Review for "Seismic interferometry in the presence of an isolated noise source" submitted by Schippkus, Snieder and Hadziioannou

The authors faced an unexpected phase in the ambient / urban noise cross-correlations of a dense seismic array covering the Vienna basin. They observed waves emitted by a small cluster of localized sources (a wind power plant). In this manuscript, they describe and illustrate the character of this signal and formulate a basic model to understand its propagation assuming it is a far-field surface wave. Based on this model, they analyze the potential problems for travel-time measurements and propose a strategy to reduce these.
The paper is well written and concise, and the illustration of a very clear isolated source effect on the dense array is very instructive. The good fit of the model with the observed data is at least visually very impressive. The paper is quite short, so I have mostly minor comments. A part of the discussion appears to contain statements that do not quite do previous studies justice, and I suggest to cite one additional reference. Comments are appended below. In addition, I will list any typographic issues I spotted in order to facilitate the typesetting.

Major points

- Eq. 9: only if we assume a medium with homogeneous propagation velocity (and including that the boundary sources are on a circular boundary, as should be specified somewhere above)

- L. 135, the first time both contributions are clearly recovered simultaneously: Maybe this statement is a little bit bold, because "spurious" arrivals of a similar amplitude as the "background" arrivals have been observed a number of times already. Notably the study of signals from Aso volcano by Zeng & Ni 2010 shows (their Fig. 2C, reference appended) signals with a particular, non-Green's function-like move-out that are due to a localized source, which is basically an observation of the same effect but not with a dense array and not using the illustrative example of a single master station (also Fig. 4 of Retailleau et al., 2017).

More importantly, there is in my opinion no difference between the "spurious" arrivals observed by Retailleau et al. (2017) and Zeng & Ni (2010), and the arrival that is observed here: Their observations are also due to surface waves from comparatively localized sources (which, depending on scale, may be somewhat more extended).

These do not necessarily contain cross-terms (the editor may weigh in on this if I am mistaken). Instead, as surface waves from sources that are very localized, their contribution to the correlation appears because there is a lack of similar, but just out-of-phase surface wave terms from neighbouring source areas, which would help cancel them out (as described in Snieder (2006), Tsai (2009)); the localized sources in all three studies are outside the stationary phase zone for many receiver pairs. If these sources were more extended, the rapid oscillations in space of the differential travel time with respect to the two stations would make them cancel out. The same principle applies in all three studies. All observe un-cancelled surface waves due to a localized source and all these signals are physical.

However, the study by Li et al. (2020) investigates actual cross-terms between body wave phases propagating along different paths through the deep Earth, and is a different case, as stated in the discussion.

Minor points

- In the introduction it would be useful to already introduce that the analysis will focus on a single component and on surface waves just to prevent any confusion especially for readers who are not that familiar with the field and may think, when entering section 4: Huh? Why surface wave Green's functions now?

- L. 16, I think Weaver is referring to any modes of seismic waves

- L. 27 are recordings for co-located geophones stacked?

- Fig. 1 shows a lot of wind turbines; does this mean that the ones at Prottes-Ollersdorf are more efficient at generating coherent noise and why?

- L. 45 ff please specify if the boundary is circular

- L. 48 expected value?

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- L 88 how is the Ricker wavelet convolved with the surface wave Green's functions? (or is it shifted according to travel time?); is amplitude considered (e.g. as geometrical spreading?)

- L 167/168 this sentence has an additional verb and is hard to understand.

- Supplement: Does the Model with several sources fit better visually, or also in terms of misfit / Correlation coefficient / ?

Typesetting:

- frequently the genitive with 's is used where it might be more appropriate to use "of ..". e.g:
L. 31, sources' impact
L. 85, the isolated source's

- L. 44 missing word: combination of noise sources

- Eq. 1 ff: the two sets of dashes (x', x'') are different in appearance, this could be homogenized during typesetting

- Eq. 4, first line: a closing triangular bracket is missing and a complex conjugation (*) of the second noise source term $N_B(r'')$ is missing

References mentioned:

Zeng, Xiangfang, and Sidao Ni. "A persistent localized microseismic source near the Kyushu Island, Japan." *Geophysical Research Letters* 37.24 (2010).

Reviewer 2

This paper uses a large-N array in Vienna to study the effects of isolated noise sources on seismic interferometry, including both observational and theoretical studies. The author's expression and writing are pretty clear. This is a unique and very interesting discovery that I personally think deserves to be published, but I hope that the following issues can be resolved before publication.

