Responses to referee 1 (Felix M. Schneider)

Manuscript SE-2018-102

A Multi-Technology Analysis of the 2017 North Korean Nuclear Test

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Dear Felix Schneider,

thank you very much for taking the time and interest to deliver this very thorough and constructive review of our manuscript. We carefully studied your comments and made changes and corrections to the manuscript where necessary. We hope our changes and and corrections are sufficient to make our article suitable for publication soon. Your comments and suggestions certainly helped to improve quality and clarity of the paper.

Respones to your comments are given in the following pages. Changes made in the manuscript related to your comments are given in green color, changes by the authors after re-reading the manuscript are given in grey color. Changes related to the second referee are given in blue color. Page and line numbers refer to the originally submitted manuscript.

Thank you again,
best regards

Peter Gaebler and co-authors

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0 Abstract

0.1 COMMENT: page 1 line 9: Do you mean Seismological investigations of depth phases? Then it must be 0.6 km below surface instead of 0.8 km (otherwise it is in contradiction with the results in 2.2). In this case I would include "of depth phases" as well in the text.

RESPONSE: Thank you for this comment. First of all, you are correct, it must be 0.6 km instead of 0.8 km. We chose not to include the word depth phases here, as the sentence is not only about depth estimation using depth phases but also about epicenter estimation using regional seismic phases. We therefore used the umbrella term *seismological investigations*.

CHANGES IN THE MANUSCRIPT: page 1 line 9: changed 0.8 km to 0.6 km.

0.2 COMMENT: page 1 line 16: ehance → enhance.

RESPONSE: Typo corrected.

CHANGES IN THE MANUSCRIPT: page 1 line 16: ehance → enhance.

1 Introduction

1.1 COMMENT: page 2 line 9: It was opened → CTBT was opened (since in the sentence before you talk about CTBT and CTBTO).

RESPONSE: Changed the sentence as suggested.

CHANGES IN THE MANUSCRIPT: page 2 line 9: It was opened → CTBT was opened.

1.2 COMMENT: page 2 line 10: Place a comma after *At the time of this study.*

RESPONSE: Added comma as suggested.

CHANGES IN THE MANUSCRIPT: page 2 line 10: added comma after *At the time of this study.*

1.3 COMMENT: page 3 line 11-25: Paragraph should be resorted and corrected (shift one sentence and 3 typos).
(a) teleseimsic → teleseismic.
(b) *Radionuclide monitoring demonstrates the importance of atmospheric transport modelling (ATM) to avoid over-interpretation of variations in 133Xe concentrations.* Place this after *... effects at the surface.*
(c) at the test site, → at the test site.
(d) *The event depth is estimated for the first time by a joint inversion of source time function (STF) and depth phase waveform modeling observed at small aperture, high-frequency arrays in teleseimsic distances.* This phrase has to be modified. You do not invert the depth phase waveform modeling. You do joint inversion of the STF and the waveform composition of direct and depth phase to retrieve the depth.
(e) Additionall → Additional.

RESPONSE:
(a) Typo corrected.
(b) Shifted the sentence as suggested.
(c) Typo corrected.
(d) We would like to appreciate this point. We performed a joint inversion of the waveform composition of the source time function (STF) and direct and surface-reflected phases to retrieve the depth. We agree that this should be further clarified in the manuscript. In accordance to the reviewers comment, we modified the phrase in the revised manuscript.
(e) Typo corrected.

CHANGES IN THE MANUSCRIPT:
(a) page 2 line 17: teleseimsic → teleseismic.
(b) shifted the sentence from page 2 line 12 to page 2 line 23.
(c) Replaced comma by point on page 2 line 21.
(d) Rephrased the sentence starting with *The event depth...* on page 2 line 16.
(e) Additionall → Additionally.

2 Seismological Investigations

2.1 Epicenter Location

2.1.1 COMMENT: page 4 line 15: I do not understand the relevance of Figure 2 for the described analysis. It shows the similarity of the events. owever, as input parameter for the double difference method one needs the lag times. It would be good to find a way to show the lag time data, for the different events, maybe in a similar way as the correlation coefficient is shown.

RESPONSE: Figure 2 illustrates the results of the correlation, which are described on page 4 lines 7-14 above the figure. The correlation results are relevant for the relative location procedure for two reasons: (1) Estimation of the time lag, as described on page 4 lines 16-18. (2) Selection of the reliable event pairs, for which a threshold of the correlation value was set. The second topic is not yet explained in the text. A corresponding statement will be included. We feel that showing lag time data for the different events is not adding that much of valuable information.

