Solid Earth Discuss.,
https://doi.org/10.5194/se-2019-118-AC1, 2019


# *Interactive comment on* "Observation and explanation of spurious seismic signals emerging in teleseismic noise correlations" *by* Lei Li et al.

**Lei Li et al.**

lilei@ies.ac.cn

Received and published: 26 November 2019

Anonymous Referee #1

General comments:

This study investigates the source of spurious arrivals in ambient noise cross-correlation functions calculated over teleseismic differences. The authors explain that such spurious seismic arrivals can be the result of the interference between seismic phases that have time delays that are 'quasi-stationary', that is, their arrival time difference does not vary strongly with source distance. This effect can occur even if the phases do not share a ray path. The authors use two seismic arrays to demonstrate an example involving the P and PKPab phases. In general, this discussion paper is a very



nice contribution that will be of interest to a wide audience. I have a few comments that I believe should be addressed before publication, but these are probably quite minor. I will go through these comments in the order in which they appear in the manuscript.

==========

Reply: The authors would like to thank the referee for the careful review and helpful suggestions on the manuscript. We modified the manuscript accordingly. Point-to-point responses are provided below.

==========

Specific comments:

- In my opinion, the introduction section of this manuscript is a bit thin on relevant detail. Currently, the authors focus on describing the construction of empirical Green's functions, and briefly mention some of the applications. They consign the majority of the detail to a citation for a review paper. I think this approach is fine when it comes to the empirical Green's function approach, as it isn't really the point of this paper, but I do think the introduction should be expanded to provide more background on the spurious arrivals instead. More specifically, the line of thinking to explain spurious arrivals followed in this paper has already been introduced by Pham et al. (2018), and yet this study has not been cited throughout the current paper. In my opinion, the work of Pham et al. should be presented in the introduction, as it would allow for a nice progression in scientific thinking: Pham et al. focuses on spurious arrivals that share a common ray path, whereas the current study explains those that do not share a ray path. Pham, T. -S., TkalĔĞ ci′ c, H., Sambridge, M., & Kennett, B. L. N. (2018). Earth's correlation wavefield: Late coda correlation. Geophysical Research Letters, 45, 3035– 3042. https://doi.org/10.1002/2018GL077244 - Similarly, there should probably be some discussion involving Pham et al. (2018) in section 7.

==========

Reply: The authors recognize that the initial version of Introduction needs to be extended. We thank the review for this comment. We were aware of the work by Pham et al. (2018) that interpreted spurious phases in earthquake coda correlations with the stationary-phase arguments: "all phases identified in the correlation wavefield correspond to differences between seismic arrivals with the same ray parameter and a subset of propagation legs in common". We initially thought that readers would be confused by an introduction on coda wave interferometry, while we only focus on microseism noise correlations. Ambient wavefields are dominated by ballistic waves from oceanic microseism sources (5 to 10 s periods). Coda waves excited by large earthquakes are dominated by high-order modes at longer periods (> 20 s) and corresponding to core-related reverberations. We notice that it has not been pointed out explicitly in existing literatures that at large scale, ambient noise correlations are distinct from earthquake coda correlations. Not mentioning the latter could also be misleading. As suggested by the reviewer, we have modified the Introduction and Conclusion sections. We have also added a new Fig. S5 that demonstrates the difference between microseism correlations and coda correlations.

==========

- This might just be a language issue, but on page 2, line 5 the authors state that there have only been a few noise-derived body wave signals. Whilst body waves are certainly more rare than surface waves, nowadays I don't think you can say there are only a few examples. Some examples that could be cited, including the retrieval of core phases, include but not limited to: https://doi.org/10.1002/grl.50237 https://doi.org/10.1002/2017GL073230 https://doi.org/10.1093/gji/ggw015 https://doi.org/10.1002/2014GL062198 (Uses the same seismic arrays as the authors)

==========

Reply: The reviewer is correct. It was a typo. We meant fewer compared to surface



waves. Several new citations have been added to the Introduction.

