Response to reviewers for the manuscript: Unlocking DAS amplitude information through coherency coupling quantification

Key:
Comments from editor/reviewers – black
Responses from authors – green

Editor and reviewer comments, with author responses:

Editor:

Both reviewers agree that your manuscript is suitable for publication after some revisions. In particular, Reviewer 2 highlighted two major concerns: (1) the definition of cable coupling and the theoretical framework. The current version does not adequately justify the use of the 1-D single-component model for characterizing coupling. (2) The effects of structural heterogeneities, which require further clarification and analysis.

We thank the editor and reviewers for their time and comments that have helped improve the manuscript. Below, we detail how we have improved the manuscript to address the reviewers' comments. Hopefully the manuscript is now improved and in a good state for publication.

#Reviewer 1
As an emerging tool, DAS has been increasingly widely used in seismic observation and subsurface imaging. The manuscript addresses one of the key issues relating to the application of DAS in observing earthquakes. The authors present a concise method to estimate the degree of coupling. Their result also indicates that, to the first order, the coupling of fiber can be simply treated as bimodal. The proposed method is reliable, and the conclusion contributes to the DAS application. The manuscript is well-written and worthy of a prompt publication. Some minor issues for the author's reference.

We thank the reviewer for their review and are glad that they found the method and manuscript clear and well written. Below we address the minor issues raised in detail.

1. They also formulated the effects of attenuation, but less discussed thereafter. For instance, the ultra-low quality factor is sometimes observed in near-surface, how will this affect the coupling estimation?

This is a good point. We had thought about this previously, but sorry that we hadn't discussed such ideas in the text. We now add text in Section 3.4.2 (L575-583) to briefly discuss this issue. Basically, we cannot isolate near-surface (sub-gauge-length) and coupling attenuation effects. We propose in the text that this doesn't really matter in practice, since for any amplitude-based analysis, one has to quantify both coupling and near-surface attenuation anyway, and it is unlikely that one would often isolate the two, unless one were really interested in very near-surface attenuation that probably can't ever be resolved in practice. The text now hopefully makes the reader aware of this limitation. Thanks for raising the point.

2. L406, "Second, we convert from strain-rate to displacement via direct integration". To make the study reproducible, more details are needed.

Thanks, good point. We have now added a sentence describing the exact integration steps and the limitations (i.e. linear stretches of fibre). Changes can be found at L427-429.

#Reviewer2
Dear Editor and Authors
This paper discusses the estimation of fiber optics (FO) coupling to the ground using a simple DAS (Distributed Acoustic Sensing) channel-to-channel correlation technique. It is supported by a minimalist theoretical framework, and some interesting examples are provided. The paper is well-written and clear. The topic is important, as DAS is an emerging instrument that has attracted significant interest for its high potential. Ground coupling is one of the main challenges for this technology. I have a few concerns, but I recommend its publication in Seismica after addressing these moderate to minor comments.

We thank the reviewer for their positive assessment of the manuscript and constructive comments that have definitely improved the manuscript. Below, we describe in detail how we have addressed their comments in order to clarify and improve the work. Hopefully these changes have sufficiently improved the manuscript for publication.

Major Comments
Definition of Coupling: My first question is about the definition of coupling. In the paper, the authors do not differentiate between changes in the material properties in contact with the fiber and changes in the mechanical nature of the material itself. I had thought that poor coupling meant a situation where a portion of the fiber experiences a change in its surrounding physics, for instance, when it passes through a fluid (air or water) or a mixture of solid and fluid that degrades the mechanical tangential displacement coupling. The simplified theory here does not distinguish between fluid and solid, which I found surprising. It seems to me that a fiber can be perfectly coupled to the ground while moving through a range of materials, from very stiff to very soft. Yet, within the paper's context, this would appear as a coupling change. I suggest that the authors expand on what they include under the term "bad coupling."

This is an excellent point. It is one that we previously did not describe in the manuscript. It is indeed a limitation of our method. We have now added a brief definition of what we refer to as coupling more explicitly in the text (L80-86, Section 2.1), and have now also added a brief section (Section 2.3.5) to communicate this concept and emphasise the limitations of our method. To summarise what we now state in that section: We deliberately establish as simple a coupling estimation method as possible in order that it is as universally applicable as possible. The method can therefore deal with different materials (e.g. fluids and solids), but not abrupt changes in materials between consecutive channels. We make readers aware of this limitation and suggest a pragmatic way to deal with it. Hopefully the new text added deals with this limitation, which shouldn't affect many use cases, but obviously is an important consideration in certain instances. Thanks again for raising the point!

Simple Theory Assumptions: My second comment also pertains to the simple theory. It is a purely 1-D, single-component theory that omits some of the critical physics involved. As shown by Capdeville et al. (2024), changes in mechanical properties near the FO at small scales (on the scale of the gauge length) induce strain components coupling. This means that strain components at the wavelength scale (the one considered here for channel-to-channel coherency) can project onto one another over a spacial distance that is independent of the wavelength. Such behavior contradicts the main assumption used here. Figure 5, right panel, in Capdeville et al. (2024), illustrates this phenomenon, showing signal coherency loss over a very short distance, even with a perfectly coupled fiber. This implies that coherency loss can stem from both poor coupling and small-scale heterogeneity. A numerical test could easily verify this effect, or it should at least be mentioned in the paper. My impression is that, at this stage, coherency loss can be due to bad coupling, change in material property, issue with the FO geometry .... We cannot yet tel from data what is at the origin the observed coherency loss.

Indeed, this is another valid point. The reviewer nicely summarises this point that a change in coherency can be due to three things: poor coupling, changes in material properties or fibre geometry issues. While we already referred to these factors to some extent in the text, it appears not to have been communicated clearly enough or in sufficient detail. We have therefore changed the text to communicate the point raised in more detail (see L546-561). We have also added some text to the conclusions to clearly communicate this too (L645-647).

To summarise the key message that we have added to the manuscript: Small-scale heterogeneities, or changes in elastic properties, can affect wavefield coherency, and therefore coupling measurements using our method. Capdeville et al. (2024) provide numerical modelling experiments that evidence that the strain wavefield is more sensitive than the displacement wavefield to heterogeneities, at least for direct P-waves. However, we suggest that when using the ambient noise wavefield, which is highly scattered, combined with a random distribution of heterogeneities, the impact on our coherency measurements may not be as pronounced as in the case of Capdeville et al. (2024). Even so, we now point readers to this work in case their experiment setting would be prone to these small-scale local heterogeneity effects. Hopefully, the text in the manuscript now addresses this point sufficiently.

We did initially undertake some numerical modelling to test the above, but deemed that the choices we made were too subjective to form a useful enough result to include in the paper.

Minor Comments
p.12, line 359: "Small scale" is typically defined relative to the wavelength. Changing to frequency does not significantly alter this definition.

Good point. We have now changed the text to improve clarity. The text now reads: “As the frequency increases, its sensitivity to heterogeneities of shorter spatial scales increases” (see L379).

Figure 1: Some text is small and of low resolution, making it difficult to read on paper.

Sorry for that. We have now made the text larger and reduced white space so that the entire figure is larger in two column format. The image is also in vector format so should now be clear on any screen as well as paper.

Best regards
Yann Capdeville

Capdeville, Y., & Sladen, A. (2024). DAS sensitivity to heterogeneity scales much smaller than the minimum wavelength. Seismica, 3(1).