CHANGES IN THE MANUSCRIPT: page 4 line 16: Made changes to the sentence starting with *Time lags of the....*

2.2 Estimation of Hypocenter Depth and Seismic Moment

2.2.1 COMMENT: page 4 line 29: *The analysis of the time lag of near-source, surface-reflected P-phases, so-called depth phases, can potentially help in such a case, because they only depend on the depth and the P-wave velocity inlayer above the source.*
(a) I found this explanation misleading. In teleseismic distances one cannot resolve a depth phase or a depth phase lag time in the seismograms. And looking at your array beams one cannot identify any separated depth phase, since the source time function is much longer than the lag time. What you are doing is modeling the wavelet under the assumption that is consist of source time function (STF) and an reflected phase, which resembles the inverted and time shifted STF. However, in the paragraph it sounds like you are able to identify the depth phases by applying beamforming.
(b) A very similar approach as been applied in the section ARRAY BEAM MODELING of Cesca et al (2017). Please cite it here. In that publications of the 2016 nuclear explosion, arrays from different azimuths had been used (ASAR, GERES and PDAR). Why those data were not utilized for the inversion shown in this paper?

RESPONSE:
(a) The reviewer raised a very valid point. It is difficult to constrain the depth of a shallow source without a close station within a focal depths distance from the source. For a more precise estimation of the source depth, we performed a wavelet modelling composed of the direct and surface-reflected P phase and STF. We identified the direct and depth phases using the ray-tracing through the assumed velocity model used to compute the Greens Functions and select a time window around the theoretical arrival times for those phase. To support the referees assertion, we modified the text in the revised paper.
(b) We appreciate this comment. We modified the text and added citation to Cesca et al. (2017). The nuclear explosion is very short in time, and STF inversion for such procedure needs high-quality signals with very good signal-to-noise ration. We also tested the robustness of our method in terms of its independence from the azimuthal coverage of the data set used. By considering these items, we utilized the data set reported in the paper, which is different from the one used in Cesca et al. (2017).

CHANGES IN THE MANUSCRIPT:
(a) Page 4 line 26-30: Modified the sentences.
(b) Reference Cesca 2017 added.

2.2.2 COMMENT: page 5 figure 3:
(a) The figure is not displayed right in the SE-manuscript (se-2018-102.pdf). However I found a proper displayed one in se-2018-102-manuscript-version1.pdf. Please make sure, that it is right in the final version.
(b) It would be good to show the residual fit to deeper depths, since it seems like the residual decreases again from 900 to 1000m. Also 2 km is the depth estimate from MTI and this result should be in the

range of the refined method. So best would be to show the residual curve down to at least 4 km.

RESPONSE:
(a) Replaced the figure that is displayed right in the final version of the manuscript.
(b) We followed the referees suggestion and changed the figure accordingly. In the new figure, the residuals curve has been shown for deeper sources as much as our pre-computed Greens Function data base makes it possible. For sources deeper than 1500 m, we have to recalculate the GreensFunctions for all arrays used, which is computationally time consuming. We hope that the referees is convinced that the residuals increase by increasing source depth. The jump seen at depth 1000 m can be because of the velocity discontinuity (e.g. layer interface) at exactly this depth in the velocity model used for waveform modelling.

CHANGES IN THE MANUSCRIPT:
(a) Figure should be displayed correctly now.
(b) Figure adjusted as suggested.

2.2.3 COMMENT: page 5 line 5: What is the best moment tensor solution? How is it determined? Is it the solution derived later in Section 2.3? Please clarify in the text.

RESPONSE: The best moment tensor solution is the one derived in Section 2.3. To clarify this, we modified the text.

CHANGES IN THE MANUSCRIPT: page 5 line 5: Added the text *described in Subsection 2.3*.

2.2.4 COMMENT: page 5 line 6: The abbreviation STF is used only in the introduction. For better readability I would define it here again as source time function (STF).

RESPONSE: We decided to avoid abbreviations in the abstract and then introduce them the first time they are mentioned. For consistency we would rather keep it that way. The term STF is already mentioned in the Introduction section.

2.2.5 COMMENT: page 5 line 12ff: *It is assumed that all moment tensor components M_{jk} have the same time dependency, which is described as normalized STF $m(t)$ with $m(t \to \infty) = 1$. The waveform of far-field displacement pulses are controlled by the time derivative of $m(t)$, which is declared as moment rate function $\dot{m}(t)$. The P-wave from an earthquake $\dot{m}(t)$ has a single-sided pulse.*
(a) In the figure caption of Fig.3 you call m(t) moment function. Here you call it normalized STF. Please be consistent.
(b) Please give a citation for this statemement: *The waveform of far-field displacement pulses are controlled by the time derivative of m(t).*
(c) Please give a citation for this statemement: *The P-wave from an earthquake, m(t) has a single-sided pulse.*

RESPONSE:
(a) Thanks for the insightful comment. To keep the text consistent, we reviewed the paper and called m(t) moment function in the entire manuscript.
(b) The statement has been slightly modified and Dahm and Krüger (2014) was cited to support the statement.
(c) Same citation as above (2.2.5b) was provided for the statement raised in the comment.

