Response letter to the reviews of: Continuous isolated noise sources induce repeating waves in the coda of ambient noise correlations.

Dear Lise,

Thank you considering our manuscript "Continuous isolated noise sources induce repeating waves in the coda of ambient noise correlations" for publication in Seismica. We have reviewed the reviewers' comments carefully and have revised our manuscript in accordance with the comments and suggestions. We hope the manuscript is now suitable for publication.

Our responses to the comments below are marked with arrows and bold text. We attach a clean and a change-tracked version of the manuscript. Line numbers given in our responses below relate to the change-tracked version of the manuscript for easier comparison.

Reviewer A

Review of "Continuous isolated noise sources induce repeating waves in the coda of ambient noise correlations" by Schippkus et al., submitted to Seismica

This is a very interesting paper that examines how the late coda of ambient noise cross correlations may contain information about direct waves propagating from isolated, continuous noise sources such as storms (or, potentially, anthropogenic sources such as trains or wind turbines). The paper presents an example using data from the Grafenburg array to motivate the study, which is mostly a modeling study of how such noise sources might express themselves in ambient noise correlation codas. This is a well written and thought provoking paper which will make a good contribution to the literature on ambient seismic noise correlations. I have a few suggestions that I hope that the authors will find useful at the revision stage, noted below. After revision I expect this will make a very good contribution to Seismica.

Major comment:

- This study is motivated by beamforming observations of the correlation Wakefield at the Grafenburg array for the years 2019 and 2020 that show evidence for energy arriving from the west. The authors propose (line 107) that they represent repeating direct waves emerging at isolated noise source locations in the northeastern Atlantic. This observations is used as the motivation for the modeling portions of the paper (sections 3 and 4), and the discussion and conclusion (sections 5 and 6) focus on the modeling results, and their implications for future studies. However, the authors don't really return to their original observation in the context of their modeling results later in the paper, as I thought they might. In particular, given the highly idealized nature of their models (e.g., with a perfectly repeating or continuously acting source), I was left questioning a bit whether the model results really support the inference of a noise source in the northeastern Atlantic as well as the authors suggest. Wouldn't ocean storms have a much less regular/continuous source than used in the modeling, particularly when considered over two years? The models used highly localized source regions (just off the southwest coast of Iceland, as well as off the Iberian Peninsula), but stormy regions would of course be substantially less localized and would vary over time. I don't think that additional models of less localized/regular sources are needed in this study, but I do think that the paper would be considerably strengthened if the authors return to a discussion of their Grafenburg array results later in the paper, in light of their modeling results, and make a more concrete argument that the results of their highly idealized models are really relevant for the interpretation of their real data.

- ➢ **The temporal stability of ocean microseism sources that we impose in our modelling has been observed on field data correlations before. Zeng & Ni (2010) computed and stacked correlations over one year that show clear spurious energy due to a localized microseism source in Japan. Similarly, Retailleau et al. 2017 found localized microseism sources off the coasts of Iceland and Ireland, also in correlations stacked over one year. It may be unintuitive that ocean microseisms, often assumed to**

be a largely random process, would show any coherence at all. These previous and our results are a clear indication that indeed the secondary microseism mechanism generates coherent sources that are somewhat stable over time. We are, however, not aware of a microseism source model that incorporates all these factors satisfactorily. Instead, we follow the current standard formulation, i.e., each frequency is excited with random but constant phase. Investigations on how varying temporal source stability and stacking influence the beamforming detections or measured velocity changes will be part of future work.

- **We added this argument to the manuscript (ll. 348).**

More minor comments:

- Line 80 - I suggest mentioning to the reader here how BRMO and OJC were chosen as the master stations.

  - **We added a short statement to the text. (ll. 81)**

- Line 83 - why not remove earthquakes at the preprocessing stage? It might be a good idea to explain this choice to the reader, as this is often done in ambient noise studies.

  - **We added a short statement to the text. (ll. 85)**

- Line 150 - "How strongly these repeating direct waves manifest depends on how highly correlated the isolated source is with itself throughout time. The example here constitutes the most extreme case, i.e., identical wavelet and exactly regular excitation pattern." This idealized case seems to be very far from the reality of the Earth, no? Similarly, in the modeling exercise described in the paragraph starting on line 157, the authors compute synthetic seismograms for a very idealized Earth model (homogeneous, isotropic, acoustic half-space) - for the real, anisotropic, heterogeneous Earth, would we expect the direct waves from the isolated source (in this case just SW of Iceland) to be much harder to observe? The authors do seem to come back to this point in their discussion (line 235, and also line 347), but it might be useful to put a brief comment on this point at line 150 as well. It might also be useful to emphasize again, perhaps in the discussion or conclusion, that the fact that the models presented here are highly idealized (excluding heterogeneity, elastic wave propagation effects such as conversions, and scattering), means that the effects of continuous isolated noise sources may be not as large in real data as in the models. (For example, a sentence could be added to the end of the paragraph that finishes at line 348 to emphasize this point.)