Major points:

1) There are many wind turbines shown in Figure 1, but why only the red cross-marked wind turbine (The wind farm Prottes-Ollersdorf) produces seismic noise, and the others are not operating? In addition, is the energy of the seismic noise related to the wind field? For example, when the wind speed is small, the impact of the isolated source on the seismic interferometry is relatively small?

2) The filter frequency band used by the author is 0.5-1 Hz, but the noise frequency band generated by wind turbines is not discussed. So I think the authors should at least be sure that the noise they're getting is coming from a wind turbine. I think the noise produced by wind turbines may be slightly higher than 1 Hz. Has the author considered other frequency bands in their calculations?

3) I'm just curious, what is the observed velocity error when multiple isolated noise sources are only on one side? As in actual observations, most of the noise is due to the ocean and is only distributed on one side. So I think this research can be very helpful for actual observations. Would this velocity measurement error be reduced if multiple isolated noise sources were present at the same time?

Minor points:

- Line 27: What is the instrument model of geophone?

- Line 29: What are the dominant frequencies of these noise sources? Will it affect the results of seismic noise interferometry in your experiments?

- Line 33: Did you remove the earthquakes from the raw data?

- Line 77: The “+” should be “-” in the middle of the equation?

- Line 118: Delete one of the “Result”.

Answers to the reviews

Reviewer #1

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- The other wind turbines produce lower energy, as observed in Schippkus et al. 2020. We believe this is consistent with our observations that they do not seem to produce significant contributions to the cross-correlations. We clarified this in the text. Early tests have shown that stacking correlations only from quiet times (where wind speed is below operational specification of the wind turbines) reduces the amplitude of the wind farm contribution but does not eliminate it entirely.
  ll. 49-55
  ll. 185-187

2) The filter frequency band used by the author is 0.5-1 Hz, but the noise frequency band generated by wind turbines is not discussed. So I think the authors should at least be sure that the noise they're getting is coming from a wind turbine. I think the noise produced by wind turbines may be slightly higher than 1 Hz. Has the author considered other frequency bands in their calculations?

- The wind turbines are indeed the most energetic source of energy at these frequencies, as was shown in Schippkus et al., 2020. To give the reader more confidence, we added a recent reference on seismic wave generation by wind turbines, which also confirms that the observed frequencies are generated by wind turbines: Neuffer et al. 2021.
  ll. 49-55

3) I'm just curious, what is the observed velocity error when multiple isolated noise sources are only on one side? As in actual observations, most of the noise is due to the ocean and is only distributed on one side. So I think this research can be very helpful for actual observations. Would this velocity measurement error be reduced if multiple isolated noise sources were present at the same time?

- Indeed, the velocity error decreases when multiple sources are present and interfere. We now discuss this briefly in the discussion. As it relates to isolated sources being only on one side, we believe this can be seen in Fig. 3, where this condition is met for all station to the South-West of r_M. We included this point in the discussion.
  ll. 223-230
  ll. 250-255

Minor points:
- Line 27: What is the instrument model of geophone?

- Added the info to the text. They are Sercel JF-20DX, recorded by AutoSeis High Definition Recorders.
  ll. 32-33

- Line 29: What are the dominant frequencies of these noise sources? Will it affect the results of seismic noise interferometry in your experiments?

- We added a sentence to mention that they are negligible.
  ll. 52

- Line 33: Did you remove the earthquakes from the raw data?

- There are no earthquakes present in the data, which we added to the text. We did no further processing other than the processing described in the text.
  ll. 40-41

- Line 77: The "+" should be "-" in the middle of the equation?

- Fixed.
  ll. 91

- Line 118: Delete one of the "Result".

- Fixed
  ll. 132

Reviewer #2

The authors faced an unexpected phase in the ambient / urban noise cross-correlations of a dense seismic array covering the Vienna basin. They observed waves emitted by a small cluster of localized sources (a wind power plant). In this manuscript, they describe and illustrate the character of this signal and formulate a basic model to understand its propagation assuming it

is a far-field surface wave. Based on this model, they analyze the potential problems for travel-time measurements and propose a strategy to reduce these.

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Major points

- Eq. 9: only if we assume a medium with homogeneous propagation velocity (and including that the boundary sources are on a circular boundary, as should be specified somewhere above)

- We clarified that this is for homogeneous medium velocity. It is our understanding that the boundary needs to be closed and evenly distributed but not necessarily circular. ll. 80

- L. 135, the first time both contributions are clearly recovered simultaneously: Maybe this statement is a little bit bold, because “spurious” arrivals of a similar amplitude as the “background” arrivals have been observed a number of times already. Notably the study of signals from Aso volcano by Zeng & Ni 2010 shows (their Fig. 2C, reference appended) signals with a particular, non-Green’s function-like move-out that are due to a localized source, which is basically an observation of the same effect but not with a dense array and not using the illustrative example of a single master station (also Fig. 4 of Retailleau et al., 2017).

