Solid Earth Discussions

Solid Earth Discuss., referee comment RC2
https://doi.org/10.5194/se-2020-219-RC2, 2021

Comment on se-2020-219

Anonymous Referee #2

Referee comment on "Strain to ground motion conversion of distributed acoustic sensing data for earthquake magnitude and stress drop determination" by Itzhak Lior et al., Solid Earth Discuss., https://doi.org/10.5194/se-2020-219-RC2, 2021

Dear authors,

thank you very much for your interesting manuscript entitled “Strain to Ground Motion Conversion of DAS Data for Earthquake Magnitude and Stress Drop Determination”.

Your manuscript is an important step in the direction of source parameter estimations from DAS data, and I think it would be great to see this work published. Nevertheless, I feel like there are some parts of the manuscript with space for improvement, so I would suggest a publication after some revisions. Please find the detailed review below. I am open to re-reviewing the manuscript after the authors implemented the changes below if required.

General Comments

- The structure of the manuscript is very clear, the scientific questions are relevant and suit well within the scope of Solid Earth.
- The manuscript does adapt existing techniques of source parameter estimations to new types of data and introduces a strain to velocity conversion utilizing slant stacks. This has not been done before (to my knowledge).
- The introduction of the semblance was slightly confusing to me. After reading the cited publication (Shi and Huo, 2019), things became much clearer. Yet I feel like this part could be improved by adding a bit more information on eq. 2 (see comment 1 below).
- The new slant-stack method to rescale strain (rate) is compared to a simple scalar velocity time domain re-scaling. The authors correctly state that this does only hold for a single plane wave and is basically the least accurate and complex method to do this. I

feel like for the synthetic test and the real data, the frequency-wavenumber approach (also mentioned by the authors) might have been possible. This would result in a much “fairer” comparison (see comment 11 below). It is mentioned that under some circumstances, the frequency-wavenumber conversion becomes problematic. If this was problematic for the available data, it would be good to briefly mention that.
- When working with DAS, one thing that comes up again and again is the problematic of instrument coupling and response. In this manuscript, there is no real discussion about this, and the DAS data is assumed to be true ground strain (rate). I feel like there could be either an analysis how the coupling / response could affect the source parameter estimation in the end, or a quantitative sensitivity analysis.

Below you can find more detailed comments to specific parts of the manuscript, directly addressed to the authors.

Specific comments

- Page 2, Line 46-47:

Whereas I agree that converting strain to displacement is a possible approach to be able to determine source parameters, I am wondering if it were possible “the other way around” as well. The mentioned source models all date back to the 70s, and I am curious if source models based on strain observations would be another way to approach this problem. I am not an expert on source parameter estimation, but I think there may be some benefits in re-visiting existing theory. I am wondering if you attempted/considered that? The key here being “the ability to invert for the source properties using conventional methods”. What about new methods, developed with a focus on strain as observation?

- Page 3, Line 87 and Page 3, line 90:

This seems to be eq. 2 from Shi and Huo (2019), where your f corresponds to the seismic trace (they call that “real part”) and h to the Hilbert transform (you call that “imaginary part”). Maybe clarify this, I found the formulation confusing, because you mention both “imaginary part” and “Hilbert transform”. Otherwise, it sounds to me like that the seismic trace f(t) (which is a time domain signal of real-valued numbers) has an imaginary part h.

The process of this complex valued trace is explained nicely in the paper you cite, and I think it would help the general understanding if you could explain this here a bit more clearly.

- Page 4, Line 107:

The formulation about the combination of signals, including potentially dispersive waves, is a bit vague. If you filter within a frequency band of interest, there still might be dispersive waves within this frequency window, and there still may be different arrivals on the array simultaneously.

- Page 4, Line 110:

If I read this correctly, you allow the phase velocity to be within +/- 100 m/s, with spacings of 5 km/s. Is this correct or is there a “k” missing (for km/s) in the outer bounds of the allowed slowness (inverse of apparent phase velocity).

- Page 4, Line 115:

If you calculate the slowness including the sign, you mention that you get the direction back – so why do you need to go through the process of a moving average? It would be great to explain this process in more detail, and why it is required.

