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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.

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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

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Discussion paper

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).

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 fulfiled in practice. Please discuss this issue and consider it in the optimization approach.

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.

3. I do not understand what you mean with the listening area and placing sensors

within that same. Maybe a sketch would help?

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.

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.

Specific Comments

1. P1, L14: 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.

3. P2, L25: 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

5. P5, L7: Greeen's → Green's

6. P5, L29: T_1 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.)

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, T_m, S_k). 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.

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.

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?

10. P7, L21: Why do you use a different notation to Eq. 10 here? ($F_{ij}(\cdots)$ vs. $F(\cdots)_{ij}$?)

11. P10, L15: assumemption $\rightarrow$ 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.

13. P18, Table 2: Piscagua → Pisagua
14. P19, Figure 9: the line for Patache is not visible
15. P20, L8: at leas → at least

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