More importantly, there is in my opinion no difference between the “spurious” arrivals observed by Retailleau et al. (2017) and Zeng & Ni (2010), and the arrival that is observed here: Their observations are also due to surface waves from comparatively localized sources (which, depending on scale, may be somewhat more extended).

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However, the study by Li et al. (2020) investigates actual cross-terms between body wave phases propagating along different paths through the deep Earth, and is a different case, as stated in the discussion.

- We thank the reviewer for bringing this to our attention. Indeed, Zeng and Ni 2010 observe the same effect and confirm our considerations, e.g., that contributions by such sources emerge at negative timelag and when the boundary-source contribution passes the isolated source location. We also believe Retailleau et al. 2017 show the same effect but reversed in time. The reviewer's comments also motivate a better distinction of the two types of spurious arrivals: localized sources vs. cross-terms. We have adapted the discussion to accommodate the points above.
  ll. 152-175
  ll. 264-267

Minor points

- In the introduction it would be useful to already introduce that the analysis will focus on a single component and on surface waves just to prevent any confusion especially for readers who are not that familiar with the field and may think, when entering section 4: Huh? Why surface wave Green's functions now?

- Added a sentence.
  ll. 23-24

- L. 16, I think Weaver is referring to any modes of seismic waves

- Fixed.
  ll. 20

- L. 27 are recordings for co-located geophones stacked?

- Yes, we added a few more details.
  ll. 32-33

- Fig. 1 shows a lot of wind turbines; does this mean that the ones at Prottes-Ollersdorf are more efficient at generating coherent noise and why?

- See our relpy to reviewer #1's comment on this. We have adapted the relevant text.
  ll. 49-55

- L. 45 ff please specify if the boundary is circular

- See our reply to the first major point.
  ll. 57

- L. 48 expected value?

  - Fixed.
    ll. 62

- Eq 3, please specify S and the integration bounds of S

  - We adapted the equations to follow the notation in Wapenaar 2005 more closely. We hope this makes it clearer.
    ll. 57, 64, 70
    eq. 3, 4, 5

- Equations in section 3: Complex conjugate is applied to the signal received at the Master station. The complex conjugate term in the correlation is the one at which signals arrive earlier. So I am not sure if this notation is consistent with the model and the observations; please double check the notation
(https://www.dsprelated.com/freebooks/mdft/Correlation_Theorem.html)

  - We also follow the convention that the master station's recording is time-reversed (complex conjugate). Our notation is consistent with this.

- L 88 how is the Ricker wavelet convolved with the surface wave Green's functions? (or is it shifted according to travel time?); is amplitude considered (e.g. as geometrical spreading?)

  - We are not quite sure what the first question is aimed at. The Green's functions in our model are simply dirac-delta functions with their peak at the travel time. Convolution with this GF is the same as shifting them. We do not consider amplitude decay in our modelling, which could have an impact on measurement errors, but distracts from the main message. We added a sentence on this.
    ll. 104-105

- L 167/168 this sentence has an additional verb and is hard to understand.

  - No longer applies after rephrasing of this part of discussion.

- Supplement: Does the Model with several sources fit better visually, or also in terms of misfit / Correlation coefficient / ?

  - We have not tried to quantify the fit, because we do not aim to accurately reproduce the actual recordings in this study. This would require knowledge of the source terms, which we do not have. We believe the visual comparison is already quite convincing and sufficient for what we aim to communicate. Replaced "improves" by "appears to

improve”
ll. 236

Typesetting:

- frequently the genitive with ‘s is used where it might be more appropriate to use “of ..”. e.g: L. 31, sources’ impact, L. 85, the isolated source’s

- We have adapted the text were appropriate.
  ll. 37, 76, 99-100, 161,

- L. 44 missing word: combination of noise sources

- Fixed.
  ll. 57

- Eq. 1 ff: the two sets of dashes (x’, x’’) are different in appearance, this could be homogenized during typesetting

- We believe this would need to be fixed in the latex template or manually during typesetting.

- Eq. 4, first line: a closing triangular bracket is missing and a complex conjugation (*) of the second noise source term NB(r’’) is missing

- Fixed.
  eq. 4

References mentioned:

Zeng, Xiangfang, and Sidao Ni. "A persistent localized microseismic source near the Kyushu Island, Japan." Geophysical Research Letters 37.24 (2010).