Reviewer feedback

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Manuscript title:	*Infrasound and seismoacoustic signatures of the September 28th 2018 Sulawesi super shear earthquake*
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SUMMARY AND RECOMMENDATION

Pilger et al., provide a data analysis on atmospheric infrasound related to the Sulawesi 28 September earthquake as recorded by a set of ground-based stations in Asia and Oceania. The processed infrasound waveform analysis is supported by transmission loss estimates based on parabolic equation-based propagation modelling, and on a semi-empirical formula developed in previous works. The key physical mechanism for generation of these infrasonic acoustical pulses is assumed to be ground-to-atmosphere coupling stimulated by the earthquake-related vibrations.

The authors also map the geographical regions where infrasound is generated using a back-projection approach which utilizes the wavefront backazimuth of arrivals at the stations under the assumption of 300 m/s acoustical wave celerity and 4 m/s solid earth wave speed. This leads to the conclusion that the earthquake main rupture zone and topography nearby are the key regions for infrasound generation.

Moreover, a comparison with other events suggests that the strong infrasound observed for the studied event is not related to the super-shear nature of the earthquake.

Conclusion

I think a manuscript revision will be necessary before this discussion paper can be accepted as an NHESS research article. I look forward to reading the updated manuscript and foresee it to be straightforward for the authors to assess the recommendations given and to implement the relevant edits.

Recommendations on specific aspects

1) Super-sherar related analyses:

   I recommend providing a more concise background on why it is interesting to understand if the observed strong infrasound waves are related to the super-shear nature of the earthquake. Then the discussion and comparison with other earthquakes can be linked to the added background information. For example, it could be explained more precisely what is a super-shear earthquake and it could be mentioned that these can be associated with an effect analogous to acoustic sonic booms with the source traveling faster than the wave propagation speed.

Since the detectability is naturally influenced by the presence of infrasound arrays within the waveguides available from the source, the authors could consider complementing Table 2 with additional information on the infrasound array network available around each of the listed events.
What are the criteria used for the assessments *weak infrasound* and *strong infrasound* in Table 2? I would also recommend stating the distance between event and station, instead of no or vague information about distance (*nearby* / *remote*).

It would be informative with a concise explanation why the registrations (or non-registrations) of seismic arrivals on the infrasound stations are relevant to the assessment? (Otherwise, I would recommend removing this information from Table 2.)

2) Back-projection analysis:

   I would recommend reinforcing the discussion on the back-projection analysis. Based on the current displays, I find it hard to analyze what are the key wavetrain segments on the different stations that contribute to the different highlighted geographical regions shown in Fig. 5.

   Maybe it would be useful to provide an additional back-projection result map per station, either in the paper body or in an appendix? This might make it more straightforward to the reader to see, e.g., if the two blue colored source regions at around 127°E are related to signals on one or on multiple stations. This could also help providing insight in the way each of the stations constrains the source region estimates on Sulawesi. As a suggestion, the coloring of the added per-station maps might be a function of the reduced time for the corresponding infrasound detections? This could then allow for an interpretation of links between array parameter output as a function of time (and the associated waveforms) in the context of the different source regions and features.

   A separate back-projection image per station would also allow the reader to better follow and assess the discussion in the last paragraph of Section 3.

3) Suggested references to add:

   - Studies including back-projection of infrasound recordings: Assink et al. (2018); Walker et al. (2013).

   - A study which shows observations of infrasonic waves generated by seismic surface waves along the Rocky Mountains: Young and Greene (1982).

4) Atmospheric model product:

   From which ECMWF atmospheric model product are the wind and temperature fields extracted?

5) Transmission loss:

   The transmission loss is estimated both using parabolic equation simulations in a lossy medium, and using a semi-empirical formula. However, I find a potential confusion in the mixed use of the concepts of attenuation and transmission loss in the text. Figure 3 and Figure 4 display the results of transmission loss estimates using the two techniques. If you agree on this viewpoint, I propose to streamline the language and the discussion of the associated results to underline that both approaches are applied to calculate transmission loss.

For example, line 137 can be confusing. It could be modified from:
In both attenuation and propagation modeling, data from the European ...
into:
In both the semi-empirical and the parabolic equation-based transmission loss estimates, data from the European ....

Moreover, it would be interesting to see a direct comparison between the two transmission loss estimates as calculated for the station sites.

6) Nomenclature:

   - I suggest homogenizing the wording related to the concept of *back-projection.* The paper also uses *backtracking* and *back-tracking*, while some other works in addition consider a more general concept of *back-propagation.* If the mix is intentional, a more concise explanation is needed. Otherwise, I suggest using only one of the constructs in the text.

