This study compares a DAS array with a collocated nodal seismometer array in terms of exploration and monitoring performance, described by the authors as (lines 52-54): "For "exploration", we mean the definition of spatial variations of elastic properties in the subsurface at high-resolution. With "monitoring", we have in mind those activities adopted for the real-time detection of "events" (in this case, seismic events)". The authors proceed to detail three procedures for the investigation of the two aforementioned tasks. For the task of exploration, the authors compute relative P-wave arrival times through a cross-correlation method (relative to the mean of the array) for both the DAS and geophone arrays. For the monitoring task, the authors consider one local and one regional earthquake. In the case of the local event, P- and S-arrivals are manually picked from the seismometer data, and DAS P-arrivals are picked automatically. The local earthquake is then located through standard travel time inversion methods. The regional earthquake is analysed by weighted time-domain beamforming of the DAS array.

It is clear that the authors put a lot of effort in these analyses, and they have produced many figures intended to support the claims made in the main text. Unfortunately, not all of these claims are sufficiently well supported by the data, in my opinion. Moreover, the authors do not seem to perform their analyses with the objectives stated in lines 52-54 in mind (no subsurface characterisation, no real-time workflows, no earthquake detection). I would strongly suggest the authors to rethink the focus of this manuscript.

Below I will try to explain my concerns, hoping that the authors recognise my criticism as ways for improvement of the manuscript. Some of my comments may have been a result of misunderstanding, as I found a few sections a bit hard to follow; in that case the authors could simply point this out and make some clarifications.

Kind regards,

Martijn van den Ende

<u>Main comments</u>:

1. **Uncertainties in the relative arrival times**. In Section 3.1, the authors investigate the potential of DAS for exploration, defined as characterising spatial heterogeneities in the phase speed of the medium which manifest themselves in the variations of the relative arrival times. These relative arrival times are estimated through cross-correlation of the waveforms in a 0.5-2 Hz frequency band. The main results of this analysis are presented in Figs. 7 and 8, and the authors discuss trends and deviations therefrom for several DAS segments. When taking Fig. 8 as an example, in which a (non-contiguous) segment of roughly 450m is shown, and assuming an average apparent P-wave speed of 5 km/s (consistent with beamforming analysis), I would expect a maximum difference in relative arrival time of less than 0.09s. However, Fig. 8b shows a variation of up to 0.4s in the DAS array, and about half that in the nodal array. This would suggest to me that the uncertainties in the arrival times are (much) larger than the expected moveout. Potentially the frequency band (up to 2 Hz) is too low for the required precision, or the waveform incoherence introduces additional uncertainties in the arrival time estimation. Regardless, without seeing error bars in Figs. 7 and 8, I am not convinced of any conclusions regarding the comparison

between the DAS and nodal arrays, and the potential for DAS for exploration tasks.

2. **Amplitude variations in relation to site effects**. In the last paragraph of Section 3.1, the authors suggest that the observed variations in the maximum P-wave amplitude are associated with the local geology, attesting to the exploration capacities of DAS. When I look at Fig. 10, I see two regions of elevated amplitudes, but interestingly these do not seem to lie on top of the hydrothermal features indicated in yellow. At other locations where the cable crosses various fault strands or is positioned close to the yellow features, I don't see such a pronounced amplification. And if I had to draw a third box in this figure, I would put it to the far left of the map, where the cable is far away from any geological features. So it would seem to me that the correlation between elevated amplitudes and the local geology is fortuitous at best. At the moment I can't quite think of any analysis that could prove more conclusive in this regard, so perhaps it would be best to take out this paragraph and corresponding figure.

3. **Objectives of Section 3.1**. When I read the authors' definition of "exploration", I initially expected the authors to perform some kind of subsurface imaging similar to what was performed by the PoroTomo team. If my concerns above regarding the uncertainty in the relative arrival times, and the correlation between the waveform amplitude and local geological features are warranted, then Section 3.1 does not offer much in addition to characterise the "spatial variations of elastic properties in the subsurface at high-resolution" (lines 52-53). Instead of relying on 1 passive source (the Hawthorne earthquake), perhaps the authors could use their automated procedure to analyse the numerous vibroseis sweeps that were performed to obtain robust and systematic anomalies in the relative arrival times, or to separate site amplification from the directional sensitivity of DAS, etc. This will likely be a lot of extra work, but it is something that could strengthen Section 3.1 and warrant its existence if the authors agree with my concerns raised in points 1 and 2 above.