CHANGES IN THE MANUSCRIPT:
(a) page 5 line 13: STF → moment function.
(b) page 5 line 15: Citation Dahm and Krüger 2014 added.
(c) page 5 line 15: Citation Dahm and Krüger 2014 added.

2.2.6 COMMENT: page 5 line 16: Green function → Green's function (2 times).

RESPONSE: Thanks for the comment, corrected all Green to Green's.

CHANGES IN THE MANUSCRIPT: page 5 line 16: Green function → Green's function (2 times).

2.2.7 COMMENT: page 5 line 23: The convolution in (3) equated and written in discrete form − > The convolution in (3) can be written in discrete form.

RESPONSE: Changed as suggested.

CHANGES IN THE MANUSCRIPT: page 5 line 23: The convolution in (3) equated and written in discrete form → The convolution in (3) can be written in discrete form.

2.2.8 COMMENT: page 6 line 5,7,9: Green function − > Green's function.

RESPONSE: Corrected, again thanks for the comment.

CHANGES IN THE MANUSCRIPT: page 6 lines 5,7,9: Green function → Green's function.

2.2.9 COMMENT: page 6 line 11f:
(a) Insert CRUST2.0 before Bassin et al., 2000.
(b) *Intrinsic attenuation for P-waves is set to 5000, since otherwise high frequencies are damped out at teleseismic distances.* Is this realistic? Please comment why you are allowed to do that. If not the question arises, what are you inverting for?
(c) The grid search depth phase modeling has been applied previously to different cases of induced seismicity (Dahm et al., 2007), but the simultaneous STF inversion is implemented for the first time. → The grid search depth phase modeling has been applied previously to different cases of induced seismicity (Dahm et al., 2007) and the 2016 nuclear explosion (Cesca et al. 2017), but the simultaneous STF inversion was implemented for the first time in this study.

RESPONSE:
(a) Thanks for the comment. We simply forgot this statement.
(b) We support the referees assertion, but we do not see a problem if the quality factor is unrealistically high in the modelling. This is because the waveforms are really unaffected when propagating up and down in the uppermost mantle. However, if the quality factor is too small, high frequency content is very difficult to observe at teleseismic distances.
(c) Changed as suggested.

CHANGES IN THE MANUSCRIPT:
(a) page 6 line 11: Added CRUST 2.0 in the manuscript before the citation of Bassin et. al, 2000.
(b) No changes in the manuscript.
(c) page 6 line 12-14: Changed sentence as suggested.

2.2.10 COMMENT: page 6 line 20ff:
(a) Such an overshoot is not expected for the rupture process of tectonic earthquakes, but commonly observed for nuclear explosions. → Such an overshoot is not expected for the rupture process of tectonic earthquakes, where the moment rate and moment functions of the P-wave are single sided pulses and monotonously increasing functions, respectively. For nuclear explosions, however, this feature is commonly observed.
(b) It can be explained → This can be explained.
(c) high-frequency mb estimates in Subsection 2.4 → high-frequency mb estimates which emerged during the magnitude estimation described in Subsection 2.4.

RESPONSE:
(a) Changed as suggested.
(b) Changed as suggested.

(c) Changed as suggested.

CHANGES IN THE MANUSCRIPT:
(a) page 6 line 21: Sentence changed as suggested.
(b) page 6 line 22: It can be explained → This can be explained.
(c) page 6 line 26: high-frequency mb estimates in Subsection 2.4 → high-frequency mb estimates which emerged during the magnitude estimation described in Subsection 2.4.

2.3 Moment Tensor Inversion of the Test and the Main Aftershock

2.3.1 COMMENT: page 7 line 11: *The best solution, found at a depth of around 2 km, has a scalar moment of 2.33x1017 Nm,equivalent to a MW of 5.55.* This is inconsistent with section 2.2, where the best depth is at 600 m. Comment on that.

RESPONSE: The estimation of the centroid depth by moment tensor inversion (2 km) has a lower resolution than the hypocentral depth discussed in Section 2.2 (0.6 km). The centroid depth result may also depend on the velocity model used, as shown in Cesca et al. (2017) for the study region. In this work we use one preferred velocity model and found good waveform fits for centroid depths varying between 0 km and 2.5 km. Considering the lower resolution of centroid depth, this corresponds well with the hypocentral depth estimated be analysis of depth phases of 400-800 m (section 2.2). We improve the text to explain the differences among hypocentral and centroid depth estimates.