- Page 4, Line 118:

When talking about filtering, it would be great to specify which filter you use. I assume you are talking about a Butterworth bandpass filter here? (After further reading, indeed you mention this specifically later in the text).

- Page 7, Line 148:

Great idea to add real ambient noise from recordings to these simulations. What is the reasoning behind not simulating noise in this frequency band? Are the added noise waveform recordings added to each channel independently, with the same spatial resolution as the numerical simulation? It would be great to get some more background on this added noise. Also, the depth of the noise recordings is from 800 meters, whereas the depth of the water column in the numerical example is 20 m. Do you expect this to have any effects on the actual measurements? What was the gauge length of the recorded noise waveforms? How did you “spatially differentiate”? Such that you did include the gauge-length effect? I think a little bit more detail would be beneficent for the readers here.

- Page 7, Line 160:

Are the mentioned wavelengths the apparent wavelengths along the fiber (propagation direction considered), or are they simply calculated by 12 Hz/Velocity? The apparent velocity along the fiber for not in-line events could potentially be much higher than this value?

- Page 8, Line 165:

Is this the apparent slowness?

- Page 8, Figure 3 caption:

The last sentence does read a bit odd. Maybe change to “The signals in the gray regions have been amplified by a factor of 6 for easy comparison….”

- Page 9, Line 186 and throughout this entire section:

The example you provide is very impressive. Yet I feel like it is an unfair comparison, since you use the same apparent phase velocity for the conversion (dotted line) for all arrivals. Estimating these for separate windows would potentially also give you a better estimate of the strain-rate derived acceleration. You also mention the Frequency-Wavenumber (FK) domain approach in your introduction, but do not compare the slant-stack conversion to this approach. One may argue that for your given example, the FK domain approach would result in a very similar converted acceleration that you get with the slant-stack conversion. So, it feels a bit “unfair” to compare your method to basically the simplest existing method with even violated assumptions (not a single plane-wave arrival). Because your method seems to work quite well, I think it would be great to compare it to the FK-domain approach.

- Page 16, L 287:

Is this parameter tuning choosing parameters based on existing literature? Or is this “tuning” actually an iterative process until you arrive at these values?

- Page 16, line 289:

Cs should be C_s?

- Page 20, Figure 11:

Are the DAS-derived magnitudes here the ones from the slant-stack conversion approach? It would be good to see the estimated magnitude comparison here for both strain to displacement conversion methods, in order to see how the ‘error’ in the conversion propagates to the final magnitude estimates. The same holds for Figures 12 and 13. I would understand if you think that Figure 9 is sufficient for this comparison, I just think that it would be nice to see this here as well base on my personal preference on how I look at figures.

- Page 22, Line 375:

It would be great to quantify “in good agreement” here.

- Page 23, Line 397:

When talking about real-time applications utilizing DAS data, the implementation due to the large amounts of data could become an issue. Did you do some back of the envelope calculations on how long such an inversion would take in real-time for the investigated earthquakes? If this would indeed be possible in real-time, I think your method does look very promising.

Technical comments:

- References:

The reference to Singh et al., 2020 seems to be missing in the List of references. Please make sure that all references are there.

- General remarks to figures:

- The figures are not inserted over the full width due to an expected two-column layout. I am aware that we live in digital times, and that we can always zoom into our pdfs – but by doing this for some of the figures, I felt the resolution was a little bit too low. I am unsure what the submission policy for figures (do they need to be within a certain file size?) is, but I hope that the final publication has higher resolution figures, such that zooming in actually reveals more information.
- I also think that the figure captions and labels should be slightly increased in text size.
- Some figures (e.g. Figure 8, panel (e)) might not be easily distinguishable by people affected by colorblindness. Most of the figures use a colormap that seems to be "viridis", which is a great choice. I would change the colormap of Figure 8, panel (e) to this as well (also for Figure 2, panel (a). Generally I think it is important to use colors that can be distinguishable by colorblind people, or use different line-styles whenever possible (e.g. dashed, dotted etc..).

Again, I want to emphasize that this manuscript is very interesting, well-structured and that I enjoyed reading it. With the suggestions above, I hope the authors will be able to improve this manuscript even further and enrich our community with a nice new publication. Thank you for the submission.