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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.

Anonymous Referee #2

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The manuscript "Method for testing the calibration of acceleration and pressure gauges installed at the ocean bottom" by Nosov et al. presents a method to remotely assess qualities of the pressure gauges and the vertical accelerometers installed at the deep-sea observatories. The manuscript first describes the theoretical basis and the calibration procedure. Then the authors demonstrate its performance from an application to the records of the 2011 Japan Earthquake.

This work deals with an important problem of on-site remote assessments on the deep-sea observatories. I can understand the theoretical basis. The calibration procedure is sufficiently summarized to allow the reproduction. The application is valid. I can read

the manuscript smoothly.

However, some of the explanation on the theoretical basis seem inadequate. I think the authors should perform more extensive analyses and discussions from some important viewpoints, since I could not find the significant expansion of the information already available on the same author's previous paper. Overall, I cannot recommend the current manuscript for publication, but it could be potentially be acceptable after a major revision.

Major comments:

[P.6 LL. 25–26] I suggest the authors discuss more in detail why/how the authors concluded the result had "quite a good precision". If the vertical bars in Figure 3 are the confidence interval (although no explanation is shown), the uncertainties seem too large to immediately conclude as the authors did.

[P. 6 LL. 27–29] I suggest that the authors discuss how to get over the difficulty in the calibration of the deepest observatories located at the trench slope region. Probably the authors can check whether an incorporation of the horizontal acceleration effects improves the calibration or not.

[P.9] I strongly suggest the authors conduct some extensive analyses from some important viewpoints. The authors can perform the application of the method to other major earthquakes to discuss temporal changes of the sensor performance. It may also be valuable to confirm the lowest magnitudes to which the calibration can be applied. This is important to discuss the feasibility of the real-time monitoring since the major events much less frequently occur. In addition, the authors can discuss advantages and disadvantages of the proposed method compared to other approaches, such as the comparison between the different types of seismometers installed at the same site.

Minor comments:

[P.2 LL.11–17] There must be many papers dealing with the calibration methods for onshore seismometers whereas there are much less researches for offshore deep-sea instruments. I suggest the authors refer to the previously proposed approaches in the introduction.

[P.3 L.8] I suggest the authors explain how “0.366” was derived.

[P.4 L.7] The authors can remove “submarine”. Large inland earthquakes must generate significant seismic waves in the seafloor observatory.

[P.4 LL. 16–20, 22–23] These sentences confused me. If the slope is insignificant or nearly horizontal, the horizontal effects will be small to be ignored. But it seems that what the authors are saying is different. Please explain carefully.

[P. 4 LL. 23–25] Please explain how “rapid decreases exponentially” and “1-2 ocean depths” are derived and/or cite the appropriate reference.

[P.8 L. 33 – P.9 L. 3] This paragraph is unkind to the readers. To completely understand what the authors are saying, I had to refer back to Nosov et al. (2018). I suggest the authors briefly explain the results of the analysis performed in Nosov et al. (2018) and describe how the authors conclude the vertical accelerometer is in worse condition.

[P.9 LL. 1–2] It is likely that Nosov et al. (2018) compares the E18 spectrum only to E17. Is it OK to use plural form here?

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