CHANGES IN THE MANUSCRIPT: page 7 line 11: Added the sentence *The centroid depth is poorly resolved and good waveform fits are found for very shallow sources down to 2.5 km. This result confirms the very shallow depth of 400-800 m accurately resolved by the analysis of depth phases in Subsection 2.2.*

2.3.2 COMMENT: page 7 figure 5: How the shown wave forms are selected? Do you only show those stations where the fit is good? Why do you show less wave forms in Fig. 7 compared to Fig. 5? It would be good to show the same example stations in both cases or explain why you select the given stations. Please highlight the selected stations on a station map (e.g. in Fig. 1).

RESPONSE: For the explosion signal we use all stations at components within the chosen range of epicentral distances, only excluding a few traces, based on lower signal-to-noise ratio. Signal amplitudes for the aftershock are significantly smaller than those of the main event. Since the signal-to-noise ratio of the aftershock is much lower that that of the main event, we decided to use only the best waveforms for each event individually.

CHANGES IN THE MANUSCRIPT: Figure 1 has been updated as suggested, also captions of figure 1 and 5 are updated in the manuscript.

2.3.3 COMMENT: page 7 figure 6: (a) It would be good arrange the Figure in two parts a) and b), a) showing the decomposition of the Moment tensor and b) showing the source type diagram. (same in Fig.8).
(b) I would find it very useful to explain roughly the Hudson-diagram, since it might be common sense for experts on moment tensor decomposition, however the paper is for a broader audience. For example all moment tensor solutions (beach balls) in the Hudson diagram represent double-couple solutions, while the best fit solution is mainly isotropic. Could you please comment on the representation and how one should read it?
(c) How was the ensemble of moment tensor solution retrieved? Do you use a statistical Monte-Carlo-type inversion? Give some more details and/or references in order to present reproducible results.

RESPONSE:
(a) The layout of Figure 6 and 8 actually fulfills the reviewer suggestion: the top part illustrates the

MT decomposition and the lower part the source type diagram. We improved figures caption to clarify this point.
(b) In the figure, the position of each beachball in the source type diagram describes the decomposition into isotropic, CLVD and DC. To provide more information we included beachballs, which describe the geometry of the DC term; however, in this case, DC terms are small for most MT solutions (they are far from the center of the diagram, which corresponds to pure DC solution). We improved the text and figure captions to clarify the figure and discussion for a broad audience.
(c) The optimization algorithm is described in Heimann et al. (2018), where the code is available (the reference has been now included); a dedicated manuscript on the methodology is stillin preparation. The method follows a sort of simulated annealing approach, first exploring the parameter space randomly, and later searching closer to best fitting solutions. Simultaneously a bootstrap approach is applied in order to investigate MT variability upon thedifferent weighting of different data. Figures 6 and 8 plot the ensemble of best solutions. We provide now more details in the manuscript with respect to the adopted method and procedure.

CHANGES IN THE MANUSCRIPT:
(a) Modified figure captions of figure 6 and added text on page 7 line 13.
(b) Text added on page 7 line 13.
(c) Text added on page 7 line 5.

2.3.4 COMMENT: page 7 figure 7: see comment 2.3.2.

RESPONSE: Please see reply to comment 2.3.2.

2.3.5 COMMENT: page 7 figure 8: see comment 2.3.3.

RESPONSE: Please see reply to comment 2.3.3.

2.4 Magnitude and Yield Estimation

2.4.1 COMMENT: You do not show any seismic section of the event. This is a pity. Here would be a good position to include one (maybe compare it to with waveforms of a previous test) and show the peak values that you are using to estimate mb. Show the 15 IMS stations you are using here on a map or give at least a reference, where one can find them.

RESPONSE: You are absolutely right, we should include some kind of seismic representation of the nuclear test (besides the traces shown in the MTI section). We will include example seismic recordings of the IMS primary seismic station GERES/PS19. For traceability, the 15 used stations for the magnitude estimation will be explicitly named in the text. An additional map could overload the article and add to much weight to this issue.

CHANGES IN THE MANUSCRIPT: page 7 subsection 2.4: We added the names of the seismic station used for the body wave magnitude estimation and added a figure showing seismic traces of the six DPRK tests recorded at the station GERES/PS19 in Germany. Furthermore we added more details to the text regarding the magnitude estimation.

2.4.2 COMMENT: page 8: *As no particular magnitude-yield relation has been approved so far for the North Korean test area, the latter relation by Bowers et al. (2001) is used in this study, as it supposedly most accurately represents the geological conditions at the test site.* Can you please comment on why the geological setting is supposed to be similar to Nova Zemlya? More important, is the North Korean test area known to consist of wet hard rock, rather than of dry unconsolidated rock? It changes the interpretation dramatically (by one order of magnitude). Give some references.

RESPONSE: Actually, we cannot provide proofed hints to associate the seismic coupling in Punggye-ri rather to the conditions in Novaya Zemlya than to the conditions in Semipalatinsk. However, wet hard rock as general site condition can be stated. See references by Pabian and Coblentz.

