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Interactive comment on "A Multi-Technology Analysis of the 2017 North Korean Nuclear Test" *by* Peter Gaebler et al.

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The paper summarizes different analysis and observations concerning the September 2017 nuclear event in North Korea. It illuminates the event from the perspectives given by the different geophysical methods, that are used for monitoring of nuclear explosions.

First, the authors give an introduction on the need of monitoring nuclear explosions. The largest part of the paper is dedicated to seismological observations and modeling, which comprise 5 subsections. One section is about infrasound observations and modeling, one section is on the surface deformation and the last section on propagation of radionuclides. Except for the section on surface deformations, all sections comprise

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both, theoretical modeling as well as analysis based on observations.

The described seismological methods provide a refined epicenter location, an depth estimate, moment tensor solution as well as a yield estimate from the retrieved magnitude. The topography effect has been modeled, showing an focusing of the seismic energy to smaller slownesses. Infrasound signals have been modeled and detected, surface displacement retrieved from DinSARmethod and atmospheric modeling performed to explain detections of radionuclides.

The paper gives a nice overview on all different aspects. However, in some details it appears to be not mature but has a lot of potential for improvements. The paper, especially the seismological/modeling analysis, appears to be incoherent in some aspects. It seems to be composed of pieces which are put one next to each other, but that are not taken into account the results from the other pieces. All individual aspects are interesting and definitively worth publishing.

One can resolve most problems without additional analysis, but by discussion and mentioning the discrepancies, which arise due to the different focus of the different methods. For example the depth estimate of the moment tensor inversion is 2 km, while the pP depth-phase depth estimate is 600 m. This is no surprise, since both methods have a different focus, but it should be mentioned and discussed in more detail. As other example, the topography effect is modeled and discussed qualitatively, but the influence on the depth phase estimate is not investigated. By discussing the modeling more quantitatively, one could possibly make arguments on slowness and frequency ranges, for which this focusing effect is relevant. It should be discussed if this has an effect on the other described methods (moment tensor solution, depth estimate, magnitude estimation by regional vs. teleseismic phases.) At other points the paper should show the observations, that the described analysis are based on. For example the input parameter for the double difference method in section 2.1 could be displayed instead of the coherency value, which is not so relevant for the analysis. As other example it would be nice to show the wave-forms, that the magnitude estimations

in section 2.4 is based on, to demonstrate the differences in high- and low-frequency magnitude estimations. In general I would appeal to the authors to keep in mind the reproducibility/traceability of their results. To achieve this, the level of details in the description has to be appropriate (which, of cause, can also be achieved by referring the reader to the relevant references).

In conclusion, I would recommend the paper for publication after MAJOR REVISIONS. Reviewing this paper was quite some work, but also very interesting. All comments/suggestions are meant to be constructive in order to improve the manuscript. In general, I have no doubt in the relevance of this work, which is why it appears to me to be worth the work that has still to be done.

This general comment as well as a list of issues to be considered before publication are included in the attached pdf file.

Please also note the supplement to this comment:
https://www.solid-earth-discuss.net/se-2018-102/se-2018-102-RC1-supplement.pdf

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