==========

- On page 3, the authors describe an interesting kurtosis-based method for discarding noise segments that are contaminated by earthquake signals. Is this the first case of this method being used for processing ambient noise? If so, a little bit more clarity is needed. In particular, the 'expectation operator' needs explaining to avoid confusion. Is it some kind of mean? I think if the equation defining kurtosis is properly explained around page 3 line 5, that would be sufficient detail for this paper.

==========

Reply: To our knowledge, it is the first time that the kurtosis has been applied to noise data processing. The reviewer is right that the expectation here refers to the mean value. We clarify it being "arithmetic mean" in the revision.

==========

- On page 4 line 20, the authors mention 'numerical experiments'. More detail probably needs to be added here. How were these experiments performed? I assume by simulating plane waves passing over the known array geometries, but it is impossible to tell from the current text.

==========

Reply: The referee assumed correctly. We have added some details on the numerical experiments. "To investigate the resolution capability of the double-array slowness analysis for the FNET-LAPNET geometry, we make numerical experiments by presuming (a) the same slowness at FNET and LAPNET (4.6 s/deg), and (b) different slownesses at FNET (4.7 s/deg) and LAPNET (4.2 s/deg). Assuming plane waves passing through FNET and LAPNET, the time delays between FNET and LAPNET station pairs can be calculated from Eq. (1). The wavelet of the observed spurious phase (5 to 10 s bandpass filtered) is convolved with the time delays to synthesize the correlation functions. The synthesized correlations are beamed by Eq. (2) for various slownesses. The results are plotted in Fig. 6. In both cases, the slownesses of the correlated waves at FNET and LAPNET are well resolved, justifying the reliability of our slowness discrepancy estimation in Fig. 5a."

==========

- Similarly, on page 4 line 20 - 21 the authors quote the simulated slowness values for their numerical experiment, but on the first read it appears as if these values drop out of thin air until it is explained that they match the observed slowness from the real data on line 25. I would suggest that the order of the explanation is changed here so that it is clear that the numerical experiment simulates the observed slowness values.

==========

Reply: We agree that exchanging figs 5 and 6 and relevant text improves readability. Thanks for this suggestion.

==========

- On page 5 lines 5 - 15, the authors explain how they identify the relevant interfering phases. In the current form, the explanation is slightly convoluted and hard to follow. I think it would benefit if the authors explicitly state that the culprits are a P-wave sourced 89 degrees from, and recorded at, FNET, and PKPab sourced 152 degrees from LAPNET. At the moment the arrays at which each phase is recorded is only implied by the text, when it is key to identifying the source region.

==========

Reply: We agree with the proposed clarification and have modified the statement accordingly.

==========

- The authors comment on page 5 line 12 that PcP-PKPab also matches the required



time delay. Is this candidate discarded due to an incorrect slowness for PcP? Again it isn't stated, but only implied. Perhaps the PcP slowness should be quoted here too to drive the point home.

==========

Reply: Yes, the slowness comparisons are critical. We have clarified this point and quoted the PcP slowness.

==========

- A minor confusion occurs on page 5 line 17, the authors state that Fig. 6 can be used to located the source responsible, when in reality Fig. 6 only gives you the source distances. Unless I'm mistaken, to actually locate the source you need other information such as the array locations, and whether the source is causal or acausal.

==========

Reply: Fig. 6 provides source-receiver distances. Receiver locations are of course necessary for locating the sources. We have clarified that in the revision.

==========

- The supplementary material is currently just a pile of a couple of figures referred to in the main text. I think the supplementary material should include the information required to stand on its own. I think a couple of sentences explaining each supplemental figure, and its relevance to the main text, are warranted.

==========

Reply: Subheadings and explanatory sentences have been added to Supplementary.

==========

Technical comments: - On page 8 lines 4 - 7 there are a few sentences that don't make much sense, and are grammatically incorrect. I suggest the authors rewrite these sentences to clarify.

==========

Reply: We have rewritten this part. Thanks for pointing this out.

==========

- In Fig. 4, on the bottom vespagram, ’spurious’ is missing an ’s’.

==========

Reply: Corrected. Thanks.

==========

In conclusion, I believe that in order to provide the clarifications and explanations that I have requested above, it is likely only a minor revision will be required.

---

Interactive comment on Solid Earth Discuss., https://doi.org/10.5194/se-2019-118, 2019.