CHANGES IN THE MANUSCRIPT: page 8 line 10: Changed/added text in the manuscript on page 8 line 10.

2.5 Influence of the Mt. Mantap Topography

2.5.1 COMMENT: The absolute transmitted energy is in all cases the same. A focusing/defocusing effect to certain slowness ranges is modeled. The conclusion is, that a focusing of the energy to small slownesses might result in an overestimation of the magnitude.

(a) For the discussion of this point I would ask the authors to be more quantitative. Give numbers: For which slowness range the energy is increased/decreased. Is it relevant only for teleseismic records or also for regional ones. Could a focusing to small slownesses also lead to an underestimation of the magnitude when using local/regional stations?

(b) Please discuss on the frequency ranges that are affected by the topography: For which frequency ranges the focusing effect is relevant? The lens (concave mirror) of the mountain has a range of approx. 2 km. For a P-wave velocity of 5.7 km/s wavelengths of the same dimension have a frequency of 2.85 Hz. Thus, I would expect, that the focusing is effective only for high frequencies (¿ 2.5 Hz). Will this still affect the magnitude estimation from teleseismic records?

(c) You show the topography effect in this chapter, however, you do not consider the topography in any of your analysis in the other chapters, especially for modeling the depth phases. Would the topography effect also effect the depth estimation by using pP phases? Please comment on that, since otherwise the paper appears to be incoherent.

RESPONSE:

(a) The focussing effect has only been investigated for a teleseismic slowness range. In figure 10 seismic energy is recorded in a rectangular box in a depth of 4 km beneath surface. The lateral extents of the box covers a slowness range for teleseismic P-phases from -9 to +9 s/degree. In our study local and regional phases have not been considered because yield is estimated using only teleseismic P-phases. Local and regional magnitude estimations are normally based on Lg-phases, these phases are not modeled in this study.

(b) In our simulations we model a broad frequency range using a point source with a delta shaped source time function. Under your assumptions for a P-wave velocity (5.7 km/s) and a lateral extent for the mountain (2 km), only waves with a frequecy above 2.85 Hz would be able to resolve the structure of the mountain. This holds true for tomographical studies for example. In the case of reflections at the surface, also waves with frequencies lower than 2.85 Hz will be affected. Therefore the focusing is effective also for lower frequencies, which are typically used in the estimation of teleseismic body wave magnitudes.

(c) We see no inconsistency between those two approaches, since the topography causes no change in differential travel times (see figure 10, bottom) but only influences the amplitudes of pP-phases. These amplitudes are not considered in the depth phase modeling approach. The influence of topography on moment tensor inversion is not studied in this paper. However we feel that topography might lead to a slight decrease of the volumetric part of the moment tensor. But this has to be investigated further in dedicated study dealing with a full wave propagation simulation on a regional scale.

CHANGES IN THE MANUSCRIPT:

(a) Added a sentence on page 8 line 31 to clarify the investigated slowness range.

(b) No changes in the manuscript.

(c) Added a sentence on page 9 line 12.

2.5.2 COMMENT: page 9 lines 6ff: *In general, this numerical modeling gives indications for: (1) Clear infrasonic signals, because surface reflections with higher amplitudes correspond to transmitted amplitudes with higher amplitudes yielding a transmission coefficient greater than 2 (see Section 3).*

(a) *Higher amplitudes* Higher than what?

(b) Where in section 3 are you taking about transmission coefficients?

(c) For me, it is not clear how you relate the pure elastic modeling to the elasto-acoustic transmission coefficient.

RESPONSE:

(a) Thank you for this comment, this sentence is a bit unclear. We rephrased the sentence for better understanding.

(b) You are correct, we are not talking about the transmission coefficient in the Infrasound section. We do not mention transmission coefficient in the rephrased sentence. See also comment 2.5.2a.

(c) The relation between elastic and acoustic energy has not been modeled in this study. Nevertheless it is well documented phenomena that infrasound is generated by earthquakes with a similar magenitude as the 2017 test. See for example Mutschlecner, 2005.

CHANGES IN THE MANUSCRIPT:

(a) Modified the sentence on page 9 line 6.

(b) See 2.5.2a.

(c) No changes in the manuscript.

3 Infrasound

3.1 COMMENT: page 9 line 23: *Figure 11a highlights the waveform beam of the Russian 4-element infrasound array I45RU (denoted as the co-located seismic station USRK in Figure 1a).*

(a) Please give here or in the next sentence the distance of I45RU to the test-site.

(b) It is surprising, that the It phase shows the strongest amplitudes. Is this in agreement with the modeling?

RESPONSE:

(a) We added the distance of around 400 km in the text.

(b) Yes, this is in good agreement with the corresponding modeling, we refer you to comment 3.3.

CHANGES IN THE MANUSCRIPT:

(a) page 9 line 21: added the distance of 401 km.