   - The paper could be clearer and more consequent on the use of the different physical infrasound and seismic wave generation processes considered in observations, analyses, and figure labels. Currently, the constructs *epicentral infrasound; seismoacoustic signals; secondary infrasound; seismoacoustic precursors; seismoacoustic successors; secondary signals* are used, and I think the readers would appreciate a consolidated use and definition of these concepts.
     I'm not sure whether the definition of *seismoacoustic waves* provided on Line 79 is appropriate:
     ... also highlight infrasound generated from secondary phenomena like remote ground motion of mountain chains or extended basin areas, and from tsunami waves hitting the coastline. This secondary infrasound is often called seismoacoustic waves, ...
     Is epicentral infrasound supposed to be included in your definition of a seismoacoustic waves?

Specific text edit suggestions

1) Line 107:
   SING data is not shown, which makes it difficult to assess the summary given on these data signatures. I propose including a SING signal plot either in the paper body or in an appendix.

2) Line 116:
   I would advise including a reference or explanation to describe what is an NDC and what is the NDC-in-a-box software.

3) Line 124:
   It needs to be better clarified what is meant when claiming that *the propagation of signals* can be identified from the apparent velocity and frequency content.

   I assume this would be related to identification of the atmospheric ducts penetrated by the acoustic waves (tropospheric / stratospheric / mesospheric / lower thermospheric?

4) Line 130:
   I suggest including a concise statement of the parameters that go into the calculation, preferably complemented by the full equation for the semi-empirical transmission loss estimates. Is this equation 2 in Le Pichon et al. (2012)?

5) Line 165:
   How is the signal duration estimated?

6) Line 168:
   I'm not sure celerity is best described as *speed over ground*? Maybe you could use, e.g., *ratio of the range to the traveltime*, or *epicentral distance divided by the traveltime*?

7) Line 178 (Figure 2):
   - Would a separation of this figure into four labeled subfigures (one per station) make the grouping of the subpanels more apparent? I think the readability of Figures 2–4 would also benefit from using a larger text label font (or larger figures).
   - The middle panel labels for each station say *signal speed*, while the paper text otherwise uses *apparent velocity*. I think this should be consolidated.
   - Suggested clarification: *are sorted by distance from above* $\longrightarrow$ *are ordered by epicentral distance.*
   - The caption should explain what the labels *SA* and *IS* denote. (Possibly the label *IS* should also be modified – see the related nomenclature remark above.)
   - Would it be possible to indicate celerity values related to the earthquake epicenter and origin time in the horizontal axis?
   - What apparent velocity is applied in the generation of the beams plotted in the lowermost station panels?
   - For the topmost station data analysis panels, the color scale goes from 0° to 180°, indicating that the display shows the absolute value of the backazimuth deviation? Intuitively, I would assume that plotting the deviation including its sign could provide additional information to the reader, for example regarding the presence of an *azimuthal sweep* as discussed in Section 3?

8) Line 189 and the rest of this paragraph:
   - When stating *all four arrays*, I assume you are referring to the four arrays displayed in Figure 2?
   - I cannot clearly distinguish the mentioned *quasi-continuous seismic waves* in the IS40 and IS30 displays.
   - The reference to *source: USGS* needs to be specified. You could also consider marking the USGS bulletin aftershock arrival times in Figure 2.
   - Last sentence: not only the apparent velocity, but also the backazimuth deviation can provide information valuable for identification of these phases. You also could consider moving the last sentence to the beginning of the paragraph.

9) Line 211:
   The reported *azimuthal sweep* between 3° and 8° is difficult for me to distinguish

in Figure 2. I would recommend a separate figure which plots backazimuth as a function of time for the different stations during the relevant time window. This would allow for an assessment of the mentioned azimuthal sweep present at IS07 (as well as the absence of a sweep at the other stations).

10) Line 228:
...their later arrival time and lack of high-frequency content correspond to the long lasting signal families following the main signal peak for many minutes in the low frequencies. These signal families can be observed together with low-frequency seismic wave activity and low frequency acoustic components from the stratospheric ducting, discernible only to a certain degree by the apparent velocities and arrival times.

    Is there a way to guide the reader on how to find these thermospheric arrivals in the data shown in Figure 2? For example, you could highlight the relevant time and frequency regimes with boxes.

    The second sentence could benefit from being split.

11) Line 270:
I recommend making it more clear to the reader what kind of estimates that were made using the INFERNO software.

    Is the claim that the acoustic energy is concentrated around 0.2 Hz based on estimates made in the current study, or is it based on general knowledge from previous works on earthquake-generated infrasound?

    Does this refer to the energy spectral content at the source or at the stations?

    As a suggestion, you could provide a display of the infrasonic spectral signature(s) at the station(s), and/or an estimate of the spectral components at the source – depending on what is the most relevant.

12) Line 315:
Would it be useful to also list the celerity values found in the simulations?

13) Line 320:
...celerities of those stratospheric ducts to be in the order of 290 m/s.

    Can you quantify what is meant by *in the order of*?

14) Line 329:
My understanding is that the PAPE code supports a range-dependent atmospheric model when simulating the wave propagation and estimating the associated transmission loss. However, from the last sentence in the Figure 4 caption, I get the impression that you indeed consider a 1-D v_{eff} profile?