4. **Local earthquake location uncertainty**. In Section 3.2.1 the authors locate a local earthquake with manually picked phases recorded on the geophones, yielding a hypocentral depth estimate of 450 +/- 40m. Performing the inversion with EDT on DAS yields a hypocentral depth that is practically at the surface. The authors suggest that "this discrepancy likely owes to the lack of observations attributed to S-waves, and the simplified velocity structure adopted for travel-time predictions" (lines 229-230). These hypotheses can be tested by performing EDT inversion of the nodal array using either only the P-wave picks or both the P- and S-wave picks. If nodal EDT inversion with only the P-arrivals also puts the source at the surface, but not with both the P- and S-arrivals, then the lack of S-wave picks is to blame. Otherwise it is likely that there is an intrinsic problem with EDT inversion of DAS data. This would render DAS inadequate for local earthquake monitoring, and so the claim that "DAS recordings can be used for monitoring and exploration purposes and their performance is at least comparable to seismological records if not superior" (lines 321-322) is unwarranted.

   When comparing the hypocentre locations estimated in the present study to those given by Li & Zhan (2018), there appears to be a (very) large discrepancy. Li & Zhan selected 5 earthquakes from the catalogue of Nathwani et al. (2011) as templates for their template matching study. All of the matched events in the catalogue of Li & Zhan should therefore be closely located to the original 5 epicentres, which are all positioned much farther southwest from the DAS array than shown in Fig. 11 of the present study (placing the inferred epicentres right at the edge of the array). Moreover, the depth of these events was estimated to lie in between 750 and 1250m, and not 450m. So regardless which type of array (geophone or DAS) or inversion method (EDT or absolute) is used, the hypocentre estimates of the authors are very different from the previous estimates. Normally it would be difficult to tell which study is correct, but fortunately the epicentres estimated by the authors are located right next to the instrumented borehole (well 56A-1, 400m deep) at the southern end of the array. From the borehole data the authors should be able to more precisely estimate the depth of the events, and also the distance if the events are really as close as the authors suggested (the propagation front should be strongly curved). So before drawing any conclusions regarding the suitability of DAS for monitoring, the authors should first establish a reliable benchmark to compare their results with. Since the DAS inversion procedure is automated (no manual phase picking), I would also like to see the inversion results of multiple local events, instead of just one (which could be a lucky hit).

5. **Regional earthquake beamforming**. Let me first mention that the manuscript of van den Ende & Ampuero (2020) was recently accepted, and that the version of record is now online. The authors may be interested in reading this revised manuscript, though the revised and newly added sections do not directly pertain to the discussion in the present study. Second, the plane-wave fitting approach (which I think what "PWF" means, but this abbreviation is not defined as far as I can tell) is essentially beamforming in the time domain. In the frequency domain, relative time delays become phase shifts, which form the basis for MUSIC beamforming as used by van den Ende & Ampuero. With MUSIC (and other forms of frequency-domain beamforming) the contribution of each sensor to the overall beampower is weighted by the correlation coefficient similar to Eq. (2) on page 5 of the manuscript. So in essence the PWF method is not very different from frequency-domain beamforming methods. The main difference between this and previous studies would be the stacking procedure that supposedly improves waveform coherence. It would be good to show this directly by comparing the original recordings with the stacked waveforms, and show that the stacking procedure positively affects the coherence/scattering.

   Having that said, the authors do indicate that they get better results that previously obtained by vdE&A. Looking at Fig. 12, it is a bit hard to verify this claim, since the scatter in panels c and d is quite large. It would be very helpful to include something like a moving average weighted by the correlation coefficient of the azimuth and the apparent velocity to see the best estimate of these quantities, and their confidence intervals (!). If I were to draw a line by eye, I would put the mean azimuth at around 180 degrees and the apparently velocity at 3 km/s, which are very different from 337 degrees and 4-6 km/s mentioned by the authors.

   Lastly, one of the main objectives of this study is to do "real-time detection of events" (lines 52-54), but Section 3.2 does not address either "real-time" or "detection".