(b) see response to comment 3.3.

3.2 COMMENT: page 9 figure 11: The color code of the modeled ray travel-time is not chosen well to show differences in travel time of It and Is2.

RESPONSE: That is a valid comment, thank you. It is hard to visualize the difference as the propagation times for It and Is2 are very similar. As a compromise I added the exact traveltimes to the figure caption to highlight the similar traveltimes of the two phases.

CHANGES IN THE MANUSCRIPT: figure 11: Slight changes in the figure caption.

3.3 COMMENT: page 10 lines 11-16: *The signal attenuation indicates that only a small portion of signal energy is ducted in the stratosphere caused by partial reflections from gravity wave variations of the stratospheric mean background. This leads to higher attenuation in the stratospheric duct and thus stronger signal amplitudes in the thermospheric duct, which corresponds to the observed waveforms.* Please explain this point in a bit more detail. This is interesting. Why is the amplitude of It even larger than Is2?

RESPONSE: Parabolic equation modeling was performed (and is shown as the background color of figure 11c) to quantify the attenuation of the signal. The attenuation is 4.9 dB lower for the thermospheric duct compared to the stratospheric duct, resembling a factor of pressure amplitude increase of 1.8. Attenuations are derived and averaged from the PE modeling at the station distance of 401 km for a 2 km wide central section of the respective duct; total values of attenuation are -46.7 dB for the stratospheric duct and -41.8 dB for the thermospheric duct, both estimated with a signal frequency of 1 Hz. For 2 Hz, the values are -48.5 dB and -44.4 dB, the difference is 4.1 dB, equaling a factor of 1.6, accordingly. It is slightly less, but in the same order of magnitude, since thermospheric signals are more strongly attenuated at higher frequencies.

CHANGES IN THE MANUSCRIPT: page 10 lines 11-16: The resulting dB numbers and amplitude factors are added at page 10 lines 11-16. Furthermore the term *parabolic-equation modeling* is mentioned in the text.

3.4 COMMENT: page 10 lines 18-20: *Apart from the strong epicentral surface movement, infrasonic signatures were also identified from seismo-acoustic coupling and the assumed cavity collapse associated to the eight minute subsequent aftershock.* This is interesting. In 400km distance? Can you show these data? (maybe put it in the supplement).

RESPONSE: We added rather detailed supplementary information to this manuscript highlighting the early and late infrasound arrivals detected at I45RU and explaining their origins, as proposed.

CHANGES IN THE MANUSCRIPT: page 10 line 20: Added text to refer to the supplementary material.

4 Remote Sensing Studies

4.1 COMMENT: page 11 line 13-14: Figure 12 shows the surface deformation restricted to the test site area after the 2017 test for pixels with a coherence of greater than 0.25. → Figure 12 shows the surface deformation at the test site area during the 2017 test restricted to pixels with coherence values greater than 0.25.

RESPONSE: Changed as suggested.

CHANGES IN THE MANUSCRIPT: page 11 line 13-14: Changed the sentence as suggested by the reviewer.

4.2 COMMENT: figure 12:
(a) Displacement scale should have more numbers. (You only show ¿10 and ¡10). Please add at least a 0 and +/-5. (b) Please add the legend, explaining the colors of numbered circles, which is present in Fig 1b), to avoid misinterpretation of these colors with displacement values.

RESPONSE:
(a) Added more numbers to the legend in figure 12 (0cm, -5cm and 5cm).
(b) You are right, these colours are misleading. I removed the colors of the numbered circles to avoid misinterpretation with the displacement values.

CHANGES IN THE MANUSCRIPT: Slight changes to figure 12.

4.3 COMMENT: page 11 lines 18-20: *Due to the incidence (43°) and lock angle (ENE) of the sensors and the calculated slope and aspect angle (20° to 27° facing SW, 10° facing NE) based on Shuttle Radar Topography Mission data, 30 to 80 % of the vertical measured displacements in the area are detected.* 30-80% is a very wide range. I do not understand the statement. Please clarify.

RESPONSE: This sentence is hard to understand, thank you for the comment. We rephrased the sentence in the manuscript. Hope it is easier to understand now.

CHANGES IN THE MANUSCRIPT: Replaced the sentence
Due to the incidence (43°) and lock angle (ENE) of the sensors and the calculated slope and aspect angle (20 to 27° facing SW, 10° facing NE) based on Shuttle Radar Topography Mission data, 30 to 80 % of the vertical measured displacements in the area are detected. The recognition of the displacements is only possible in line of sight in the direction towards or away from the sensor
with the sentence
Due to the incidence and look angle of the sensor measured with respect to the ground and north direction, respectively, it is not possible to fully resolve the amount of the vertical surface displacement caused by the 2017 nuclear test. During data acquisition on August 29th and September 12th 2017 the sensor had an incidence angle of 43° and look angle facing East North East. These angles combined with the topography of Mt. Mantap result in the maximum mensurable percentage of vertical surface displacement. For the western flank of Mt. Mantap pointing towards the sensor with a slope of around 27° the percentage of resolvable surface discplacement is only around 30%, for the eastern flank pointing away from the sensor with a slope of around 10° the value of maximum menaurable surface displacement increases to around 80 %.

