here): J. BEHRENS, A. ANDROSOV, A. Y. BABEYKO, S. HARIG, F. KLASCHKA, L. MENTRUP (2010): A new multi-sensor approach to simulation assisted tsunami early warning, Nat. Hazards Earth Syst. Sci.,10:1085-1100, DOI:10.5194/nhess-10-1085-2010.

R:> The suggestion was considered.

Page 2, Line 25 - estimating the tsunami hazard (e.g. Gusman and Tanioka, 2014; Gusman et al., 2014; Cienfuegos et al., 2018). -> estimating the tsunami hazard (e.g. Behrens et al., 2010; Gusman and Tanioka, 2014; Gusman et al., 2014; Cienfuegos et al., 2018).

3. P2, L25: Hence it highly -> Hence it is highly

R:> The suggestion was considered.

Page 2, Line 25 - Hence it highly -> Hence it is highly

4. P3, L7: In the case of Japan, they have... - consider to change the sentence for a better style

R:> The suggestion was considered.

Page 3, Line 7 - In the case of Japan, they have -> For instance, Japan has a few submarine

5. P5, L7: Greeen's -> Green's

R:> The suggestion was considered.

Page 5, Line 7 - Greeen's -> Green's

6. P5, L29: T1 is indeed used in a TWS, i.e. InaTEWS (see Rakowksy et al. (2013): Operational tsunami modelling with TsunAWI – recent developments and applications, Nat. Hazards Earth Syst. Sci., 13:1629-1642, DOI:10.5194/nhess-13-1629-2013.)

R:> The reference was considered.

Page 5, Line 29 - "Although this definition is not used in operational TWS, it is considered a relevant estimate for the time required to trigger 30 a warning status. However, considering that in some cases, the tsunami time series might not exceed the threshold" -> This arrival time definition is also used by the German-Indonesian Tsunami Early Warning System (GITEWS) (Rakowsky et al 2013)

7. P6, Eq. (6)-(8): Why do you take the minimum with 1 here, this is no normalization! Dividing through the maximum would normalize, maybe... But this is just a "limiting". Additionally, it does not preserve the specific behavior of the corresponding functions (H, Tm, Sk). You argue that a limit of 1 indicates 100% relative error, which does make sense. But you should clarify this: If the values are normalized then they cannot exceed 1.

R> Indeed, it is effectively capped at a certain value. We have changed the word "normalized" by "adimensionalized".

The original intention was to provide a common base of comparison, and to minimize the possibility of one of the metric carrying too much weight in the evaluation. By saturating at 100%, all cases reaching this value are weighted the same thereby allowing discrimination by differences in the other metrics.

We had briefly mentioned this in the original manuscript, but we have now expanded

the description in line 8-9, page 7.

Page 7, Line8-9: "To provide a common comparison basis for all possible sensor configurations, each parameter error is adimensionalized by dividing by the reference provided by the observed dataset."

8. P7, Eq. (9): It is important to stress that the sum of weights needs to be 1 again in order to preserve the normalization! You did this intuitively, but should point this out.

R:> Thank you for pointing out the omission. Indeed, we considered it implicitly but did not declare it. We have pointed this explicitly in P7, L26-27.

Page 7, Line 26-27: "The sum of the weights should add to one, as to preserve the comparison basis."

9. P7, Eq. (10): I am not sure if you want to sum up all values. This will give you a non-normalized norm. Additionally, by summing the values up, this corresponds to a 1-norm. Why didn't you take a 2-norm (squareroot of summed squared values)? Why didn't you devide by the number of forecast points N to get a normalized value again?

R:> It was a typo, as we had considered dividing by the number of forecast points.

10. P7, L21: Why do you use a different notation to Eq. 10 here? (Fij(. . .) vs. F(. . .)ij?)

R:> The suggestion was considered.

Page 7, Line 21 - F(. . .)ij -> Fij(. . .)

11. P10, L15: assumemption -> assumption

R:> The suggestion was considered.

Page 10, Line 15 - assumemption -> assumption

12. P10, L23ff.: That the use of different tsunami models for producing Green's functions and for propagating to forecast points can be considered as replication of natural

variability (aka uncertainty) appears quite ad hoc. You should probably justify this by looking at some quantitative measures.

R:> Rather than preserving natural variability, our intention was for minimize possible overfitting that could occur when the input and target signals are created with the same model. We have reworded the text to clarify this.

13. P18, Table 2: Piscagua -> Pisagua

R:> The suggestion was considered

Page 18 - Table 2 - Piscagua -> Pisagua

14. P19, Figure 9: the line for Patache is not visible

R:> Patache is not visible since Patache and Antofagasta are overlapped. They have a similar error, close to the unit. Therefore, if weights are changed, the behavior is similar for all evaluated combination.

Page 20, Line 10 - On the other hand, Patache and Antofagasta, which are overlapped since they have a similar, but not equal error, close to the saturation limit, sees a large drop in the error associated with the arrival time.

15. P20, L8: at leas -> at least

R:> Thank you for catching these typos. The suggestions were considered. However, we noticed that these were not all instances. Therefore, these and some other modifications in the text aimed at improving style and grammar were introduced. The reviewer is referred to the version with tracked changes for details.

Please also note the supplement to this comment:
https://www.nat-hazards-earth-syst-sci-discuss.net/nhess-2018-269/nhess-2018-269-AC2-supplement.pdf

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2018-269, 2018.