    If a 1-D approximation of the atmosphere is applied (which might well be an appropriate approximation), I think it should be clarified why this is appropriate, e.g., by showing how much the v_{eff} profiles vary along the great circle connecting the event and the stations.

15) Line 332:
Maybe you could also show propagation results generated for a lower frequency?

16) Paragraph starting on line 361:

    - For what time intervals (and frequency ranges) are the respective station array processing output back-projected?

- The sentence starting with *The uncertainties of the measurements ...* is difficult for me to interpret.

17) Line 377:
The colorbar title *Number of events* can be confusing, because we consider signals from a single earthquake event. Is it appropriate to instead write *Number of PMCC pixels*?

18) Line 393:
As mentioned also above [*Recommendation on specific aspects*, remark 2)], the discussion regarding association between station-data segments and signatures in the consolidated back-projection map would be facilitated if separate maps were provided per station.

19) Line 483:
The relevance of this study to CTBT verification and related infrasound monitoring is not so clear from the text. Maybe links can be provided to the objectives of studies like Assink et al. (2018, 2016); Bowman (2019); Gaebler et al. (2019)? [Which could be given in the Introduction if preferred.]

Technical remarks and corrections

1) Title date format:
Looking at previous NHESS papers, it seems like dates in the title are given in the format “28 September 2018”, instead of the format “September 28th 2018” used in the submitted manuscript.

2) Line 55:
Is there a reference or DOI available which allows for citing the USGS analysis of the event?

3) Line 65:
Suggested clarification: *The intense ground shaking of either the epicentral region or the or the nearby topography from the Sulawesi earthquake* ⟶ *The intense ground shaking of either the epicentral region or the topography nearby the Sulawesi earthquake*

4) Line 69:
Suggested clarification: *to highly sensitive infrasound arrays* ⟶ *to be recorded at highly sensitive infrasound arrays*

5) Line 83:
Suggested replacement: *Although there is quite a large number of studies* ⟶ *Although there are many studies*

6) Line 88:
Suggested replacement: *of a* ⟶ *related to a*

7) Line 89:
Suggested replacement: *Therefore, one of the main tasks of* ⟶ *Therefore, a main objective*

8) Line 99:
Suggested replacement: *Data from various infrasound arrays of the International Monitoring System (IMS) established under the Comprehensive Nuclear-Test-Ban Treaty (CTBT), are used within this study* ⟶
This study mainly considers data recorded at infrasound arrays of the International Monitoring System (IMS) established under the Comprehensive Nuclear-Test-Ban Treaty (CTBT).

In addition, a reference could be given to key literature describing the IMS.

9) Line 111:
I think the epicenter symbol shown is rather a star than an asterisk? (Not sure.)

10) Line 197:
Suggested clarification: *since the local infrasound observations generated from* ⟶ *microbarometer output generated from*

11) Line 198:
Suggested clarification: *fairly well as seismic arrays here* ⟶ *fairly well as seismic arrays for this event*

12) Line 206:
Suggested clarification: *. . . since the back-azimuth calculations as well as the beam-forming are focused on the respective theoretical back-azimuth for the epicenter calculated for each station.* ⟶ *. . . where the array beams are focused towards the earthquake epicenter.*

13) Line 237:
As a service to readers not familiar with microbaroms, I would suggest providing a sentence or two plus some key literature reference that explains what is a microbarom signal.

14) Line 243:
Suggested clarification: *acoustic velocities* ⟶ *acoustic apparent velocities.*

15) Line 248 (and the final paragraph of Section 3):
To facilitate reading this discussion, islands mentioned in the text could be labeled in at least one of the map figures.

16) Line 255:
This sentence can be hard to follow, I suggest to re-formulate and/or split it.

17) Line 287:
This sentence can be hard to follow, I suggest to re-formulate and/or split it.

18) Line 337:
The stability of the ducting conditions are best expressed by quantifying the effective sound speed . . .

I don't expect the effective sound speed ratio is providing the *best* estimate of the acoustic duct availability. For example, wave-propagation modelling can provide a more detailed analysis. Possible alternative formulation:

The availability of atmospheric ducts can be quantified using the effective sound speed . . .

19) Line 346:
This sentence is not so clear to me.

20) Line 396:
Suggested replacement: *ground movement* $\longrightarrow$ *ground motion.*

21) Line 461:
I recommend to split and re-formulate the sentences in this paragraph.

22) Line 467:
I recommend to split and re-formulate the sentences in this paragraph.

23) Line 473:
This statement is vague – maybe it can be omitted without loss?

24) Line 478, sentence starting with *Taking into account ...*:
Can this be re-formulated and split?

References

J. Assink, G. Averbuch, S. Shani-Kadmiel, P. Smets, and L. Evers. A seismo-acoustic analysis of the 2017 North Korean nuclear test. *Seismological Research Letters*, 89(6): 2025–2033, 2018.

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