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Geoscientific Instrumentation Methods and Data Systems Discussions

Interactive comment on "Method for testing the calibration of acceleration and pressure gauges installed at the ocean bottom" *by* Mikhail Nosov et al.

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We express our sincere gratitude to Anonymous Referee #1 for attentive reading of the manuscript, understanding of manuscript's essence and valuable comments. Our responses to all the comments are listed below.

Anonymous Referee #1

[General comments] This article proposes a way for testing accelerometers and pressure sensors of seafloor cabled observatories by taking ratio between power spectra of

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acceleration and pressure records. The idea is very simple but its background theory is well established. It looks that the proposed method can identify errors in records either of accelerometers or pressure gauges effectively. However, this idea was already pointed out by early work of the authors group (Nosov et al., EPS, 2018). I could not find any significant advancement since the previous paper, in the present manuscript. The dataset (accelerograms and pressure records obtained by DONET in Japan) for testing the proposed method is completely identical to the previous work, in which the relation between the seafloor acceleration and pressure is proved by analyzing the field observations. The results presented here is not new at all, even the previous paper did not show the power spectra ratios explicitly. Therefore, I have very negative feeling about the novelty of the presently submitted manuscript and would not recommend the editor to accept it for publication.

The Referee is only partially right here. In our paper (Nosov et al., EPS, 2018; page 6, right column, the last paragraph) it is indeed mentioned that the revealed relationship between pressure and acceleration can be used for testing the calibration precision of sensors in situ. However the methods is not introduced in the EPS paper. Obviously, it is next to impossible to squeeze the description of the method into one paragraph! The method for testing accelerometers and pressure sensors - including description of all the necessary stages - is developed in present our manuscript. So, we are strongly disagree with Referee's conclusion regarding the absence of novelty.

I would suggest to the authors that they develop discussion on applicability of the method in practice. As the authors concluded, the method works only when the system records seafloor motions strong enough. Quantitative elaboration of this aspect may increase the value of this work. How large ground shaking would be necessary for the test with this method? How often can we expect such strong shaking is observed by a certain seafloor cable system? The limitation does not come only from the amplitudes but from the durations of the signals, as the authors also pointed out. What

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would be the minimum length of records to assess if the sensors work properly? Since the duration is related to the sizes (magnitudes) of earthquakes, this estimation is also important to know how often we can make the test using the method proposed here. In other words, it is very hard to see if the proposed method is practically useful to diagnose seafloor sensors remotely without the discussion of this kind.

We thank Referee for these fruitful ideas. The related discussion will be developed in the revised manuscript.

The systems deployed along subduction zones may have good chances but I'm not sure if the method will be useful for SMART project, introduced in the manuscript, which may be placed on low-seismicity areas.

There are no problems with the so-called "low-seismicity areas". We have to recall here that seismic events with magnitudes greater than 5 are strong enough to be detected by a seismograph and/or pressure gauge anywhere in the world except earthquake shadow zones (http://www.isc.ac.uk/).

[Specific comments] A textbook written by Saito [2019]* would be cited in the explanation of theoretical background (section 2). The textbook gives comprehensive description regarding the ocean acoustic waves and the relation between bottom pressure and acceleration. * Satio, T., Tsunami generation and propagation, Springer, doi:10.1007/978-4-431-56850-6, 2019.

This idea is reasonable. We shall refer the textbook in the revised version of manuscript.

The meaning of a constant 0.366 appearing in equation (3) should be explained briefly. Although the physical meaning of the equation is explained, it is not clear why the

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coefficient must be 0.366 in this definition.

Brief explanation here involves just a couple of formulas:
1. $1/\cosh(k_0) = 0.01$ – from this equation we get the root k_0;
2. $\sqrt{k_0 \tanh(k_0)}/2\pi = 0.366$ – this formula is from the dispersion relation for gravity waves.

We are sure that such an explanation looks much more nebulous than simply the value of 0.366 together with necessary reference. Unfortunately, the meaning of the constant 0.366 can not be explained briefly. It requires at least the following points: (1) description of the problem of tsunami generation by dynamical displacement of ocean-bottom, (2) analytical solution to this problem – rather cumbersome formulas, (3) interpretation of the solution (preferably illustrated by a figure). We have already provided this explanation repeatedly, for example in the monograph "Physics of Tsunamis" by Levin&Nosov 2016 (P. 211-213). It is senseless to repeat over and over again descriptions that had been already published.

I'm curious about the treatment of tide variations in pressure data. Tide variations are well outside of the frequency band for taking spectral ratios, but it must affect the averaged total pressure (P-bar), if not removed from the records.

We do not think it is necessarily to detide the records. At first sight, the influence of the tide should really negligible $\sim$0.1% because at large ocean depth ($\sim$1000 m) tide amplitude is about 1 m. In the revised version of manuscript we shall provide a more detailed comment regarding this problem.

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