  - **We agree that it is surprising that a highly idealized Earth and source model can sufficiently reproduce our observations. However, in our view this means that the impact of continuous isolated noise sources must be quite large on field data (and not smaller as suggested by the reviewer), because the effect is easily observable on field data, where other effects such as heterogeneity etc. are certainly relevant, that our model does not account for. We are currently not aware of another process that could produce the measured beamforming results. We have adapted the text to emphasize this aspect more clearly, also in correspondence with the reviewer's major comment. (ll. 359)**

- Line 176 - "…which are better described as continuously acting sources…" I suggest adding "…continuously acting sources, which we discuss below" or something similar to the end of this sentence. I initially struggled to understand the meaning of this sentence, until I got to section 4. I think a transition such as this would help the reader understand the logical flow of the paper.

  - **Added. (ll. 185)**

- Not a suggestion, just a comment: I found the discussion of the use of sources such as wind turbines or trains for monitoring of velocity variations, and the possibility that they may act as highly correlated repeating sources, to be fascinating! Very cool, and would make for a nice follow-up study to this one.

Recommendation: Revisions Required

Reviewer B

The authors carry out an analysis of the influence of isolated noise sources on ambient noise cross-correlations, building on their previous work on this topic. Through simple numerical experiments, they approximate real observations of coherent energy within the very late coda (100s of seconds after the main arrivals). They demonstrate that repeating isolated noise sources, such as mechanical sources or the microseism, can contaminate the coda of correlation functions with implications for velocity monitoring techniques that rely on the assumption that coda is composed only of coherent scattered wave energy near the seismometer.

The analysis is described very clearly, and the work is completely reproducible as the authors have included the Python scripts used to generate the figures – this is impressive and very much appreciated! Overall, I greatly enjoyed reading this manuscript and believe it is an important contribution to the ambient noise community. Below are some minor questions and suggestions for clarification as well as one relatively minor (but potentially important) caveat regarding the main conclusion that should be addressed.

Sincerely,

Joshua Russell, Syracuse University

The main conclusion of the manuscript is that isolated noise sources can mask coherent scattering in the coda of correlation functions. However, could this simply be explained by the "master" station being far away from the array stations? What is the average spacing between the master station and array for the two examples shown here. The separation in Figure 2 appears to be several hundred kilometers. I would not expect coherent scattering to be strongly observed over several hundreds of kilometers distance as the energy would be unlikely to reach the station before dissipating, and so it makes sense to me that isolated noise sources would dominate the coda at these long distances. Studies of dv/v commonly use closely spaced stations (tens of km) or even a single station autocorrelation. One way to test whether distance plays a factor would be to try beamforming using the array stations only and check whether they are still dominated by the isolated noise sources.

- **The reviewer points out a potentially valid concern, which is why we followed his suggestion to repeat the beamforming measurement on correlations with a nearby station. For convenience, we chose the southern-most station of the Gräfenberg array (GR.GRC2) as the master station. While concerns about plane-wave assumption and inter-station distance are certainly relevant for this geometry, we still observe the same stable behaviour as for the other master stations (see Figure below). We choose to not include this figure in the main text as we think it does not provide sufficient new insight. Instead, we address this aspect briefly in the discussion, where we clarify that the same effect can be observed with nearby master stations. (ll. 258)**

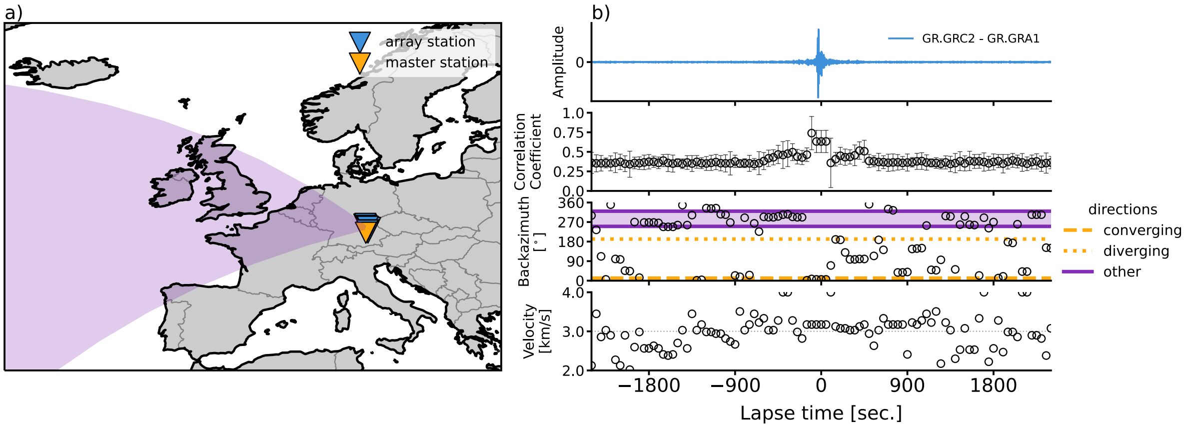

Figure 1: Reproduction of Figure 1 (manuscript) with GR.GRC2 as the master station.