4.4 COMMENT: page 11 lines 20-27:
(a) I could not find the tunnel entrance on Fig 12 of Fig 1b. Please mark it in Figure 12.
(b) Please comment, why your processing of TerraSAR-X did not work here, while Wang et al was able to calculate the displacements.
(c) addtional → additional
(d) calcultes → calculates

RESPONSE:
(a) You are right the tunnel entrance is missing in figure 12. To avoid overloading of the figure I changed the passage in text from *tunnel entrance* to *main support area*. The main support area is shown in the figure 12 as well as in figure 1b.
(b) Wang used a different method to analyse the TerraSAR-X data set. The author used the method of amplitude tracking (also called pixel-offset-tracking or feature-tracking). This is a different method and is mainly used to investigate data sets with bad coherence values (for example glacier movements). Coherence values can be low for very strong ground movements (as for example in the case of the nuclear test). For the method used in this study coherence loss in the center of the test area was to high, and it was therefore not possible to calculate the ground displacements in the central area.
(c) Typo corrected.
(d) Typo corrected.

CHANGES IN THE MANUSCRIPT:
(a) page line 21: tunnel entrance → main support area.
(b) No changes in the manuscript.
(c) page 11 line 24: addtional → additional.
(d) page 11 line 25: calcultes → calculates.

4.5 COMMENT: page 11 lines 27-32: *To validate the displacement maps of the DInSAR analysis of the nuclear test, Pleiades data sets from August 26th and from September 8th 2017 were processed to reveal surface characteristics related to test (Figure 13). Change detection analysis show numerous landslides activated during the test and aftershocks.* How does the results validate each other? For the reader it is not clear how they are related. Please show the locations of the purple patches also in map view to make the results comparable.

RESPONSE: The temporal and spatial connection of the strong landslides (results obtained from the Pleiades data set) and the nuclear test allow the conclusion, that the landslides are caused by the test. We added a sentence in the text to underline this connection. Also the figure was changed according to your suggestions.

CHANGES IN THE MANUSCRIPT: Figure 13 is changed according to your suggestions. Also a sentence is added on page 11 line 29 to underline the relation between landslides and test/aftershock: *This relation is concluded by the strong temporal and spatial connection of the landslides to the test and aftershock origin time and epicenter locations.*

4.6 COMMENT: page 11 lines 32-33: *As a result of the processing of the ALOS-2 data, an area of around 3×4 km2 can be delineated, where surface movement rates range between -10 and 10 cm.* Is this result of relevance for the conclusions or discussion? Can you show these results? Where is this area located?

RESPONSE: This is just an observation of how large the effected surface area is. It simply underlines the strenght of the 2017 test in comparison to the former tests. I will add a sentence in the manuscript and also outline the area on the map.

CHANGES IN THE MANUSCRIPT: Added a sentence in text comparing the area of the 2017 event to the January 2016 test. Also modified figure 12 and caption of figure 12.

5 Radionuclide Monitoring and Atmospheric Transport Modeling

5.1 COMMENT: page 12: *During October three peaks containing five samples with 133 Xe activity concentrations between 0.5 and 1 mBq/m3 were measured at the station RN58, which went back to operation in between, atthe days indicated by the forward simulations.*
(a) Please describe the data that you want to explain by modeling at the begin of the section. Both data-sets with positive detections should be described, the one from South Korea that you want to disprove coming from the test site, as well as the one from RN58, that might be explained by a late release from the test site.
(b) What is the exact timing of the detections at RN58?
(c) Why did you choose October 4th and not any other day in October to run your simulations?

RESPONSE:
(a) The analysis of the raw radionuclide data is not within the scope of this study. The radioxenon concentrations measured in national capacity in South Korea (north-eastern stationary measurements) are taken from the cited press release. Please note that we don not "want to disprove" a suggested origin of the radioactive xenon isotopes, we just figured out that the overlay of the simulated source regions indicate another coincident source region than the test site. The xenon measurement data taken at RN58 are confidential according to the CTBT framework. We used the collection times of samples for which an elevated concentration of 133Xe was reported to the State Signatories for the timing of our backward simulations.
(b) The backward calculations for RN58 were made for 12 hour air samples from 00:00-12:00 (UTC) on 5th, 6th, 19th, 26th and 27th of October 2017.
(c) The forward dispersion modelling starting October 4th is complementary to the backward simulation from RN58 on October 5th. It is just one example for the fairly direct atmospheric connection from the test site to Ussurysk. Similar simulations are available for the other dates with elevated 133Xe at RN58.