6. **Conclusions**. The authors conclude their work with 4 statements, of which I believe are not well supported by the data, or are not a result of this study at all. I would advise the authors to distil a number of take-away messages from their own results.
   a. I've already disputed Statement #1 that DAS is equal or even superior to conventional seismometers for monitoring and exploration.

b. Statement #2 pertains to the volume of DAS data, but this does not make DAS any different from conventional seismic data in terms of the analyses. Other seismological studies (e.g. Roux et al., 2016, GJI) also deal with very dense arrays, and DAS data can always be subsampled if computational resources are limited. Moreover, data volumes and efficiency have not really been the topic of this study.
c. Statement #3 suggests that standard seismological tools cannot be applied to single-component measurements. This is true for polarity analyses, but does not necessarily apply to phase travel time inversions, full-waveform inversion, surface wave tomography, ambient noise interferometry, event detection, beamforming, and possibly numerous other analyses that I forgot to list here. And again, the importance of 1C vs. 3C does not really result from the analyses presented in this study.
d. Statement #4 rehashes previous work on earthquake detection, which was not considered in this study.

Technical comments:

7. In line 120, the authors set-up a velocity model with Vp = 3 km/s. Feigl et al. estimated an average Vp of 2.1 km/s (across all depths), which seems consistent with the beamforming analysis of van den Ende & Ampuero (very crude estimate, though). This may be one reason why the inverted hypocentres are much closer to the array than previously estimated. Also in this line the authors state that they picked 75 vertical and 48 horizontal geophone channels. Shouldn't the picks be the same for all the channels on a given geophone? Why is there a difference in the number of picks? Why did the authors resort to only manual picking for the geophones, and not also do automated picking to assess the quality of the automated picks to compare to the DAS picks?
8. I did not quite follow the stacking procedure (lines 123-126). It is mentioned earlier that only 1 channel is used per gauge, so does "11 adjacent channels are stacked, with a 20-channel step" mean that a stack is created over 11 gauge lengths? Are these stacks of delayed waveforms (aligning the first arrival so that it doesn't get smeared out)? Then in line 126 it is mentioned that the array is downsampled to 274 channels, "similar to the nodal array", but for the nodal array only 75 (+ 48?) stations were picked.
9. Lines 179-180, "Nevertheless, this result supports our workflow where DAS data need to be strictly compared to co-located, re-oriented Nodal data": why is this the case? I don't see how this can be concluded from the preceding paragraph.
10. In Figure 8 I would suggest to add breaks in the red/green/blue lines to indicate where each segment starts and ends, and not showing the gaps in between the segments as measured data.
11. Lines 234-235: but the automated picks should all be for P-waves and their conversions. Was the picking window so large that the direct S-wave could have been picked by mistake?
12. Section 3.2.1: was the origin time co-inverted in the absolute travel time inversion procedure (for the nodal seismometers)? Or was it fixed by the catalogue origin time? EDT doesn't include the origin time, so this information cannot be used to constrain the hypocentre location, which may be another reason why EDT puts the source at the surface. This can be checked by performing EDT on the nodal array.

13. To fully comply with the Open Science philosophy of Solid Earth, could the scripts that are used to process the data be made available in a repository (e.g. Zenodo or Figshare), so that others could reproduce the results?

Minor comments:

- Line 28: “strain” = “particle motion”
- Line 43: the URL to Feigl’s department page is not very long-term sustainable, and it will probably turn into a dead link within a few years. Better to refer to the GDR repositories.
- Line 117: typo in the UTC timestamp
- Line 255, tiny detail: van den Ende & Ampuero estimated an apparent P-wave speed of 4-6 km/s (page 919), not 5-6 km/s (although it all falls within the uncertainty).
- Lines 301-302 and 309-310: vdE&A also performed beamforming on subarrays that spanned the entire DAS array (see their Figs. 10-12).
- Lines 305-307: this is actually the other way around. By assuming a single plane-wave, any additional “sources” are discarded to the noise space. By minimising the projection onto the noise space, the effect of scattering is minimised if there is a stronger plane-wave with array-scale coherence. The more sources are assumed, the more the results are biased towards scatterers.
- Lines 311-312: see Lior et al. (10.5194/se-2020-219) for magnitude estimation with DAS.