Minor suggestions, edits, and questions:

In Figure 2, the north-northeast backazimuth still appears in the beamforming similar to Figure 1, at a broad range of lapse times. What is the physical meaning of this? In Figure 1 this is called the converging wave, but energy also appears at large lapse times long after the converging wave passes. Could there be another isolated source to the north, perhaps in the Baltic Sea region?

- **We added this aspect to the description of observations a few sentences later. (ll. 111)**

Equation 2: rho and c are not defined in the text

- **Added definitions. (ll. 124)**

Line 156: "If there was no correlation, they would disappear."

To clarify, the repeated waves would disappear, but the largest amplitude central peak would remain – correct? The arrival time of the main spurious arrival should be L/c*cos(theta), where L is the interstation distance, c is velocity, and theta is the angular difference between the azimuth of the spurious noise source and the interstation azimuth.

- **Yes, this is correct. We clarified in the text that these arguments only concern the repeating waves, i.e., the cross-terms within the auto-correlation function. (ll. 163)**

Line 182: It would be helpful to mention in the main text that phase \Phi_i is randomly selected from a uniform distribution between 0 and 2pi.

- **Added to the text. (ll. 190)**

Figure 6: The caption states the Green's function is the same as in Figure 3c, but they are different.

- **Thanks for catching this mistake. Fixed. (Fig. 6)**

Line 259: "the coda, and thus measured velocity changes, may be dominantly sensitive to the path from the isolated noise source to the array station."

I have a hard time understanding how this could be true. The path between the isolated noise source and the array should not strongly influence the correlation functions (aside from scattering, attenuation, focusing/defocusing prior to entering the array). As the isolated noise source is far-field, the correlations should mostly reflect structure between the stations. For example, velocities extracted from beamforming of the coda in Figures 1 and 2 reflect structure between the array and master station, not structure far outside the array – correct? I believe the case of van Dinther et al. (2021) differs in that the scatterer is very nearby the two stations considered (on the order of station separation).

- **Thanks for pointing out that this aspect requires some further clarification. It is important to distinguish between our beamforming results, a standard coda wave interferometry application, and a potential velocity variation measurement on repeating waves from isolated source:**
  (i) In a standard CWI application, coda waves originate at the master station and eventually reach the other receiver. A measured velocity change has then happened somewhere along this entire path. Because there is no clear way to know where exactly the wave "has been" and thus the change has happened, recently developed coda wave sensitivity kernels are statistical descriptions of where the wave might have been, depending on the scattering properties of the medium.
  (ii) In the beamforming results we show here, the estimated velocity (e.g., Fig. 1) is the local phase velocity of the most coherent part of the wavefield. If one would compute repeated correlation wavefields (e.g., daily) and beamform them to estimate velocity, a potentially measured change would be localized within the array, assuming constant sources. However, this is not what we're doing here and not what we're referencing in our statement in Line 259. There we refer to the third case.
  (iii) A potential single-correlation measurement of velocity variations in some part of the coda where repeating waves by isolated sources dominate should be sensitive to the entire propagation path, like

(i). The difference to (i) lies in the origin of the observed correlation wavefield contribution. Our main hypothesis in this paper is that such repeating waves originate from the isolated source, not the master station, see our correlation wavefield sketch (Fig. 4). Therefore, a measured velocity change must have happened somewhere along the path from isolated source to receivers.
We have adapted the text to include the arguments above and make this distinction clear. (ll. 271)

Lines 296–298: I do not understand what is meant in these two sentences regarding temporal resolution of isolated noise sources and "real" time. Please consider rewording this.

- **With "real" time we tried referring to the points in time the seismogram was recorded. We rephrased this to "time in the raw signal domain" to hopefully clarify this point. (ll. 323)**

Line 310 typo: direction

- **Fixed. (l. 338)**

Line 313 typo: repeating

- **Fixed. (l. 341)**

Line 356 typo: opportunities

- **Fixed. (l. 356)**

Recommendation: Revisions Required

Summary of changes to manuscript:

Line numbers refer to the change-tracked version.

ll. 81: addresses reviewer A comment
ll. 85: addresses reviewer A comment
ll. 111: addresses reviewer B comment
ll. 124: addresses reviewer B comment
ll. 163: addresses reviewer B comment
ll. 185: addresses reviewer A comment
ll. 190: addresses reviewer B comment
l. 198: typo
l. 235: typo
l. 239: grammar fix
ll. 258: addresses reviewer B comment
ll. 271: addresses reviewer B comment
l. 314: clarification
ll. 323: addresses reviewer B comment
l. 331: added recent citation for gulf of guinea signal
l. 338: addresses reviewer B comment
l. 341: addresses reviewer B comment
l. 356: addresses reviewer B comment
ll. 348: addresses reviewer A comment
ll. 359: addresses reviewer A comment
l. 369: clarification
l. 399: addresses reviewer B comment
Fig. 6: addresses reviewer B comment
Fig. 6 caption: typo