CHANGES IN THE MANUSCRIPT:
(a) page 12 line 29: text changed for better understanding.

(b) page 12 line 29: Added sample times in the text.
(c) page 12 line 27: Text adjusted for clarification.

6 Discussion

6.1 COMMENT: page 13 line 14: *The depth phase modeling of the P-waveform was performed at 2.5 Hz and found a centroid depth between 400 m and 800 m, which is within the expected resolution of about half the wavelength.* What do you want to state here? Seismograms have been filtered between 0.5 and 2.5 Hz (See caption Fig. 3). You do not show the spectral content of the signal. Clearly 2.5 Hz is only the upper limit. If 400 m is half a wavelength, lambda=800 and c=lambda*f = 800*2.5=2 km/s for the upper limit and c=800*0.5=400m/s for the lower limit. This is too low for P-waves velocities, so I do not get the point here.

RESPONSE: As mentioned in the paper, we used CRUST 2.0 profiles at the source and receiver side for waveform modelling. According to the regional model at the source side, the expected P-wave wavelength is aprrox. 1000 m, which indicates that the depth is still resolvable. This point is made more clearly in the revised manuscript.

CHANGES IN THE MANUSCRIPT: Revised the text on page 13 line 12-16 for clarification.

6.2 COMMENT: page 14 line 2ff:
(a) main event around eight minutes later a large → main event, around eight minutes later, a large.
(b) that this is event → that this event.
(c) event is at least a partial → event is caused by at least a partial.

RESPONSE:
(a) Changed as suggested.
(b) Typo corrected, thanks for the hint.
(c) Changed as suggested.

CHANGES IN THE MANUSCRIPT:
(a) page 14 line 2: main event around eight minutes later a large → main event, around eight minutes later, a large.
(b) page 14 line 3: that this is event → that this event.
(c) page 14 line 3: event is at least a partial → event is caused by at least a partial.

7 Conclusions

7.1 COMMENT: page 15 line 22 : *Explosive character of the September 3rd 2017 North Korean event is confirmed by cross correlation and MTI analysis.* How does cross correlation confirm the explosive character of the event?

RESPONSE: Earlier events where identified as nuclear explosions. High correlations between the 2017 event and earlier events might lead to the conclusion that the 2017 event is also a nuclear explosion. But you are right, it is a little bit measleading and not clear. I removed the term *cross correlation* from the manuscript.

CHANGES IN THE MANUSCRIPT: page 15 line 22: removed the words *cross correlation.*

7.2 COMMENT: page 15 line 26ff: *The estimated yield of the nuclear device is certainly smaller than the largest documented yield ever achieved by a boosted fission device and is therefore still compatible with a fission only device.* On the other hand: would it also be compatible with a small fusion bomb?

RESPONSE: Yes. We cannot exclude that fusion was involved. It is beyond our expertise to speculate if it would be technically even more difficult to limit the yield of a thermonuclear device to a few hundreds kt TNT than reaching the megaton range (as other states did with their first tests of hydrogen bombs).

7.3 COMMENT: page 15 line 27-28: *Strong surface deformations (+-10 cm) are observed in an area of 3x4 km2. Furthermore multiple landslides as well as a number of aftershocks were observed in the aftermath of the test.* This is the observation. What is here the conclusion? → either skip this point or add a conclusion to the observation.

RESPONSE: Please see response to comment 4.4. I will add some text to the conclusion to underline the enormous power of the 2017 test.

CHANGES IN THE MANUSCRIPT: page 15 line 28: Added text underlinig the explosive power of the 2017 test.

7.4 COMMENT: page 16 line 3-4: No immediate measurements of radionuclides related to the test in September were observable, but later occurrences of radionuclides are consistent with a delayed leakage from the test site in October. − > Due to the non-operating radionuclide station RN38, no immediate measurements of radionuclides related to the test in September were observable. However, later occurrences of radionuclides are consistent with a delayed leakage from the test site in October.

RESPONSE: It is not clear if there would have been radionuclide measurements in September with an operational station RN58. If we write *Due to the non-operating...*, it would imply that there would have been measurements. We suggest to leave the sentence as it is at the moment to avoid misunderstandings.

7.5 COMMENT: page 16 line 5: The test site might be strongly stressed and shattered and might be rendered useless for further test activities. − > The test site might have been strongly stressed and shattered and thus rendered useless for further test activities.

RESPONSE: Changed as suggested.

CHANGES IN THE MANUSCRIPT: page 16 line 5: The test site might be strongly stressed and shattered and might be rendered useless for further test activities. − > The test site might have been strongly stressed and shattered and thus rendered useless for further test activities.